# Climbing the Mountain

*The Scientific Biography of Julian Schwinger*

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1

# A New York City childhood

## Growing up

Julian Seymour Schwinger was born on 12 February 1918 ('just five score and nine years after the birthday of Abraham Lincoln'[1]) in New York City into a middle class family. His father, Benjamin Schwinger, was born in the town of Nowy Sacz in the foothills of the Carpathian Mountains in the part of Poland which throughout the nineteenth century remained under the rule of Austro-Hungarian Empire. Nowy Sacz, then called Neusandez by the Austrians, became home for a small Jewish community brought there by Emperor Leopold II of Austria in an attempt to install them on land as farmers. The settlers eventually returned to their traditional professions and trades and moved into the town, which became recognized among members of the orthodox Jewry for its rabbinical dynasty established by Chaim Halberstam, also called by his numerous disciples Reb Chaim Sandzer.

Benjamin chose to emigrate and came to the United States of America by himself around 1880 as a very young man. Having to support himself prevented him from obtaining more than the most basic education. He attended schools only to learn English, but did not go to college. In New York City he became a very successful designer of women's apparel. Benjamin eventually acquired his own couturier business, which prospered as Julian was growing up. However, he lost it in the stock market crash of 1929, and his life became difficult; he began to work for various firms as a designer. Since he was a gifted designer of women's clothes, with an eye for lines and design, he became well known in the Seventh Avenue clothing trade. Although the family was no longer as affluent as before, still they were quite well off and lived a quiet and comfortable life.

Julian's mother Bella (called 'Belle' by everyone), was born in the Polish industrial city of Łodz; she came to New York City as an infant with her family. In the nineteenth century Łodz flourished as a commercial and financial center with a large concentration of textile manufacturers, where German, Polish, Jewish, and Russian cultures mixed and coexisted; it became one of the foremost intellectual and cultural centers of East European Jewry. Belle's family owned a prosperous clothing manufacturing business in Łodz. Her father, Solomon

Rosenfeld, had been raised as an orthodox Jew, and he maintained this tradition in his household. He continued his career as a clothing manufacturer after he emigrated to the United States; he was also involved in the import business, and Julian recalled that he used to import toys from Nuremberg.[2]

At the time of his birth, Julian Schwinger's parents lived on the West Side, 141st Street on Riverside Drive, but some years after he was born they moved to a larger and newer apartment on 103rd Street, still on Riverside Drive. Their first child, a son named Harold, had been born in 1911 when they lived in a well-to-do Jewish neighborhood in Harlem, preceding Julian by seven years. Belle's parents rented an apartment next to their daughter's; following the old tradition the two families lived side by side and Belle was quite dependent on her mother.[2]

Benjamin's work and family were his life. He worked very hard and spent less time with his children than did his wife. Belle became the disciplinarian of the family; she nurtured Harold and Julian's artistic talents and got them to partake of the great cultural riches of New York City. On the other hand, having been raised as a princess, she was not very maternal toward her son.[3] Julian always remembered running over to his maternal grandparents' house and cherished 'distinct olfactory memories of foods prepared in the old Polish style.'[2] There were marvellous breads, soups, and other things, and Julian was left with an abiding taste for Middle European cuisine. The maternal grandparents showered a lot of affection on Harold and Julian, and the two boys were quite 'spoilt' with the attention they received.

Belle had a younger sister and a brother. Her sister had children of her own, and Julian had interactions with them. The brother, Al Rosenfeld, was a successful businessman; he dealt in perfumes and took many trips to Europe, and made a great impression on Julian with his stories about travels to the faraway world. None of the family had any interest in science or other intellectual pursuits, and when Julian became seriously interested in physics 'they tolerated me, but had no understanding of what it was about.'[2]

Julian did not remember any time in his life, even in his earliest memories, when he could not read or write. His parents employed a German nursemaid, Hedwig, as well as a Hungarian maidservant. Hedwig would take Julian to a movie house every Saturday, and at the age of three the boy amazed her by reading the marquee from a long distance and telling her what it said.

There occurred a couple of episodes in Julian's early life that attracted his attention towards scientific and technical things. One was a total eclipse of the Sun (which took place on 24 January 1925). 'I have a distinct memory of putting my head out of the window and staring with awe at this phenomenon. Then I equally well remember—as we know, the United States received reparations from Germany after World War I—and one of them was a dirigible called

*Shenandoah* that flew over New York. It must have come from Dresden airport, one of the early transatlantic flights, and again I remember looking out of the window at this incredible thing flying over. The *Shenandoah* arrived in 1923—which would have made me five years old, practically an adult!'[2]

As a little boy, Julian attended a kindergarten. He recalled one incident when he played hooky. The whole class was taken on an outing and they all went to wherever it was, a few blocks from his house. At one point he decided that he had had enough of that, so he 'gradually faded away' and went home. 'It's the only memory I have of kindergarten.'[2]

As they grew up, Harold and Julian shared a large room at home, and attended a public school, P.S. 186 on 145th Street between Broadway and Amsterdam Avenue, five blocks away from their home. At school Julian was interested in everything, and was a quick learner. Even though he was advanced to a higher grade several times, he was not considered nearly as bright as his older brother. At the elementary school, Harold won all the recognition and all the prizes came his way, but still his teachers complained that he was not living up to his potential. Julian followed in his brilliant brother's footsteps. In their mother's eyes, Harold was always the successful one. 'She always thought Julian was a kind of a failure.* His brother became a lawyer and Jewish mothers always want their sons to be lawyers, and Julian didn't. Someone said to her, after the Nobel Prize, "You must be very proud of Julian!" "Well, . . . ." '[5]

From the elementary school Julian remembered an incident when the teacher was trying to explain why the Moon always presents the same face to us. 'As she was describing all this I remember sitting at my desk and looking up at her and following what she was saying by moving my fingers. That was the Earth and that was the Moon and she saw what I was doing and nodded, "yes, yes, that's it!"'[2] This happened when Julian was perhaps in the second grade. 'It was very early and I was obviously very eager to learn.'[2]

It was very helpful to have a brother who was several years older. Harold of course went to high school and college, but his textbooks were always lying around, and Julian began to read them. Two things became very important: 'One was that college level books were available to me at an early age; second, the family had somehow acquired a set of *Encyclopaedia Britannica*, in which I read the scientific articles from cover to cover. My family had acquired it for Harold; it was very valuable to have an older brother! Of course, for a boy of ten he wouldn't have had much of an interest in a three-year old, but my memories of interacting with him come from much later when I was sent to a summer

* Sidney Borowitz recalled an encounter between his wife and Julian's mother at a restaurant years later. Belle was disparaging of Julian's accomplishments compared with those of Harold, who also had provided her with a grandchild.[4]

camp where he was counselor. [In the camp, Julian spent most of his time sitting on his bunk and devouring books. Harold had literally to drag him away from the books to the lake where he managed to teach him how to swim.*] The camp was in the Adirondacks, and that must have been when I was ten or eleven, that's a later stage.'[2]

Julian was not without interest or ability in athletics. He recalled the visit of a famous tennis pro to summer camp. 'Bill Tilden came through [this summer camp] on some lake in northern New Hampshire. He gave a demonstration and of course all the campers were there. It was a big tennis court and they were all distributed around. Tilden was playing with somebody and then a ball went astray and I reached up and caught it and threw it to Tilden. He looked at me and said, "You have a great future ahead of you." I've always interpreted that to mean in tennis.... I was also pretty good in baseball.'[2]

Julian was certainly precocious. 'That's an objective fact, because I ran very rapidly through the elementary school, skipping classes and all sorts of things. I don't think anybody directed my attention to scientific and technical things; it was somehow in the genes. There was no doubt that I was bright, and particularly overspecialized even then. I would pick up my brother's mathematics books, perhaps the calculus. I certainly remember the calculus book; I remember once when I was lying in bed reading his book and he was doing something else, and I turned to him and asked, "What does *osculating* mean?" I did not know what *osculating* meant. *Osculating*, as one knows well, means kissing. Funny thing to remember! I was reading a fairly innocuous book on mathematics, not a lurid novel, and I asked my brother about this word when it occurred in the book.'[2]

After elementary school, Julian attended a junior high school in Upper Manhattan, near Broadway and 180th Street, some 40 blocks from home, and he had to use the subway. Then, very soon, he enrolled in Townsend Harris High School, from where he would graduate in 1933.

In 1848 New York's Board of Education had decided to establish the city's first municipal institution of free higher education, the New York Free Academy, which, in due course, became the City College of New York. It had a preparatory component known as the 'introductory' year which later separated and grew into Townsend Harris High School. It was named after Townsend Harris, the president of the Board of Education at the time of inception of the Free Academy, and was located on the campus of the City College on Amsterdam Avenue at 136th Street. It flourished until 1942 when it was closed as being 'inessential' by New York Mayor Fiorello La Guardia.

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* In fact, on one occasion his father visited the camp and found Julian reading in his bunk, so Benjamin grabbed him and threw him in the lake.[3]

Julian did not recall having been inspired by any of his teachers until he went to Townsend Harris. 'Before that I was simply enduring it. My education came from myself rather than from my teachers. Townsend Harris High School was exceptional. In 1929, there occurred the crash of the stock market and the Depression began. My family suddenly became poor. Of course, they wanted to send me to college. Why my father's business should have been wiped out by the Depression, that I never understood. However, they no longer felt that they could send me to Columbia University. That was the logic for my attending Townsend Harris: there was no tuition to be paid there; it was one of the regular high schools, but it was specifically oriented towards City College.'[2] Besides, Harold had also attended Townsend Harris.

During the Depression, the City College was an outstanding institution. Admission was highly selective, yet for most of these bright students, attendance at one of the tuition-free colleges of the City of New York was their only opportunity of obtaining a college education. Moreover, with the economic crisis, talented people who, for example, were working for their doctorates at places like Columbia or New York University, simply had to earn money by taking teaching jobs in schools, and this was typical of teachers at Townsend Harris High School. They were a very unusual set of teachers, people who were active researchers at the same time, and that was just wonderful for the young Julian Schwinger. 'I did not have much interaction with my fellow students, but I was interested in the teachers. I was fascinated by courses in physics, much less so in chemistry. I imposed myself outrageously on one of my teachers, Irving Lowen, who was doing research for his doctorate at New York University. [He later taught at NYU.] Already then I was at a level when Lowen said, "Look, instead of talking to me, why don't you go to see my professor at the University?" In a sense I had begun to do research, though not so much at Townsend Harris as at the City College, but the connection had been made at Townsend Harris. Some of these teachers of mine, who were graduate students at various universities, and not my fellow students, became my friends.'[2]

Bernard Feld recalled a legend told at City College, of how Lowen discovered Julian. 'Irving came across this kid sitting in the library reading the *Physical Review* and he looked over his shoulder and there was this kid reading Dirac and so Irv thought, well, here's another of these smart aleck kids that, you know, we get them every once in a while, so he quizzed him about what he was reading and Julian allegedly was not only capable of telling him what he was reading but also told him what needed to be done to complete what Dirac hadn't completed in this particular paper.'[6]

Lowen was a good teacher of physics. He explained to his pupils all about the Bohr theory of the atom and Julian would go to him afterwards and ask about quantum mechanics. 'That was an exciting period. I found books in the public library. Beginning with the local libraries, which I exhausted very quickly, I went to the New York Public Library at 42nd Street, where everything was available, including periodicals. The announcement of the Nobel Prize in Physics in 1933 to Heisenberg [for 1932] and Dirac and Schrödinger caught my fancy. There were articles in the newspapers referring to this mysterious quantum mechanics. I went through the popular books, which, of course, I left in disgust, but there was a book by [James] Jeans,[7] in which I found murky references to strange things going on inside atoms as compared to macroscopic physics. But Jeans' attempt at popularization left me completely frustrated, so I kept going. That's when I began hunting for books on quantum mechanics.'[2]

Julian used to receive a small allowance from his parents, which he would use for subway fares and buying books. Sometimes he wouldn't have enough money left to get back home. 'Subway fare must have been five cents. And I remember I once had four pennies, not five.'[2] Being almost a child he began to cry and someone stopped to inquire what the matter was, and said, 'Here's a nickel,' which brought Julian home.[2]

As he recalled, 'there was something else I was preoccupied with: electrical engineering. Gadgets. I would read all about radios and some of the underlying theories about inductances, capacitances, and so forth, until I gradually realized that I didn't really care about that very much. Somewhere I encountered a mysterious set of equations invented by Maxwell, and that's where I had to go.'[2]

With the help of sharp razors and glue Julian put together very beautiful toy airplanes. 'I wasn't impractical, but I did not build radios. Actually I did build things, but they were not radios; they were model airplanes. I had models of all the fighter planes of World War I, in particular those of the famous Red Baron [Baron von Richthofen], who flew a triplane. I did play with electrical things, but I don't know why I didn't play with radios. I do remember that I used to do things like putting wires into sockets and making sparks; I'm surprised I'm still alive!'[2]

Julian encountered P. A. M. Dirac's classic book on *The principles of quantum mechanics* in 1931, a year after its first publication.[8] He was 13 years old. He also read George Birtwistle's *The quantum theory of the atom*, which had been published in 1926, and of which Julian bought a second-hand copy at Barnes and Noble, the used book shop on lower Fifth Avenue.[9] Birtwistle's book summarized all the recent papers on quantum mechanics, and it gave a sequential development of all the recent work on the subject; Julian found it exciting and provocative. Julian became a prolific reader and books became his world. He was not much interested in activities typical of boys of his own age. This is not to say that he was uninterested, for example, in sports. As David Saxon recounted many years later, referring to the war years at the MIT Radiation Lab, 'I discovered to my astonishment that he was interested in sports. We'd have a picnic and he'd throw a left-handed football very well. That was part of the reason he came [to UCLA in 1971]; he wanted to play tennis, ski, and swim. He had the kind of normal athletic interests that any young kid would have. He had a quiet background interest that would not be revealed except under the right circumstances.'[5]

When Irving Lowen told Julian about going to meet his professor at New York University, he did indeed do so. The physics professor was Otto Halpern. A year and a half later, Halpern and Schwinger published a paper together in the *Physical Review*, but that happened only after Julian had enrolled in the City College after graduating from Townsend Harris High School. He did perfectly well in his grades, and his family had nothing to worry about. His brother Harold (who had received a bachelor's degree in business from City College in 1931, and a master's degree from Columbia a year later) got a law degree from Fordham University in 1936. After taking his degree, Harold held two jobs simultaneously—he worked as a law clerk during the day for $10 a week, so he had to make ends meet by working at night in a bank.[3]

## Going to college

In the fall of 1933 Julian Schwinger became a student at the City College of New York (CCNY). He started as a sophomore and first took the normal run of general core courses. Quite soon at the City College Julian came into contact with Hyman Goldsmith, who later became one of the founders of the *Bulletin of Atomic Scientists*. At that time, Goldsmith was 'in the category of permanent graduate students, somewhat dilettantish, with a great interest in music, which was a very important sideline for me. Goldsmith did wonderful things for me by bringing me into a musical environment. He did one terrible thing for me because he was interested in tennis. I had been interested in tennis when I was quite young and then I stopped being athletic. He said, "Oh, I want to play tennis. Want to come along?" I said okay. They were hitting the ball and I said, "Can I try that?" So I picked up a racket and he hit a ball to me and I was totally awkward because I hadn't touched a tennis racket in five or six years. So I just put the racket out and the ball went straight up in the air. He walked over, took the tennis racket away from me, and said, "That's all." And I was just mad as hell because with a little bit of practice I could have shown my inherent tennis ability, which does exist. I do think I'm quite good at tennis. Sometime in the 1950s he died in a foolish accident by drowning. He loomed very large as an influence on me.'[2] Goldsmith and Schwinger published four joint papers after Julian went to Columbia University.

Townsend Harris provided automatic entry into the City College. Thus far Julian had been a solitary type of person, but 'at City College I began to meet people, especially the students, who were at my level, and that was a novelty. I met Joseph Weinberg and Morton Hamermesh. There was an incredible number of good people. There was Robert Hofstadter; he went to Stanford and won the Nobel Prize for scattering electrons off nuclei and determining the form factor of the proton. For the first time I ran into people, not exactly my contemporaries, because I was still the youngest one around, but people who were teaching themselves and were aware of the developments. Not so much in physics as in mathematics, but that was much easier to come by. Certainly Hamermesh, with his interest in mathematics and chess, was one. They were a little closer to my own age but not at the same level. Joseph Weinberg was the person I talked to the most.'[2]

Weinberg vividly recalled their first meeting. Because of his outstanding laboratory reports, he had been granted the privilege of entering the closed library stacks at City College. One day he was seeking a mathematics book (Townsend's book on real variables[10]) which had been mentioned at the Math Club the day before, and while he reached for it, another youngster was trying to get it. They had both heard the talk, on functions which are continuous but nowhere differentiable, so they shared the book between them, balancing the heavy volume on one knee each. The other fellow kept finishing reading the page before Weinberg, who was a very fast reader. Of course, his impatient co-reader was Julian Schwinger. Both were 15. Weinberg mentioned that he usually spent his time, not in the mathematics section of the library, but in the physics section, which turned out to be Julian's base as well. Weinberg recalled that Dirac's book on quantum mechanics[8] was very interesting and exciting, but difficult to follow. Julian concurred, and said it was because it was polished too highly; he said that Dirac's original papers were much more accessible. Weinberg had never conceived of consulting the original literature, so this opened a door for him.[11]

Later on in 1937 Schwinger and Weinberg attended the Summer School together at the University of Michigan in Ann Arbor. 'When I went to Columbia, Weinberg and I lost contact, but somehow got together again to go to Michigan. Later on, when I arrived in Berkeley to work with Robert Oppenheimer, he (Weinberg) was already there as a student. He got his degree from Oppenheimer in 1942. His thesis is still occasionally referred to as an early attempt [to formulate] certain aspects of field theory.'[2] Weinberg ended up with an endowed professorial chair at Syracuse University.

At the City College, Julian took the normal run of courses on general education subjects. At Townsend Harris he had studied French and German. He dropped the German when he went to City College and concentrated rather heavily on French. He later found it useful in France, but not otherwise. 'It is the one foreign language that I can speak fairly fluently. I learned American history a great deal. I had the standard liberal arts education. I joined the City College as a sophomore, and there were requirements which, of course, I went through.* But I was beginning to become more and more reluctant to spend time on things other than physics. And so I took physics courses; I had to take elementary physics and felt very uncomfortable. I was bored. I'm afraid I occasionally got uppity. I remember there was one lecturer who did not belong to the special class of people I knew. He was telling us about heat and asked, "Does anybody know about what happens to the spectrum when you change the temperature?" So I raised my hand and said, "The Planck distribution is such and such," and he stared at me and said, "Sit down. I don't want to hear about that!" I think I was occasionally brash. The teacher was put off by my remark. At least he did not think there was anyone in the class to bring up such topics. He may not have known it himself; it was not common knowledge at that time. I look back at these things with horror!'[2]

Edward Gerjuoy was one of Julian's classmates at City College. 'My main claim to fame is that Julian and I took the same course in mechanics together, taught by a man named Shea, and I got an A and Julian a B,' because Julian did not do the work. 'It took about a week before the people in the class realized we were dealing with somebody of a different order of magnitude.' At a time when knowledge of a bit of vector algebra was considered commendable, 'Julian could make integrals vanish—he was very, very impressive. The only person in the classroom who didn't understand this about Julian was the instructor himself.' 'He was flunking out of City College in everything except math and physics. He was a phenomenon. He didn't lead the conventional life of a high school student before he came to City College'—unlike Gerjuoy and Sidney Borowitz he was not on the math team in high school so they had not known him earlier—'when he appeared he was just a phenomenon.'[12]

Morton Hamermesh recalled another disastrous course. 'We were in a class called Modern Geometry. It was taught by an old dodderer named Fredrick B. Reynolds. He was head of the math department. He really knew absolutely nothing. It was amazing. But he taught this course on Modern Geometry. It was a course in projective geometry from a miserable book by a man named Graustein from Princeton, and Julian was in the class, but it was very strange

* City College had an enormous number of required courses. Among them were two years of gymnasium. One had to pass exams in hurdling, chinning, parallel bars, and swimming. Because Weinberg and Julian had nearby lockers, they often fell into physics conversations half dressed, and failed the class for lack of attendance. Weinberg remembered seeing Julian's hurdling exam. Julian ran up to the bar, but came to a standstill when he was supposed to jump over sideways. The instructor reprimanded him, at which point Julian said, *sotto voce*, 'there's not enough time to solve the equations of motion.'[11]

because he obviously never could get to class, at least not very often, and he didn't own the book. That was clear. And every once in a while, he'd grab me before class and ask me to show him my copy of this book and he would skim through it fast and see what was going on. And this fellow Reynolds, although he was a dodderer, was a very mean character.* He used to send people up to the board to do a problem and he was always sending Julian to the board to do problems because he knew he'd never seen the course and Julian would get up at the board, and—of course, projective geometry is a very strange subject. The problems are trivial if you think about them pictorially, but Julian never would do them this way. He would insist on doing them algebraically and so he'd get up at the board at the beginning of the hour and he'd work through the whole hour and he'd finish the thing and by that time the course was over and anyway, Reynolds didn't understand the proof, and that would end it for the day.'[14] Sidney Borowitz, another classmate of Julian's, recalled that 'we had the pleasure of seeing Julian attack a problem *de novo*, and this used to drive Reynolds crazy.'[4]

Julian also took advanced courses in mathematics. There was one course in group theory. 'However, I have no memory of City College. I can't quite separate what was at City College and what was at Columbia University. But, of course, I was interested in mathematics.' Julian was at the City College for only about one year or so, and then he transferred to Columbia in 1935. By the time Julian went through the City College, he knew quantum mechanics quite well at the advanced level. He particularly cherished Dirac's classic book. 'No doubt it was my bible. I have distinct memories of Joe Weinberg and me talking about the book of Dirac, which we both recognized as the only thing to be considered. Of course, I had access to Birtwistle's book, which I studied at the same time as Dirac's book. I also knew the book of Pauling and Wilson.[15] I also read Hermann Weyl's book on *Group theory and quantum mechanics*,[16] the English translation of which came later; it had a tremendous effect on me. I think I took group theory at Columbia. But by then I was so much imbued with Dirac's book that I did not need group theory. I thought that the mathematical niceties of group theory were quite unnecessary. Quantum mechanics had the idea of symmetry built in it and if I needed symmetry ideas in quantum mechanics I would use quantum-mechanical language, not this entirely separate knowledge of group theory, which, of course, is a very old-fashioned idea. I've felt that way all through my life. If you want a branch of mathematics, you develop it in the physical context, not as something separate, which you then try to apply, rather than integrating it from the beginning. That's part of my philosophy.'[2]

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* In addition, he was also apparently a notorious anti-Semite. He used to discourage Jewish students from studying mathematics, which worked to the advantage of physics.[13]

Eventually Joe Weinberg persuaded Julian to join the Math Club. His inaugural talk was on the quantum-mechanical harmonic oscillator in Dirac's operator representation, showing that you only needed to compute the ground state, and everything else could be constructed by use of raising and lowering operators. The mathematicians in the audience did not receive this presentation very well, because they were not concerned about getting 'pregnant formalisms.' We see that Julian was already anticipating his insightful work on angular momentum as an adolescent. Later, Weinberg was studying the anomalous Zeeman effect, which was treated in the first edition of Dirac's book,[8] and suspected that what underlay Dirac's treatment was the presence of a Lie group. Weinberg discussed this with Julian, and neither recognized the group—it turned out to be $SP(2, R)$. They discussed the nature of the group and realized that it could be represented by two harmonic oscillators—which was the basis of Julian's monumental paper on angular momentum many years later [69].[11,*]

The Julian Schwinger archive at the University of California, Los Angeles, contains a small notebook dating from the City College days, probably 1935.[17] About half the pages are filled with notes from mathematics courses that Julian was taking there on group theory and complex variables. But interspersed with that are citations to important contemporary papers, along with a detailed, remarkably mature, working out of those papers in his own hand. This document, and other similar notebooks, is extremely revealing regarding the process by which Julian taught himself what was going on in current research. The papers he worked out included Pauli and Weisskopf's 1934 paper on the quantization of the scalar relativistic wave equation [spin and statistics],[18] the 1929 paper of Heisenberg and Pauli on quantum electrodynamics,[19] Heisenberg's 1934 paper on fluctuations in electric charge,[20] Bethe's paper on neutron phase shifts,[21] two papers by Dirac from 1929 and 1933,[22] and several others.

At the City College Julian met and became friends with Lloyd Motz, who was a part-time instructor there. This encounter had a strong impact on Schwinger's future career, since it was Motz who introduced him to Columbia University. Motz was about ten years older than Schwinger. Beginning in 1926 he attended City College and excelled in his studies so consistently that on the basis of his grades he was judged the top sophomore. Each year the sophomore with the highest grades was awarded the Naunberg fellowship to spend a year in any foreign university, which was uncommon and something of a high honor at that time. Motz studied physics but was also seriously interested in mathematics; therefore he chose to go to Göttingen, where he spent the entire academic year 1928–1929 and had a very exciting time taking courses from Max Born, Walter Heitler, and Robert Wichard Pohl, among others. Upon his graduation from

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* Square brackets [ ] signify the references to Schwinger's papers in Appendix A.

City College in 1930, Motz was accepted as a graduate student at Columbia University. At first he did not work directly under Isidor Isaac Rabi, but took a course in statistical mechanics from him; he earned a good reputation, received Rabi's attention and eventually a $1,500 Columbia fellowship.

Motz heard about Julian from his older brother. 'I became very friendly with Harold Schwinger at that time. [Harold and Lloyd Motz were classmates at the City College.] He came to me one day (in 1930 or 1931) and said, "Lloyd, I have a brother who I think is a genius, but we don't know." And later Hyman Goldsmith, whom I knew very well, was talking about a young kid at Townsend Harris and said, "This kid is incredible; he knows so much!" So he introduced me to him and that's when I first met Julian. It must have been around 1933. At first, I didn't do much with him, but Julian was the sort of youngster who wanted to have intimate relationships with professors. He wanted to be invited to things but he would never ask. You had to invite him, then he would respond. His response was always very warm; he was a very warm, loving young man.'[23]

Schwinger never took a course taught by Motz, but he did meet him at the City College. 'I suppose I had become a sort of local celebrity. There's a strange mixture in my makeup. I'm by nature very shy, and yet in these matters I'll press forward. If I have something to say I'll say it, and so it became known that this kid knew a lot. Rather than a "celebrity," more like a "peculiar chap." I did go to classes where many of these people came, and just as I mentioned the Planck distribution, which was totally gratuitous, but I couldn't help myself, I knew it so why shouldn't I say it? [he made himself known]. Now Lloyd Motz and I got to know each other at the City College, and just the same thing happened with Hyman Goldsmith and Irving Lowen. These were people with contacts at Columbia and New York University, and I followed both avenues.'[2]

Schwinger began working with Motz in 1934, when the latter was already a University Fellow. He became a frequent visitor to Motz's eighth-floor office at Columbia. Motz was surprised by the 16-year-old Schwinger's scientific skills. 'We would come into my office and he would start working and before I knew it he was so far ahead of me, so quick, that I could not always follow what he was doing. Absolutely right though all the time!'[23]

This was Schwinger's first real research calculation in physics; it was related to a just published article by Bethe and Heitler[24] on the stopping of fast charged particles by the emission of radiation under the influence of the electric field of a nucleus, and it had obvious implications for cosmic-ray physics. Bethe and Heitler had calculated the energy loss of a charged particle by the emission of radiation caused by the braking of the particle's acceleration by the electromagnetic field, the famous Bremsstrahlung effect; they had also used the Born approximation to compute the consequences of a similar effect due to electron–antielectron pair creation. After reading this article, Julian suggested to Motz that they consider an additional effect: the interaction of electrons back on the field. They worked on it and thought that they had a beautiful theory until Motz submitted the calculation to Hans Bethe, who dismissed it by pointing out that the interaction operator which Motz and Schwinger had used was non-Hermitian and thus unphysical.[23] Schwinger, the perfectionist, was extremely upset and crestfallen because it was he who had talked Motz into using that operator, despite his older colleague's earlier objections which had been basically in line with Bethe's later criticism.

## Paper Number Zero

By the end of his short stay at the City College, Julian had learned and well understood most of the current attempts to expand the scope of quantum mechanics to field theory. He had started by reading and following papers in the *Physical Review*, such as the article by Wendell Furry and J. Robert Oppenheimer[25] in which they had eliminated the infinite Dirac sea of electrons in which the emptiness rather than the fullness of a state of negative kinetic energy is regarded as being equivalent to the presence of a particle. Soon Schwinger's reading of original papers expanded to include everything that was available in English, German, or French. In particular, he read N. F. Mott's articles on electron scattering, including his 1930 paper entitled 'The Collision Between Two Electrons'[26] on the Coulomb interactions between two similar, thus indistinguishable, particles. Contrary to the scattering of different particles, this problem required using symmetrized or antisymmetrized wavefunctions, depending on whether the particles obeyed Bose or Fermi statistics. In 1930 one could not be sure of the implications of such a procedure, even whether the so-called leading-order differential cross-sections would reproduce the classical Rutherford formula as is the case for different particles. At the same time, Julian studied a paper by Christian Møller,[27] in which he had calculated the two-particle scattering cross-section by using a retarded interaction potential. Of course, Schwinger read all of Dirac's papers on quantum field theory, and was particularly impressed by the one on 'Relativistic Quantum Mechanics,'[28] 'in which Dirac went through his attempt to recreate an electrodynamics in which the particles and light were treated differently. [It was] a paper in 1932, in which the electromagnetic field was not described by an energy but was simply an operator function of time.'[2] In a paper of Dirac, V. A. Fock, and B. Podolsky,[29] 'it was recognized that this was simply a unitary transformation of the Heisenberg–Pauli theory[19] in which the unitary transformation was applied to the electromagnetic field. And I said to myself, "Why don't we apply a similar unitary transformation to the second-quantized electron field?" I did that and worked out the lowest approximation to the scattering amplitudes in unrelativistic notation. It was a relativistic theory but it was not covariant. That was in 1934, and I would use it later; [the notion, called the 'interaction representation'] is always ascribed to Tomonaga, but I had done it much earlier.'[2]

Thus before he left the City College, Schwinger did write his paper 'On the Interaction of Several Electrons' [0], in which he introduced a procedure which he would later call the interaction representation to describe the scattering of spin-$\frac{1}{2}$ Dirac particles, electron–electron scattering, or Møller scattering.* 'Furry and Oppenheimer had written their paper on multiparticle interactions using a local potential and second quantization.[25] And I thought to myself that relativistic interactions are not local; they are functions of momenta and so on as in the Møller interaction. So I asked myself whether I could develop a second quantized theory which would allow for non-local interactions, which is an essential aspect of a relativistic theory.'[2]

The original typescript of this unpublished paper is in the Schwinger archive at UCLA.[17] The flavor of this short six-page note is caught in its second paragraph: 'It is the fundamental assumption of all field theories that two particles do not interact directly but, rather, the interaction is explained as being caused by one of the particles influencing the field in its vicinity, which influence spreads until it reaches the second particle. Hence we may express the Hamiltonian of our system of particles in terms of the Hamiltonians of the several particles in interaction with the field. The well-known expression for the Hamiltonian is then

$$\sum_{pq} a_p^\dagger (p|H + U|q) a_q, \tag{1.1}$$

where

$$H = c\alpha \cdot p + \beta mc^2 \tag{1.2}$$

and

$$U = -e(\phi - \alpha \cdot A).\text{'} \tag{1.3}$$

Here $a_q$ and $a_p^\dagger$ are annihilation and creation operators for electrons in the states $q$ and $p$, respectively. The scalar and vector electromagnetic potentials are given by $\phi$ and $A$, respectively, while $\beta$ and $\alpha$ are the usual Dirac matrices, for a spin-$\frac{1}{2}$ charged particle of mass $m$ and charge $e$, $c$ being the speed of light.

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* Sometime earlier, perhaps, Julian had helped Weinberg calculate the Klein–Nishina formula for Compton scattering, for which there was no clue in Heitler's book. Weinberg's calculation had 'fallen apart,' and he asked Julian to have a look at it. Julian calculated it correctly, in which 'he ran through spin sum after spin sum, and got them all right,' and detected the simple error in Weinberg's calculation.[11]

The key point occurred on the second page, when Schwinger transformed the unperturbed Hamiltonian away, by the unitary transformation

$$\Psi = e^{-(i/\hbar)H_0 t}\psi, \quad H_0 = \sum_{pq} a_p^\dagger (p|H|q) a_q, \tag{1.4}$$

thereby leaving the theory in the 'interaction representation'. Then, by 'successive approximations,' i.e. a perturbative expansion, Schwinger obtained, quite straightforwardly, the Schrödinger equation (back in the Schrödinger picture)

$$i\hbar\frac{\partial\Psi}{\partial t} = \left\{\sum_{pq} a_p^\dagger (p|H|q) a_q + \frac{1}{2}\sum_{pqrs} a_p^\dagger a_q^\dagger (pq|V|rs) a_s a_r\right\}\Psi, \tag{1.5}$$

where the second term in the braces involves 'Møller's expression for the matrix element of the interaction between two electrons,'

$$(pq|V|rs) = \frac{e^2\hbar^2}{V}\frac{(u_p^\dagger u_r)(u_q^\dagger u_s) - (u_p^\dagger \alpha u_r)\cdot(u_q^\dagger \alpha u_s)}{|\mathrm{p}_q - \mathrm{p}_s|^2 - (E_q - E_s)^2/c^2}. \tag{1.6}$$

The notation is standard: the *u*s are the momentum-space wavefunctions for the electrons, and p and $E$ stand for the electron's momentum and energy in the various states. $V$ is the infinite volume of space, a normalizing factor.

In deriving this result, Schwinger had to omit a term which 'represents the infinite self-energy of the charges and must be discarded.' This he eventually came to see as a mistake: 'The last injunction merely parrots the wisdom of my elders, to be later rejected, that the theory was fatally flawed, as witnessed by such infinite terms, which at best, had to be discarded, or subtracted. Thus, the "subtraction physics" of the 1930s.' [197]

This particular paper, which Schwinger did not publish, was important for his later work because this was the starting point of his work on covariant perturbation theory. 'Although it took me a while to recognize it, it was part of my makeup already. This was worked out at the City College. I wrote it as a paper, but why I did not send it for publication I don't know. At that time I had no idea what a publishable paper was; I was still pretty young [Julian was then 16 years old].'[2] He did not even ask Lloyd Motz what to do with his paper on the interaction representation. 'I was rather secretive about it,' he recalled. 'It was written for myself, a little practice in writing.'[2]

## First publications

While working on his secret paper, Julian was also engaged upon other research that led to two publications in Letters to the Editor of the *Physical Review*. Both articles were co-authored with experienced physicists who had suggested the topic of research and offered advice, but the actual calculations were done by Julian alone.

The first of these letters, 'On the polarization of electrons by double scattering' [1], was co-authored with Otto Halpern and dated 6 June 1935. Schwinger was then 17, but the paper contained results of earlier work, and was finished as early as 1934, while he was still a student at the City College.

Julian's first collaboration with Halpern was not successful. 'Bethe had written a paper on the stopping power of a neutrino if it had a small magnetic moment. I don't think Halpern was aware of that paper. I was. But he brought up the same question. "If the neutrino had a small magnetic moment, how would you calculate something, like the scattering properties?" So I did [the calculation]. And he said, "Oh, that's nice, we must write a paper." And then I think I said, "Oh, but you know Bethe has already written a paper on this subject." I took for granted that Halpern would have known about the paper. I was learning that not everybody reads all the literature.'[2]

The fact that young Julian had a joint paper with Halpern was in itself remarkable. Otto Halpern, an emigré physicist of Jewish descent was offered a professorship of theoretical physics at New York University (as successor to Gregory Breit who had left for Wisconsin) after he was forced out of Nazi Germany. His stimulating but patently contrarian attitude in scientific discourse was legendary. He was a man of imposing physique who dominated a room by his presence; he engaged in heated arguments with any recognized authority, even of the stature of Enrico Fermi. He was especially intimidating to doctoral students, whom he ignored until he deemed them worthy of the privilege of discussing physics with him. Together with I. I. Rabi, Halpern conducted a weekly seminar which attracted large audiences to University Heights on Wednesday nights. Morton Hamermesh described these events as 'a sort of battlefield, just violent fights.'[14] Despite that, Julian Schwinger, who was unbashful in scientific matters, had agreed to give a talk in Halpern's seminar on two recently published papers by Max Born and Leopold Infeld in the *Proceedings of the Royal Society of London* on the quantization of the electrodynamic field equations.[30]

Schwinger gave a lucid, well-organized presentation which impressed everybody. Halpern treated the young man with respect and soon the two were exchanging ideas on a problem of electron scattering theory. They discussed Mott's study of electron scattering[31] in which he had made use of Dirac's theory to compute the cross-section for the elastic scattering of electrons from the Coulomb field of nuclei. Julian's familiarity with this work dated from days before he wrote his 'Paper Number Zero' [0]. Halpern suggested that Julian investigate the discrepancy between the measured distributions of polarizations of electrons scattered by nuclei and Mott's theoretical values. Mott's calculations were relativistic and had been carried to one order beyond the lowest

Born approximation. This was necessary because the experimental tests of theoretically derived cross-sections for one-on-one collisions were intrinsically difficult. Even if very thin metal foils were used as targets, a substantial portion of electrons interacted with more than one nucleus. Another significant higher-order process that affected the results was the quantum shielding effect from atomic electrons. Mott had not considered the shielding effects, which are relevant only for small values of the scattering angles, but included double scattering on two separate nuclei in the target. He noticed that since Coulomb scattering processes were spin dependent, the electrons that took part in two consecutive interactions should have partially polarized spins. The experimental data showed no evidence of polarization and the reason for this discrepancy remained obscure.

In fact, Mott's final expression for the cross-section was incorrect, but the error went unrecognized for a long time until the calculation was redone in 1948 by Feshbach and McKinley.[32] However, in 1934 so little was known about the nature of forces at nuclear distances that ascribing the discrepancies to a still unknown additional interaction between the electron and the nucleus was a question worthy of investigation. On Halpern's advice, Schwinger repeated Mott's calculations for the case of a Coulomb potential slightly weakened by the admixture of a short-range repulsive potential of the type

$$V(r) = \frac{b}{r^5}, \tag{1.7}$$

truncated at short distances from the center to avoid the effects of the singularity at the origin. Julian found that the magnitudes of the free parameter $b$ and the required short distance cutoff could be appropriately fitted to make the polarization effects disappear from the second-order perturbation.

The assumption worked because the correcting potential was significant only near the surface of the nucleus where the Coulomb interaction is strong, and the electrons scatter at large angles. Incidentally, this was the area where the effects of Mott's error were most significant. Also the supplementary potential weakened the Coulomb field of the nucleus, thus simulating some effects of shielding. Therefore it is not surprising that with a proper choice of the inverse power of the distance and of the parameter $b$ Schwinger found a fit for a single physical quantity calculated in low orders of the perturbation expansion. Still, for Julian it was real research work and he remembered it as a fascinating and illuminating experience. It was indeed the first time that he, entirely by himself, had successfully carried out a complete, fully relativistic, perturbation calculation by a proper accounting of spin effects.

In August 1935, shortly after publishing the paper with Halpern, Schwinger (with Motz) submitted another letter to the *Physical Review*, entitled 'On the $\beta$-Radioactivity of Neutrons' [2]. Like the preceding paper, it contained the results of research completed by Julian as a student at the City College. Schwinger had no special regard for this article, which contained the results of a not very original calculation applied to an unsuccessful model of weak interactions. However, one must remember that it was produced by a 17-year-old trying to resolve valid theoretical questions related to beta decay, and not even a full three years after the discovery of the neutron! Therefore it is worthwhile to compare the events of young Schwinger's life with the scientific revelations that began with the discovery of the neutron.

When in 1932 James Chadwick announced the surprising discovery[33] of a chargeless constituent component of the atomic nucleus, the neutron, Schwinger was 14. One year later, in October 1933, the seventh Solvay Conference was convened.[34] For nuclear physics it was an important event, marked by the general acceptance of the Heisenberg and Majorana two-body theories of nuclear forces based on the exchange principle, which reasonably explained, to within an order of magnitude, the nuclear binding energies and disintegration rates through alpha particle emissions. As for beta decay, it was not even clear whether free neutrons are stable or can decay spontaneously. Wolfgang Pauli's intriguing proposal of the neutrino made at that conference began slowly to prevail, although even great physicists like Bohr and Heisenberg still remained unconvinced, and thought that energy and angular momentum might not be conserved in the neutron's disintegration.

Pauli's hypothesis of the new particle, and the discussions in Brussels, inspired Fermi to propose a Hamiltonian interaction for weak interactions which, with some modifications, reigned under the rubric of 'the four-fermion theory' until the advent of modern gauge theories. Fermi's article was rejected by the editor of *Nature* as being 'too speculative,' but he published it in a shortened version in *La Ricerca Scientifica*, and later in full detail in Italian and German in *Il Nuovo Cimento* and *Zeitschrift für Physik*, respectively.[35] When the latter two articles appeared in 1934, Schwinger was 16.

Fermi wrote his Hamiltonian as

$$H = g[Q(\Psi_2\Phi_1 - \Psi_1\Phi_2 + \Psi_3\Phi_4 - \Psi_4\Phi_3) + \text{complex conjugate}], \qquad (1.8)$$

where $\Psi_i$ and $\Phi_i$ represent components of electron and antineutrino spinor wavefunctions, respectively, $Q$ is the operator that transmutes a proton into a neutron, and $g$ is the strength of the weak coupling responsible for beta decay. This Hamiltonian was soon replaced by a more general four-fermion interaction

$$H = g[(\overline{\Psi}_e O_L \Psi_\nu)(\overline{\Psi}_p O_H \Psi_n) + \text{complex conjugate}], \qquad (1.9)$$

where the operators $O_L$ and $O_H$ act on what are now called the leptonic (electron and neutrino) and hadronic (proton and neutron) field components, respectively, and are constructed from the Dirac matrices as Lorentz scalars, pseudoscalars, polar or axial vectors, or tensors.

In 1935 Konopinski and Uhlenbeck[36] found significant discrepancies, mainly in the low-momentum part of the electron spectrum, between weak decay spectra and the predictions of the Fermi theory. They tried to improve the agreement by replacing the polar vector coupling with a space–time gradient of the neutrino wavefunction,

$$\left(\frac{\partial}{\partial x_\mu}\overline{\Psi}_\nu\right)\Psi_e. \tag{1.10}$$

The life of the Konopinski–Uhlenbeck model, because of the derivative coupling being even more singular than the four-fermion theory, was short. It was unsuccessful and quickly abandoned: however, in 1935, only a year after the introduction of the four-fermion theory, there was no reason to reject it out of hand. It appeared attractive to Lloyd Motz, and his 16-year-old collaborator was already perfectly able to apply the Konopinski–Uhlenbeck Hamiltonian in practical calculations of the neutron's lifetime and the cross- section for nuclear reabsorption of an antineutrino from a decaying neutron. Julian found no difficulty in carrying out a standard quantum-mechanical calculation which, in the first order of approximation, followed from the expression given by Konopinski and Uhlenbeck,

$$P(E) = \frac{g^2}{2\pi^4}\langle|H|\rangle^2 pE(E_0 - E)^2 \tag{1.11}$$

for the probability $P(E)$ of the process $\mathrm{n} \to \mathrm{p} + \mathrm{e}^- + \overline{\nu}$, as a function of energy $E$ of the electron (in units of the electron's rest mass). Here, $E_0$ is the total energy released in the process. The calculation produced an inaccurate estimate of about 3.5 days for the neutron's half-life (the correct lifetime is 15 minutes), and a (then) negligibly small probability of a neutrino capture in the extremely rare process $\mathrm{p} + \overline{\nu} \to \mathrm{n} + \mathrm{e}^+$.

## Conclusion

By 1935, at the age of 17, after Julian Schwinger had spent two years at City College, it was already clear that he would makc a major mark in physics. He had mastered the literature of the most fundamental branch of science at the time, nuclear physics, and was beginning to make original contributions to research. All those who were in contact with him then were aware of his prodigious powers. All that was needed for his genius to flower was a transplantation to a research environment. The career of one of the greatest American physicists of the twentieth century had begun.

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# Julian Schwinger at Columbia University

## Transfer to Columbia

Although Benjamin Schwinger was not able to fulfill his dream of sending his sons to Columbia University, Julian did not think much about it; in any case, he became a frequent visitor to the excellent Columbia University Library. The Schwingers lived quite close to the University, and Julian often walked there, casually entering the library, picking up a book and finding a quiet place somewhere to read. This normally unallowed procedure continued for several months until one day a librarian, puzzled by his young age, asked Julian whether he had library privileges. He lied to her that that he did, and she asked for his name, then looked up the list of library card-holders to verify. To the amazement of the young boy, who was already resigned to hear a reprimand, a strange thing happened: She indeed found a Schwinger on the list of registered users! From then on, mistaken for this unknown relative, Julian continued to use the library at will until he became a regular student at Columbia.[1]

In early 1935 Schwinger also began frequenting seminars and colloquia at Columbia in the company of Lloyd Motz. He found them exciting and became a regular visitor there. After he had been attending these events for several months, Isidor Rabi noticed him and, intrigued by his youth, asked Motz: 'Who is that sleepy-eyed kid you bring along with you?' Motz explained that 'he is a very brilliant, incredibly bright sophomore from the City College' and promised to bring him over one day and introduce him to Rabi.[2] The occasion presented itself soon, when one day Julian and Motz were talking in front of the library. The library and Rabi's office opened on to the same hallway on the eleventh floor of the Pupin Physics Laboratory. Suddenly the door opened and Rabi appeared; he invited Motz into his office to discuss 'a certain paper by Einstein in the *Physical Review*.' Motz introduced Julian and asked if he could bring his young friend along; Rabi did not object, and so it began.[1]

The Einstein article turned out to be the famous paper of Einstein, Podolsky, and Rosen,[3] with which young Julian was already familiar. He had studied quantum mechanics with Professor Wills at the City College, and discussed with him the problem of the reduction of a wave packet after additional information about a quantum system is gained from a measurement. 'Then they [Rabi and Motz] began talking and I sat down in the corner. They talked about the details of Einstein's paper, and somehow the conversation hinged on some mathematical point which had to do with whether something was bigger or smaller, and they couldn't make any progress. Then I spoke up and said, "Oh, but that is easy. All you have to do is to use the completeness theorem." Rabi turned and stared at me. Then it followed from there. Motz had to explain that I knew these things. I recall only Rabi's mouth gaping, and he said, "Oh, I see. Well, come over and tell us about it." I told them about how the completeness theorem would settle the matter. From that moment I became Rabi's protégé. He asked, "Where are you studying?" "Oh, at City College." "Do you like it there?" I said, "No, I'm very bored." '[1]

Watching young Julian demonstrate such 'deep understanding of things that were at the time at the frontier and not clearly understood,'[2] Rabi decided on the spot to talk to George Pegram, then chairman of the physics department and dean of the graduate faculty, to arrange Julian's immediate transfer to Columbia. He and Motz left Julian waiting and went to see Pegram who also had an office in the same building. Motz stayed behind and waited outside Pegram's office. Rabi emerged a few minutes later with the word that there might be a scholarship available and Pegram would help in carrying the transfer through. Motz hurried to bring the good news to Julian, but he was astonished to find the independently minded Schwinger hesitate. The unique intellectual atmosphere of the City College where he had made many friends and felt at home had worked very well for him so far; therefore he decided not to rush and first to seek a transfer to the honors program at the College, and only if this didn't work would he accept Rabi's offer.[2]

The honors program at the City College was generally available, with the approval of the physics department's chairman Charles Corcoran, to the best physics majors after they had completed the core curriculum physics courses, but Schwinger had finished only the basic first-year requirements and was at odds with Corcoran for not returning his laboratory reports. Therefore Motz felt that he had better chances with Corcoran than did Julian, and offered to bring up the subject with the boss himself. The chairman had already heard about Julian, but the City College at that time was a unique place, full of excellent students, brought up with the attitude of studying and passionate about learning, more brilliant than the faculty, who had come from backgrounds that did not emphasize intellectualism much.[2] The place abounded with talent and Corcoran did not see anything extraordinary in Schwinger, whose grades outside mathematics and physics were quite abominable because he always performed poorly if the nature of the course did not agree with his individualistic patterns of study. After Motz made an impolitic remark that Julian knew more about physics than did most people on the faculty, Corcoran bristled with anger and ruled that the proposition was out of the question. According to Bernard Feld, 'Corcoran is alleged to have said, "Over my dead body. As long as I'm chairman of this department, no smart-ass kid is going to be allowed to skip taking my course in elementary physics."'[4] Several days later he even criticized Motz in a department meeting for trying to ruin the fine-tuned process of educating the young man in the only natural way, that is gradually.[2,*]

Rather upset, Schwinger returned to Rabi and asked him to set the process of transfer to Columbia in motion. To Rabi's astonishment it turned out to be more difficult than he had expected. The obstacle was Julian's terrible grades. An official who examined his transcripts from the City College declared that on their basis Julian could not even be admitted to Columbia University. Rabi felt a little insulted and asked: 'Suppose he were a football player?' and decided to override the administration with Pegram's assistance and the help of Hans Bethe. Bethe provided an enthusiastic letter of support after he read Julian's notes on electrodynamics.[6] Bethe's letter, dated 10 July 1935, reads as follows:

'Dear Rabi,

Thank you very much for giving me the opportunity to talk to Mr. Schwinger. When discussing his problem with him, I entirely forgot that he was a sophomore 17 years of age. I spoke to him just as to any of the leading theoretical physicists. His knowledge of quantum electrodynamics is certainly equal to my own, and I can hardly understand how he could acquire that knowledge in less than two years and almost all by himself.

He is not the frequent type of man who just "knows" without being able to make his knowledge useful. On the contrary, his main interest consists in doing research, and in doing it exactly at the point where it is most needed at present. That is shown by his choice of his problem: When studying quantum electrodynamics, he found that an important point had been left out in a paper of mine concerning the radiation emitted by fast electrons. That radiation is at present one of the most crucial points of quantum theory. It has been found to disagree with experiment. It is quite conceivable that the error which Mr. Schwinger found in my paper might bring about agreement between theory and experiment which would be of fundamental importance for the further development of quantum electrodynamics. I may add that the mistake has not

* 'The rigidity of Corcoran's concerning the physics department's requirements was typical of the whole CCNY curriculum. There was an astoundingly large number of required courses outside the major, which just couldn't be avoided.'[5]

only escaped my own detection but also that of all the other theoretical physicists although the problem has been in the centre of discussion last year.

The way in which Schwinger treated his problem is that of an accomplished theoretical physicist. He has the ability to arrange lengthy and complicated calculations in such a way that they appear simple and can be carried out without any great danger of errors. This gift is, I believe, the most essential requirement for a first-class theoretical physicist besides a thorough understanding of physics.

His handling of quantum theory is so perfect that I am sure he knows practically everything in physics. If there are points he does not know, he will certainly be able to acquire all the necessary knowledge in a very short time by reading. It would be just a waste of time if he continued listening to the ordinary physics course, 90% of whose subject he knows already while he could learn the remaining 10% in a few days. I feel that nobody could assume the responsibility of forcing him to hear any more undergraduate (and even the ordinary graduate) physics courses.

He needs, of course, some more courses in minor subjects, principally mathematics and chemistry and a small amount of physical laboratory work. In physics the only thing he has to learn is teaching physics, i.e., to explain himself very simply—an art which can be learnt only by experience. He will learn that art automatically if he works at a great institution with other students of similar caliber.

I do not need to emphasize that Schwinger's personality is very attractive.

I feel quite convinced that Schwinger will develop into one of the world's foremost theoretical physicists if properly guided, i.e., if his curriculum is largely left to his own free choice.'*

Eventually, Schwinger was admitted to Columbia as a junior, with a full tuition scholarship starting in September 1935, but in the preceding summer semester he had to take some required courses that he had missed out at the City College.[1]

Rabi laid down a contract to Julian. 'You're coming here and you are going to take all undergraduate courses and I want you to get As in all those classes.'[2] Julian obliged for a while, but soon returned to his own individualistic ways. He disliked writing themes or laboratory reports and treated them as a nuisance that distracted him from his real vocation, which was learning physics. He admitted unabashedly: 'I did not learn anything in my physics courses [other] than what I already knew from my own private studies.'[1] Therefore, to avoid any pressure to attend lectures he began to develop work patterns which gradually drifted into later and later working hours, extending deep into the night. He

* We are grateful to Karl von Meyenn for bringing to our notice a complete photocopy of Bethe's letter to Rabi.

would sleep through the day and show up at Columbia around 6 o'clock in the evening. He spent most of his time reading advanced texts in physics and mathematics and journal articles in the library, and writing papers. It was a relatively simple matter for him to pass oral examinations by stunning his professors who watched him inventing on the spot his own proofs or non-standard methods of approaching standard problems. Sometimes this did not work, and he flunked the chemistry course of Victor LaMer, who had the custom of introducing his own peculiar notation and demanding that his students make full use of it, and which was obscure to anybody who did not attend his class regularly.[7],*

Rabi recalled, 'LaMer was, for a chemist, awfully good. A great part of his lifework was testing the Debye–Hückel theory[9] rather brilliantly. But he was this rigid, reactionary type. He had this mean way about him. He said, "You have this Schwinger? He didn't pass my final exam." I said, "He didn't? I'll look into it." So I spoke to a number of people who'd taken the same course. And they had been greatly assisted in that subject by Julian. So I said, I'll fix that guy. We'll see what character he has. "Now Vicky, what sort of guy are you anyway, what are your principles? What're you going to do about this?" Well, he did flunk Julian, and I think it's quite a badge of distinction for him, and I for one am not sorry at this point, they have this black mark on Julian's rather elevated record. But he did get Phi Beta Kappa† as an undergraduate, something I never managed to do.'[6]

Norman Ramsey added an amusing footnote to this story. In 1948 Schwinger had to repeat his brilliant lecture on quantum electrodynamics three times at the American Physical Society meeting at Columbia, in successively larger rooms.‡ 'It was a superb lecture. We were impressed. And as we walked back together—Rabi and I were sitting together during the lecture—Rabi invited me to the Columbia Faculty Club for lunch. We got in the elevator [in the Faculty Club] when who should happen to walk in the elevator with us but LaMer. And as soon as Rabi saw that, a mischievous gleam came into his eye and he began by saying that was the most sensational thing that's ever happened in the American

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* It was a dull course with a dull exam. A question on the final exam was 'Prove that $d\epsilon = d\xi + d\eta$,' where none of the variables $\epsilon$, $\xi$, or $\eta$ were defined.[8]

† Phi Beta Kappa is the most honored academic fraternity of young American students, to which they are elected by their peers and seniors entirely on the basis of academic excellence.

‡ K. K. Darrow, secretary of the Physical Society, who apparently had little appreciation of theory, always scheduled the theoretical sessions in the smallest room. Schwinger's second lecture was given in the largest lecture hall in Pupin Lab, and the third in the largest theatre on campus.[8]

Physical Society. The first time there's been this three repeats—it's a marvelous revolution that's been done—LaMer got more and more interested and finally said, "Who did this marvelous thing?" And Rabi said, "Oh, you know him, you gave him an F, Julian Schwinger." '[7]

Somewhat later George Uhlenbeck came from Holland as a visiting professor to Columbia and taught a course in statistical mechanics in which he was a great expert. A large number of students signed up to take his class and many faculty members also attended. Schwinger registered, but never went to class, and did not bother to take the final exam. Uhlenbeck complained to Rabi that he was not even given a chance to see the invisible student. Rabi became infuriated. He knew that Schwinger had just begun dating and felt concerned that he might be getting distracted away from physics. He decided to see Julian in person and ordered him to take the oral examination (as was the Dutch custom) immediately. Schwinger bargained that he would do so but only at 10 o'clock in the evening. This request was beyond Uhlenbeck's limit of tolerance, yet a special examination date was arranged for 10 o'clock in the morning. With Schwinger's answers in the examination, Uhlenbeck was overwhelmed: 'I can say nothing. Not only did he hand in a perfect paper, but he did it in the way I did everything, as though he had sat through every lecture. This is amazing. So I have nothing to say,' he declared to Rabi.[2,6,10]

Schwinger's lack of attendance at lectures and completion of coursework caused other problems as well. Many years later Norman Ramsey recalled that when he proposed him for membership in Phi Beta Kappa, many people objected and cited his uncompleted courses and bad grades. Julian was eventually elected to Phi Beta Kappa, but only after a big argument in which Ramsey pointedly remarked that Schwinger had published more papers that year than anybody on the faculty.[7,8]

At Columbia, Julian had somewhat severed his relations with most of his peers in class. To a greater extent than at City College, when because of his young age he could develop by emulating his fellow students, at Columbia he benefited more from the faculty. He had good working relations with graduate students and professors and generally enjoyed interacting with people. He participated in seminars and was a good listener, since from his early articles it is evident that he had a detailed knowledge of the most recent experimental data and formal developments. He also offered himself as a lecturer in seminars and discussion groups and discussed matters in a mature manner.

Lloyd Motz vividly described one such seminar talk, given at a weekly tea meeting of the Astronomy Journal Club run by Jan Schilt and attended by astronomers from colleges and universities in the New York metropolitan area. Neutron stars were then the hot topic in their discussions, some two years before the definitive paper of Oppenheimer and Volkov appeared.[11] Schilt looked for someone able to present at the Journal Club the rules of the new quantum statistics, the questions related to quantum degeneracy, properties of the degenerate electron gas, and similar topics. Motz suggested Schwinger as the most suitable person for the task. Julian quickly agreed: 'Sure, no problem. As soon as you want me!'[2] The lecture was a revelation. 'Everything was perfect. He would begin writing at one end of the blackboard, and then finish at the other. He would do things with his left hand. He was ambidextrous so he would write on the board with both hands. It was all so beautiful. It came out of him the way music came out of Mozart, as though he had been born with it. He never made a mistake. It didn't matter what question you would ask him; he always had a ready answer.'[2]

Julian sought isolation for his work. Of course, the habit of shifting to work late in the evening and sleeping through the day had to be in conflict with the basic responsibilities of the undergraduate's life. Julian ceased attending classes; he felt he did not need them. Very likely he shifted his life into the night pattern just to avoid being pressured to go to classes and waste his time listening to other people explaining what he already knew. He was indeed a very strange kind of undergraduate, whom Rabi often asked to be his substitute for teaching a graduate quantum mechanics class for him when he was away or had other engagements. According to Rabi, 'Whenever I had to go away, I'd ask Julian, who was an undergraduate, to take the class. I can assure you it was a great improvement. He's a much better teacher than I ever was.'[6]

Rabi praised Julian enormously for his willingness to offer help in any calculation; he would not stop at the final formula, but work with the phenomenological data until he could produce a final number as an answer. Similarly, he was very friendly and helpful in his interactions with fellow students. Morton Hamermesh recalled Schwinger teaching him (!) group theory, and the intricacies of using Bessel functions in theoretical calculations: he coached Hamermesh for days, several hours at a time.[12]

Rabi had great confidence in his protégé, but it was not limitless. He was afraid that one day his genius would turn out to be a flash in the pan and he had to reassure himself periodically by introducing Schwinger to any physicist of consequence who visited Columbia. They all left impressed by his age and by the sheer volume of knowledge he had acquired. Wolfgang Pauli wrote a letter to Rabi saying how impressed he was and closed with the words, 'And give my love to this physicist in knee pants.' Recall that in the summer of 1935 while Rabi was trying to get Julian into Columbia, Hans Bethe arrived and Rabi asked him to assess Julian's progress and send a written evaluation. Bethe sent a letter full of superlatives including a strong endorsement. After receiving this remarkable assessment, a happy and beaming Rabi showed Bethe's letter to Motz with the words, 'Now, I am satisfied.'[2]

## Spin resonance

At Columbia University Schwinger, for a while, continued his research contacts with Halpern and Motz, but soon he gained so much confidence in himself that he did not need their support and encouragement. Besides, they were no match for him in the speed of doing calculations. By September 1936, at the age of 18, only one academic year and two summer sessions after his matriculation at Columbia, Julian received his undergraduate degree and, in passing, produced a quantity of research ordinarily considered sufficient for a doctoral dissertation. Rabi recalled that it was not altogether trivial to get Julian's undergraduate degree in such short time. Columbia required more than just completing a sufficient number of hours. You had to have 'a certain weight of ordinary credits and a certain weight of maturity credits. One Sunday morning I was called up by the dean, Dean Hawkes, and he said, what shall I do about Schwinger? I said, what's the problem. He said he has enough credits to graduate but he hasn't enough maturity credits. It seemed too absurd. How can you talk about things that way? So I said, well, you have your rules. I don't know what you can do about it. I wasn't going to make a great plea. See how the thing'd work. Well, he was a real man, and on Sunday, he was a religious person, he said, I'll be damned if I won't let Schwinger graduate because he doesn't have enough maturity credits. Of course, this gave me great faith.'[6] It seems that Edward Teller was the first person to deem Schwinger's work on neutron scattering as worthy of a PhD,[2,13] but the requirements of the graduate school at Columbia set a minimum two-year residence period for doctoral candidates. Considering Julian's young age, there seemed to be no compelling reason to depart from this rule. Schwinger himself did not see any point to the rule: 'Why they didn't let me out of Columbia two years earlier, I will never know.'[1] In the meantime he registered for more courses, and ever faithful to his custom, he seldom if ever attended classes and kept on working on problems of scattering theory and spin. He soon became a sought-after expert in this subject, a real catalyst (Motz called him a 'spark plug') for Rabi's spin resonance team and also J. R. Dunning's cyclotron experimental group, where he helped to interpret the influx of data produced with the use of this emerging (just five-year-old) technology.

Julian carried around ever thicker-growing notebooks, but never felt compelled to write articles, even though Motz and Rabi insisted that he finally write up at least a part of his results. Finally, during the year 1937, Schwinger published five papers in the *Physical Review*. They became his doctoral dissertation; he never sat down to write a doctoral thesis as such, but submitted a bound-up set of these papers as his dissertation.

The common trait of these articles was that they were all devoted to spin and magnetic moment-dependent aspects of neutron scattering. In 1936 and

1937 so little was known about the neutron that in his comprehensive review of nuclear physics, published then in the *Reviews of Modem Physics*,[14] Hans Bethe still had to invoke the argument of simplicity to justify a value of one-half for neutron's spin over an equally plausible magnitude of three-halves. Due to the fact that the neutron has no charge, nuclear physicists had to rely on indirect information from nuclear spins and from the data gathered in proton–neutron and proton–deuteron scattering experiments.

Rabi remained the strongest influence upon Julian at that time and it was therefore no accident that the first two articles Schwinger wrote at Columbia, begun while still an undergraduate, were related to his interests. In these articles, Schwinger improved upon or corrected the works of Rabi and Felix Bloch. The first of his Columbia papers was a full-length article 'On the Magnetic Scattering of Neutrons' [3], which included the work he had completed by himself, albeit with Rabi's blessing, in 1936. Earlier that year, Felix Bloch had proposed a technique for measuring the neutron's magnetic moment from the spatial distribution of neutrons scattered twice on targets magnetized in different directions.[15] Bloch argued that since the range of nuclear interactions is short and the neutrons carry no charge, the scattering of thermal neutrons from atomic targets is dominated by the magnetic interaction between the neutron's spin and the atomic electrons. Therefore an unpolarized stream of neutrons scattered by a magnetized target (ideally by saturated magnetized iron plates) becomes partially polarized. If it is then scattered for the second time, the angular distributions of emerging neutrons depend on the relative orientations of magnetization vectors of the targets and on the magnitude of the neutron's magnetic moment, which can therefore be determined from such data. Soon after the publication of Bloch's paper, a preliminary experimental trial of the double scattering method was performed by Bethe, Hoffman, and Livingston.[16] Their experiment had indeed registered an asymmetry in the scattered beam caused by rotating the magnetization vector of the analyzer, but the effect was too small to produce any reliable estimate for the magnitude of the magnetic moment.

Schwinger did not trust Bloch's calculations, which were based on the classical form of interaction between two magnetic dipoles. Julian had done somewhat similar work on Coulomb double scattering for his paper with Halpern [1], and now he decided also to recalculate the magnetic effect. He employed the techniques he had learned from Mott and Massey's *Theory of atomic collisions*,[17] and used an interaction Hamiltonian in which in addition to the term corresponding to the magnetic interaction between neutron's magnetic moment and electron spin, he also included the nucleonic potential at the position of the neutron. According to the current practice (which soon thereafter, also thanks to Schwinger's work, was found to be incorrect [13]), Julian considered it to be a potential of a central (spin-dependent) force of still unknown nature. Such a form of interaction was then known as Wigner's force, although Schwinger attributed it to Van Vleck.[18] The situation was difficult, because this unknown interaction was strong and had to be included exactly, while the better-known magnetic force was to be treated as a perturbation. However, Schwinger was still able to carry out the calculation because only the slowest thermal neutrons spend a long enough time in the proximity of the target atoms to experience any significant magnetic effects. He knew that thermal neutrons do not create metastable states with iron nuclei and, having zero orbital momentum, scatter on the nuclear potential in a spherically symmetric manner almost independently of the actual form of the Hamiltonian. For the cross-section for long-range magnetic scattering only the asymptotic forms of the wavefunctions corresponding to the scattering by nuclear forces are important and, for the zero orbital momentum neutrons, the single most relevant parameter that characterizes that asymptotic behavior is the phase shift of the scattered S-wave with respect to the incident wave. It is linked to the overall strength of the nuclear force, and Schwinger was able to infer its magnitude from other neutron-scattering experiments.

This allowed Schwinger to proceed to the next step of the approximation, using the magnetic moment–spin interaction as a perturbation. This fully quantum-mechanical calculation produced an angular distribution and spin density of the elastically scattered neutron different from Bloch's. In addition to the classical term, which Bloch had correctly derived, it included a pure quantum term in the part dependent on the spin density of the incident radiation. Julian continued with a discussion of how to configure the experimental trials optimally. He applied his results to the scattering of polarized and unpolarized beams from ferromagnets. Then he analyzed the practical limitations of measuring the spatial distributions of neutrons double-scattered from two separate magnetized targets. The thoroughness of this analysis substantiated Rabi's opinion that Julian represented the ideal of a theorist from an experimentalist's viewpoint, one who was always willing and able to come up not only with a general analysis but also with 'a final number as an answer.'[2]

Schwinger found that 'the intensity of double scattering with parallel orientation of magnetizations [could be] 15 times that with antiparallel orientation. However, despite the large magnitude of the asymmetry, this effect will be difficult to detect with present methods because of the small intensity of the double-scattering neutrons.' Thus he proposed studying the induced polarization of the undeflected beam. If the transmitted beam was subsequently scattered, the experimenter could measure a polarization asymmetry defined as the 'difference in intensity between antiparallel and parallel orientations of magnetization divided by the average intensity.' The asymmetry, in certain configurations, could reach a value of more than 90%. He also proposed a double transmission experiment in which there was a compromise between having sufficient intensity transmitted and yet having a substantial polarization. Polarization asymmetries of about 40% were nevertheless achievable.

Although they were more feasible in yielding information than was double scattering, double transmission and transmission scattering were never successfully applied for the purpose of measuring the neutron's magnetic moment. Instead, subsequent experiments were based on resonance depolarization in neutron beams and were similar to the method used in Rabi's original molecular beam spin resonance apparatus, except that the neutrons passed not through Rabi's constant magnetic fields with opposite gradients but through ferromagnetic plates.[19] By then the more elaborate theory of magnetic interactions of neutrons had already come to exist, but Schwinger's calculation represented the first correct quantum-mechanical quantitative description of Bloch scattering.[20]

Hans Bethe was the referee of the paper, and, while praising it, suggested it be rewritten to emphasize the difference between the classical interaction between the dipoles, used by Bloch, and the correct Dirac treatment. He suggested that Schwinger was being too modest. The editors of the *Physical Review*, however, disregarded this advice, and the paper was published unchanged.[13]

Schwinger's next article in 1937 [4] appeared side-by-side with a related paper of Rabi on magnetically induced spin transitions in atomic beams.[21] Like the previous article on Bloch scattering, it contained a detailed and expanded calculation of an effect that had been previously analyzed semi-classically, this time by Rabi, who had studied the behavior of spin one-half atoms in a precessing magnetic field.[22] A few attempts on the theory of this effect already existed, but they were very limited in scope, and Motz, who looked at Rabi's results, found a troublesome discrepancy between them and those obtained somewhat earlier by Güttinger[23] for the case of a rotating magnetic field. The discrepancy demanded immediate reconsideration with the help of rigorous quantum-mechanical procedures, and Rabi presented Julian with the task of performing it.

The underlying physics involved the classic problem of the evolution of a state coupled to a variable external field. If the transition between any two states of the field was rapid (as in the case of a sudden reversal of an external magnetic field), the dynamical state of the system would be unchanged. On the other hand, in the case of an adiabatic transition (such as the infinitesimally slow rotation of the same field by 180°), Ehrenfest's adiabatic theorem applies and the system follows the external changes continuously and gradually evolves from one energy eigenstate into another.

Being interested in the most general case of a time-dependent interaction, Schwinger considered the Schrödinger equation for a system with a

Hamiltonian which involved no time-dependent variables except those associated with the external field. In such perturbative calculations one expands the wavefunction in a complete set of orthogonal energy eigenfunctions, treating the coefficients of expansion as time-dependent functions depending on the external field. Knowledge of these functions suffices for finding the transition amplitudes; however, the coefficients must be determined from the equations that expressly involve the energy eigenfunctions themselves. Therefore, in general, it is necessary first to solve the full dynamical problem and find the eigenfunctions, and only then proceed with the calculation of the lifetimes of individual energy levels.

In the simpler case of a magnetic field rotating with constant angular velocity, Güttinger had derived a set of equations for the coefficients that had the advantage of not involving the eigenfunctions, but only the energy eigenvalues, which in some cases could be inferred without the complete knowledge of the individual eigenfunctions. Starting from scratch in the general case, Schwinger recovered the Güttinger equation, but with an additional term which included the eigenfunction. Julian found a way of solving for this function in a general case and expressing this term by means of the angular momentum, magnetic quantum numbers, and the spherical components of the external magnetic field. This additional term happens to vanish for the transitions induced by a steadily rotating magnetic field; therefore Güttinger's results were correct for the case he considered, but not for the case of Rabi's precessing field. That is, only in the case when the magnetic field was perpendicular to the precession axis was Güttinger's result correct. This explained the discrepancy found by Motz, who had used the unmodified Güttinger equations outside the bounds of their applicability.

This paper was a precursor to Schwinger's later definitive work on the theory of angular momentum. As Schwinger noted, 'In fact, this was the origin of the work I did later about the general theory of angular momentum and so on [69]. But the whole interest in angular momentum goes back to these Rabi, molecular, atomic beam problems. And I'm sure this was done while I was still an undergraduate, or very soon thereafter.'[1] Norman Ramsey, then Rabi's graduate student, characterized the significance of the Rabi–Schwinger papers: 'They are the fundamental papers for nuclear magnetic resonance.'[8]

## 'Because I, not my distinguished colleague, wrote it'

In the mid-1930s convincing evidence had emerged that nuclear forces were spin dependent. For example, neutron–proton binding forces were found to be much stronger than the forces between the neutrons or the protons themselves. Also, the exclusion principle ruled out any binding between pairs of neutrons or protons unless they had antiparallel spins so they could form only singlet bound states. No such restriction applied to neutron–proton pairs, yet no singlet states had been observed. In 1935, Gregory Breit and Eugene Wigner pointed out[24] that if one includes a singlet state in neutron–proton scattering processes then, in order to provide even crude agreement with experimental data, the singlet and triplet bound states must yield drastically different contributions to the total cross section. This could not be explained on the basis of existing models or small modifications of them.

As we have noted earlier, global effects of scattering of slow neutrons are well described by the phase shift of the scattered wave with respect to the incoming $S$-wave. The phase shift is a dynamical quantity dependent on the interaction potential. Since it is readily calculable and directly connected to the total cross section for $S$-wave scattering of neutrons it was used as a convenient tool in model testing, together with the Fermi scattering length, which is the radius of a sphere of surface area equal to the total cross section taken with a sign depending on that of the phase shift. At low energies, the relation between the phase shift $\delta$, the energy $E$, and the scattering length $a$ is

$$k \cot \delta = -\frac{1}{a}, \quad k = \frac{\sqrt{2mE}}{\hbar}, \tag{2.1}$$

$m$ being the mass of the neutron.

The scattering length depends on the volume of the potential well, but is relatively insensitive to its shape. Therefore all initial attempts to adjust the form of the nuclear potential to achieve satisfactory accord with experimental data failed hopelessly. For example, Wigner's calculation[25] with the use of rectangular well potentials produced low momentum neutron–proton cross sections of about two and a half barns,* while the experimental value was then thought to be about 13 barns.[26] Wigner suggested that there must also exist a singlet neutron–proton bound state, different from the triplet ground state of a deuteron.[27] It ought to have a very small binding energy but a very large scattering cross section at low energy. The total cross section would then be a sum of the cross sections due to the singlet state and those due to triplet states with statistical weights of one and three-quarters, respectively. However, the singlet binding energy revealed little about the nature of the binding potential. As a first step, the sign of the scattering length was needed because the sign of the ratio of the triplet to the singlet scattering lengths determined whether the singlet state was real or virtual, that is, whether the binding energy was positive or negative. No such information could be found from $S$-wave neutron scattering cross sections by protons in bulk matter.

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* A barn, an originally facetious term referring to its unexpected largeness, is $10^{24}$ cm$^2$.

In 1936, Edward Teller remarked that, if nuclear forces are spin dependent, one should expect differences between the scattering cross sections in ortho- and parahydrogen,[28] which have parallel and antiparallel spins, respectively. He also noticed that since the waves scattered on two hydrogen nuclei in a molecule interfere, such scattering should provide information about things like the sign of the scattering length and the range of the *n–p* force.

Schwinger learned about Teller's suggestion from Bethe's review articles in the *Reviews of Modern Physics.*[14] He saw it as another opportunity to deploy his skill in calculations involving spins and started to compute the cross sections without hesitation. He progressed rapidly and soon he had some results to show to Rabi. Rabi suggested that he should go to Washington and discuss them directly with Teller, who was then at George Washington University. Teller was very interested in solving the problem of neutron scattering by molecular hydrogen, but apparently was not able to do the calculations by himself. He greatly welcomed help and invited Schwinger to come to Washington, and offered him a room to stay in his house.

Julian stayed with the Tellers for about two weeks, during which time he became timidly but intensely infatuated with the grace and enchanting accent of Teller's Hungarian wife, Mitzi.[1] This unexpected relapse into adolescence did not take his mind away from the project, which he continued and completed, doing all the calculations by himself. Apparently Teller offered advice and critique, but did not contribute to the progress of the work. The preliminary results of this somewhat uneven cooperation soon appeared in a letter to the *Physical Review* [5], and a regular article on 'The scattering of neutrons by ortho- and parahydrogen' [8] followed shortly thereafter.* The Schwinger–Teller paper quickly inspired experiments and thus this was the first Schwinger article which became a standard textbook reference. Schwinger made no bones about whom the credit for this work should belong to. In 1979, a collection of his major articles was published[29] and in it he provided pithy, often one-line comments on these selected papers. The punch line included on the paper with Teller read: 'Because I, not my distinguished colleague, wrote it.'

The article was written in the characteristic style of Schwinger's early papers, in which the details of complex calculations were mixed with phenomenological approximations based on generally scarce data, and which ended with the interpretation of possible results for future experiments. In the absence of an accepted theory of underlying forces, the calculation had to be essentially model-independent; thus, as in the case of Bloch scattering, it required neutrons

* It is interesting to note that the abstract of the article was quite long, a habit Schwinger often cultivated, and is nearly identical to the entire letter submitted two months previously.

of de Broglie wavelength large enough to be insensitive to the details of the spatial form of the nuclear potential. This restriction had an additional simplifying effect: Schwinger could calculate the coherent scattering cross–sections by simply summing up the scattering amplitudes from the two participating nuclei. Not having to worry about the radial dependence of the force, Schwinger treated the interaction potential as a contact interaction, vanishing unless the position of the proton and neutron, $\mathbf{r}_p$ and $\mathbf{r}_n$ coincide, and proportional to

$$\left[\frac{1}{2}a_t(1+Q)+\frac{1}{2}a_s(1-Q)\right]\delta(\mathbf{r}_n-\mathbf{r}_p), \tag{2.2}$$

where $Q$ is a spin operator constructed from the Pauli spin matrices of the proton and neutron and having the eigenvalue plus one in the triplet and minus one in the singlet state of proton–neutron system, and $a_t$ and $a_s$ are the scattering lengths for triplet and singlet spin states, respectively. The potential for coherent scattering was a sum of two terms of the type (2.2), one for each of the different hydrogen nuclei in a molecule. The final form of this sum turned out to contain two types of terms, one symmetric, the other antisymmetric in the proton spin. The antisymmetric part could induce transitions between the states of orbital quantum number differing by one unit, thus inducing conversions previously thought to be forbidden between the ortho- and parahydrogen. It was proportional to $a_t - a_s$ and even the very existence of such transitions, no matter how rare, would demonstrate a spin asymmetry of the nuclear interaction.

By treating the molecule as a quantum rigid rotator, and neutrons as plane waves normalized in a finite volume, Schwinger calculated the transition probabilities between the lowest energy levels of orbital angular momentum equal to zero or one, which were the only states that could significantly contribute to the total cross section at low temperatures. Experiments had to be performed at cryogenic temperatures so that neutrons had energies small compared with molecular rotational energy levels, which are different in ortho- and parahydrogen. With the rotational excitations eliminated, any difference in cross sections would have to be caused by the spin dependence of the interaction. The results confirmed Teller's expectations: the cross sections for ortho- and parahydrogen were different for very slow neutrons; moreover, the difference between the two depended very strongly on the relative sign of $a_t$ and $a_s$. The conclusion of the letter and the article was straight to the point. '(a) The orthoscattering cross section for liquid-air neutrons should be about 300 times the corresponding parascattering cross section. (b) The parascattering cross section for ordinary thermal neutrons should be roughly 100 times the parascattering cross section for liquid air neutrons. For a real singlet state, however, these ratios are of the order of one.' Although not stated explicitly in the original article, but as may be easily inferred from the cross sections given there, in the limit of zero initial energy for the neutron there exists a simple relation between the cross sections (which are purely elastic in this limit)

$$\sigma_{\text{ortho}} - \sigma_{\text{para}} = \frac{32\pi}{3}(a_{\text{t}} - a_{\text{s}})^2. \tag{2.3}$$

The approximate value of $a_{\text{s}}$, had already been found by Wigner, but now it became possible to determine its sign, since for $a_{\text{t}}$ and $a_{\text{s}}$, having opposite signs, the difference $\sigma_{\text{ortho}} - \sigma_{\text{para}}$ would be much larger than in the case of identical signs. Naturally, the former alternative appeared to be more likely, as it implied that the as-yet unobserved singlet energy level of a deuteron was virtual.

The chances for successful experimental applications were excellent. Indeed, many experimenters rushed to do so, and the results of the first experiment by Otto Stern and his collaborators were even published before the appearance of the article of Schwinger and Teller.[30] They confirmed the suspected virtual nature of the singlet state.

## Exploring the properties of neutrons

In 1937, having made the transition from being an undergraduate to a graduate student (a matter of pure technicality, since he had completed the entire graduate curriculum as an undergraduate), Julian Schwinger remained focused on the physics of neutrons, which was pursued aggressively by the research community at Columbia University. By then, Rabi began to realize that he had taught his protégé all he could. Therefore, he encouraged Julian to broaden his contacts and learn from new experiences by interacting with other physicists. Large numbers of interesting physicists came through Columbia, and Schwinger literally met all of them. With no more lectures to attend, he just did research; this was the goal he was aiming at and working for all along. He kept on lending his help to experimentalists and in the course of the next two years these collaborations proved to be fruitful; however, only a portion of all this work was ever published, often after a delay of several years. Some of it was presented as short communications at meetings of the American Physical Society, such as the one with Rabi on 'Depolarization by neutron–proton scattering' [6]. A conceptual descendant of the work with Teller, this paper discussed a method for determining the relative signs and magnitudes of singlet and triplet neutron–proton scattering lengths and described an alternative, viable, but not very practical, design of a suitable experiment in which the changes of the polarization vector of a neutron beam due to collisions with protons in a hydrogen-rich target would be used to determine the ratio of scattering lengths. The idea of such an experiment was Rabi's, while Schwinger derived the polarization formula which was the heart of this brief report: 'I just worked out the theory of it, which was two lines!'[1]

At the same Spring 1937 Washington meeting of the American Physical Society, Hyman Goldsmith and John Manley, both from Columbia, spoke about their joint experiments on neutron absorption [7]. Manley was about to move to the University of Illinois, but at the same time he was still working with Rabi's group. He was a talented experimentalist with considerable experience in molecular beams, but his interests were just then turning to neutron physics. Manley teamed with Goldsmith, who had a good knowledge of virtually all the literature on this subject, and they enlisted Schwinger's help to carry out the computation and interpretation of the data. This work eventually grew into a longer article [10], which addressed the puzzling problem of selective energy absorption of slow neutrons which had been described about two years earlier in England and the United States.[31] The absorption of neutrons had all the characteristics of a resonance process. The absorption rates changed if the beam had been previously filtered through a thickness of the same material as the absorber; they seemed to be greater in a given element if the same element was also used as a detector of radiation. The discovery of these properties had led to the concept of neutron 'groups,' actually neutrons of separate bands of different kinetic energies, labelled by letters and characterized by the element which was their best absorber. However, the absorption process itself was poorly understood and the existing models involved large numbers of free parameters.

Manley, Goldsmith, and Schwinger explained the energy-selective absorption as a resonance capture in which a neutron and a nucleus create a virtual bound state. Quantum mechanics predicts that the cross section for such a capture by an individual nucleus is a bell-shaped (Lorentzian or Breit–Wigner) function of energy with width and height depending on the total width $\Gamma$ of the bound state, the resonance energy $E_0$, and the energy $E$,

$$\sigma(E) \propto \frac{1}{(E-E_0)^2+\Gamma^2/4}; \tag{2.4}$$

$\Gamma$, the full width of the cross section curve, is inversely related to the lifetime of the resonant state. The theory was to be tested on the known transmission curves for various thicknesses of rhodium, indium, and iridium, but the interpretation of these data was made complex by several factors such as the absorption taking place in bulk matter, the position of the resonance being affected by a Doppler shift due to recoil (which turned out to be negligible), and also because the angular distribution of resonance neutrons was unknown and had to be assumed.

A particular consequence of the resonance character of neutron absorption is the so-called 'self-reversal' of resonance lines. The energy-selective absorption process removes from the beam those neutrons whose energy is close to the resonance value, and most of them disappear from the beam within a small thickness of the absorber. A larger thickness is thus less effective in further reduction of the beam's intensity and the apparent absorption coefficient paradoxically appears as a rapidly decreasing function of the thickness of the absorber. The main achievement of Manley, Goldsmith, and Schwinger was the determination of the cross section for the capture at resonance and the width of the characteristic resonance (of rhodium) from such 'self-reversal' curves. A single resonance was sufficient to account for the activation of the 44 s half-life rhodium state, $^{104}$Rh. In doing so, they arrived at the value of the scattering cross section (to be precise, at the effective absorption coefficient, which is proportional to the cross section at the resonance) that was 20 times larger than that obtained in 1936 by Fermi and Amaldi,[32] who had not yet recognized the importance of the self-reversal effects. However, since they defied the great authority of Fermi's school, the recognition of these correct results came only slowly.

## On his own: a winter in Wisconsin

One day in 1937, Rabi had a conversation with Julian in which he said something like, 'Well, I have taught you everything I know. Why don't you go and study with other people?'[1] He suggested that Julian might go first to Madison, and work through the winter with Gregory Breit and Eugene Wigner at the University of Wisconsin. He arranged a traveling fellowship for Schwinger for one year: first to go to Wisconsin, and, maybe sometime in spring, he might go and work with J. Robert Oppenheimer's group at Berkeley. This was the Tyndall Fellowship, which Schwinger retained when he returned home to Columbia in the Fall of 1938. Before that trip, Schwinger, in the company of his college friend Joe Weinberg,* also went to Ann Arbor to attend the 1937 Summer School, which was then organized and run by Samuel Goudsmit and George Uhlenbeck at the University of Michigan. The activities at the school did not fully occupy Julian and left him enough time to learn to drive a car, courtesy of a friendly acquaintance.[1] Julian was fascinated by cars and eventually over the years even developed a strong affinity first for Cadillacs and then exotic sports cars, but for

* Weinberg recalled that Julian received graduate credit at Columbia for attending the Summer Symposium at Ann Arbor, Michigan. So Weinberg, who was not yet a graduate student, approached Uhlenbeck to request graduate credit as well. To support his petition, he showed him a manuscript he had written on weak interactions. Uhlenbeck glanced at it, said it was 'impossible' because it violated parity—after all, Michigan was the home of Laporte, of Laporte's Rule fame—and unceremoniously discarded the paper in the wastebasket. Julian stayed in the $\Delta\Upsilon$ house with the lecturers, for example, Fermi and Uhlenbeck,[33] while Weinberg, feeling a less exalted status, stayed in the graduate dormitory.

a while he had no opportunity to put this new interest into practice. Until he reached Berkeley he could not afford an automobile; before that, living in New York, he could comfortably get by without one.

Schwinger went to Madison for the fall semester and then stayed on there through the entire severe Wisconsin winter. As Van Vleck telegraphically noted later, 'Columbia is to be felicitated in giving Schwinger a traveling fellowship to Wisconsin in 1937 so that he could get a good education right after his doctorate [sic]. This was the golden year in theoretical physics in Madison with Schwinger, Wigner, and Breit all on campus at the same time.'[34] He had never before lived alone nor had to fend for himself for that long a period of time; he had always lived at home with only occasional excursions. In Madison, he settled in a small room in a boarding house which Gregory Breit found for him. He had arrived in Madison equipped with a trunk full of clothes and basic necessities which his concerned mother had chosen and packed for him. He still depended on his family for all his daily needs so completely that when the frigid winter weather set in he was freezing in his autumn clothes and suffered unnecessarily, totally unaware that there was a nice, warm winter coat waiting for him at the bottom of his only partially unpacked trunk.[1]

For Julian, the encounter with his new energetic hosts did not turn out to be as fertile as his interactions with Rabi. He had arrived in Madison with a specific research project in mind. 'During the fall of 1937 and all through 1938, I was thinking about tensor forces. I was certainly working on a field theory because the inspiration for the consideration of tensor forces came from field theory. I recall a paper written in 1937 by a fairly well known, but not famous, person who worked out a theory of spin-one particles. It could have been Nicholas Kemmer [certainly Schwinger had in mind Kemmer's articles on the "Nature of the nuclear field" and "Charge dependence of nuclear forces"[35]]; he wrote a paper in which he worked out his spin-one theory. So I read that paper and noticed the spin–orbit tensor forces, which I thought was very interesting. Why don't I incorporate them into the theory? I was a nuclear physicist fundamentally at that time, so I said to myself. "Why don't I see what effect the tensor forces have on nuclear physics?" '[1]

Thus Schwinger decided to try to incorporate non-central tensor and spin–orbit forces and see what their effect might be on the nuclear bound states and nucleon scattering. He found the atmosphere at the University of Wisconsin very pleasant, largely because he needed a temporary respite from analyzing data for experimental groups. Few people knew him at Madison and nobody expected anything in particular from him. Of course, it was anticipated that he would join his hosts in their research in some way. At that time, Breit and Wigner were completely absorbed in their work on the resonances in cross sections for the absorption of neutrons by nuclei. It was only a year since they had explained the shape of these resonances in cross sections by the famous Breit–Wigner formula (2.4).[36] Even though not particularly fascinated with the problems they were working on, after his experience with Manley and Goldsmith, Schwinger felt very confident in this area and was willing to join in. Unfortunately, to his horror, he found that the style of collaboration between Breit and Wigner relied on constant interaction, discussions, and excited conversations; in his shyness he perceived all this as 'constant giggling,'[1] which did not sit well with his own more private and concentrated method of working. He decided that he could not commit himself to their rules of engagement and, for fear of being controlled and pressured, he began to avoid encountering them. This was not at all difficult since both Breit and Wigner were day persons, while Julian worked best at night. He had already developed his favorite technique of avoiding unwanted interruptions by working late at night. Now Schwinger was free, with no obligations of student life, courses, examinations, and what at Columbia had merely been a preference in Wisconsin became a norm—he became a completely nocturnal person. He studied and worked in his room until dawn, then slept long, and did not interact with anybody until late afternoon. He still met some interesting people and learned from them a few things which broadened his horizons. The main influences upon him at that time were not Breit and Wigner but Julian Knipp, a theorist who later turned up at Purdue, and Robert Sachs, a young man just one year senior to him, and with whom Schwinger developed a lasting friendship. The two also collaborated in writing a joint paper on the magnetic moments of light nuclei immediately before the entry of the United States in World War II [32, 36].

Rabi later amusingly summarized Schwinger's year in Wisconsin. 'I thought that he had about had everything in Columbia that we could offer—by we, as theoretical physics is concerned, [I mean] me. So I got him this fellowship to go to Wisconsin, with the general idea that there were Breit and Wigner and they could carry on. It was a disastrous idea in one respect, because, before then, Julian was a regular guy. Present in the daytime. So I'd ask Julian (I'd see him from time to time) "How are you doing?" "Oh, fine, fine." "Getting anything out of Breit and Wigner?" "Oh yes, they're very good, very good." I asked them. They said, "We never see him." And this is my own theory—I've never checked it with Julian—that—there's one thing about Julian you all know—I think he's an even more quiet man than Dirac. He is not a fighter in any way. And I imagine his ideas and Wigner's and Breit's or their personalities did not agree. I don't fault him for this, but he's such a gentle soul, he avoided the battle by working at night. He got this idea of working nights—it's pure theory, it has nothing to do with the truth.'[6] But the theory seems validated.[1, 37]

Breit and Wigner let Rabi know that their contacts with Schwinger were minimal, although they could see that he was doing fine on his own. Schwinger was virtually invisible most of the time, but he gave up his plans to go to California, studied eagerly, showed up regularly at weekly seminars and himself gave four talks.[13] His first seminar, in October, was on neutron scattering in ortho- and parahydrogen; then in winter, he spoke twice on the magnetic scattering of electrons. Before returning home in May 1938, he gave one more talk, this time on deuteron reactions. Schwinger did not publish anything major during his stay at Madison. He studied field theory and made progress on several projects in nuclear physics, which he completed later on (sometimes with the help of others if extensive numerical computations were required).

Joseph Weinberg, it turned out, was also at Wisconsin that year, now as a graduate student. He was very unhappy working with Breit. Weinberg seldom saw Julian, although they occasionally double-dated, and recalled that Julian favored short girls, his own height. He noted that Julian was beginning to get interested in music, a passion of Weinberg, but exclusively in Mozart. Julian's interests were narrow, with no interest in history, or literature, or even biology.* He thought the work that Julian was doing in Madison on the deuteron was uninspiring. In any case, Julian was very reluctant to discuss what he was doing.[33]

Schwinger later recalled his work at Wisconsin leading to the prediction of a quadrupole moment for the deuteron, an outgrowth of his study of tensor forces. 'Well, I wasn't exactly inactive then. I was reading the literature. And there was a paper written by Kemmer which was on the then very primitive theory of the mesotron, explaining, looking into the kind of nuclear forces that would come out of that theory. This was in 1937. And among those forces was one that's quite familiar electrically, such as the force between two magnets, which depends on angles, and so I looked at this and I said, that's kind of interesting, nobody's thought about this in nuclear physics. What would it do? So I began in '37, kept on in '38, applied it to neutron–proton scattering, gradually got around to saying what would it do to the ground state of the deuteron and of course what it would do was produce a quadrupole moment. Now I came back to Columbia working on this, totally unaware that meanwhile at the same time they were busy discovering the quadrupole moment. So here in Columbia, independently, the theory, ready to receive the experiment, and experimental facts, and it all fitted together. In other words, things were just exploding.'[37] He presented his prediction of the quadrupole moment of the deuteron in a talk at the November 1938 meeting of the American Physical Society meeting in Chicago [13]. Rabi and Ramsey had already experimentally discovered that quadrupole moment, but let Schwinger present his result first. In an historic roundtable at which both Rabi and Ramsey were participants, Schwinger later

* In Berkeley he later asked Weinberg 'why Oppy was interested in so many things.'

stated, 'I went to give a paper at the November 1938 meeting in Chicago—the Physical Society—which was generally about the so-called tensor forces and I remember you came to me and said, are you going to talk about the quadrupole moment? I looked at you surprised. I didn't think you knew—and I said, yes, and then you didn't say anything, you walked away, and I didn't until later appreciate that in a way you were letting me scoop you—I didn't—because nobody paid any attention to it.'[37],*

The only paper bearing his Wisconsin address, and one in which he duly acknowledged his 'deep gratitude to Professors Breit and Wigner for the benefit of stimulating conversations on this and other subjects,' was a letter to the *Physical Review* 'On the spin of the neutron' [9]. This short paper contained the first quantitative analysis of the scattering data to support the hypothesis of neutrons being spin one-half particles. Previously this proposition could be supported only by arguments of simplicity because all data appeared to be equally consistent with the value of spin being one- or three-halves. This letter was an extension of Schwinger's earlier work on ortho- and parahydrogen [8] and followed it closely in all technical aspects. He recalculated the cross section for a transition between ortho- and para- states of molecular hydrogen assuming spin-$\frac{3}{2}$ neutrons. Such high-spin neutrons would produce quintet rather than singlet excited states with protons; the algebra of spin states would be different and would result in a different value for the ratio of cross sections, $\sigma_{\text{ortho}}/\sigma_{\text{para}}$, than in the case of spin-$\frac{1}{2}$ neutrons. The calculated value, of order unity, was in such discord with reality that it removed any doubts one might still have about the spin of the neutron.

## The final year in graduate school

Julian Schwinger left Wisconsin and gladly returned home in the spring of 1938. The trip to Berkeley was postponed until after graduation and he could look forward to another year of complete freedom from outside pressure or obligations. Undistracted, he studied intensively and pursued a variety of fields and topics. He considered himself first and foremost a 'quantum mechanician' who completely devoured the works of Heisenberg, Pauli, and Dirac, all of whom he revered as gods and with whose creations he intellectually identified himself. He also developed a working interest in thermodynamics; the kinetic

* If Schwinger had remained at Columbia during the winter of 1937–38, he might have known of the discovery of the quadrupole moment of the deuteron earlier. But since Schwinger was back in Columbia during the fall and winter of 1938, it is surprising he did not receive a hint of the experimental result.[8] However, in the abstract for the November 1938 meeting Rabi's group only claimed an anomaly. The existence of a quadrupole moment was only asserted in print in early 1939.[38]

theory drew him into the study of relaxation phenomena of molecules and eventually to the propagation of sound and acoustic dispersion in gases.[1]

Schwinger pursued such interests only as sidelines of his main projects in nuclear physics. He always maintained a keen interest in experimental work and enjoyed the diversity of work on several concurrent projects. Rabi was stunned by Schwinger's surge of energy and was glad to see that after a year in seclusion at Madison, spent on purely theoretical studies, he again engaged himself in experimental collaboration. First of all, the article with Manley and Goldsmith on the width of nuclear energy levels had to be written up [10]. The situation was somewhat complicated since the attempts to apply the method to another isotope, $^{115}$In, were frustrated by inconclusive data [12], and Manley in the meantime had gone to the University of Illinois in Urbana. Goldsmith teamed up with Victor Cohen, another denizen of Columbia laboratories, who also worked on nuclear magnetic moments.

At the end of the decade of the 1930s, one of the most interesting experimental challenges was to devise techniques for measuring the magnitudes of neutron–proton scattering cross sections. The first attempts were undertaken as early as 1936 by Enrico Fermi. Fermi was the inventor of many practical methods which made it possible to analyze complex nuclear scattering data. The difficulty in measuring the scattering of neutrons off protons was that the latter were bound in a material such as paraffin. Fermi pointed out that in typical experiments with slow neutrons on paraffin targets, hydrogen nuclei in paraffin in general could not be treated as free protons unless neutrons have energies above the ground state vibrational level of the paraffin molecule.[39] This energy level is about 0.3 eV, which is roughly ten times the energy of thermal neutrons. Although it was possible to estimate the effects of binding, the results involved a high degree of uncertainty due to subtle factors, such as the effects of imperfect geometry of the beam and counters, the thickness of the scatterer, and the scattering of neutrons by carbon nuclei in paraffin. With still quite rudimentary experimental methods, which further added to the uncertainty, it was easier to use higher neutron energies, of the order of at least a few electronvolts. Although in this range of energies it was difficult to generate neutrons sufficiently homogeneous in energy and design selective detectors, the total cross section could be easily determined from the exponential drop of intensity of the beam as a function of the thickness of the scatterer.

Cohen, Goldsmith, and Schwinger achieved the equivalent of a monoenergetic source by utilizing the resonance levels for neutron absorption in the energy range of between one and ten electronvolts. Rhodium plates surrounded by cadmium that removed background thermal neutrons served both as absorbing filters and energy-selective detectors. Irradiated with neutrons, the detectors emitted secondary ionizing radiation which was subsequently measured with proportional Geiger–Müller counters. In this particular experiment, Schwinger's role was more than just providing theoretical support: he was truly dominant in all aspects of the work. The original suggestion to do the experiment came from him, and he designed it and fully participated in taking the data.[13] The experimental procedure was not very sophisticated; it consisted of essentially irradiating samples with neutrons from a radon–beryllium source and measuring secondary radiation, but it required much leg work since the experimenters did not have a laboratory and equipment of their own. The Geiger counters were located on the other side of the building from the neutron sources and rapidly decaying samples had to be rushed back and forth across the Pupin Laboratory building. (The separation was presumably necessary to avoid background radiation.) Most of this running took place from late evening until the middle of the night. Later, Schwinger's co-workers would retire but he, after taking a hearty meal, would return to his desk for several hours of quiet work and study. Hamermesh recalled that at the time 'I would work up at NYU or City College, come to Columbia around three o'clock, start doing calculations. Julian would appear sometime between four and six and we would have a meal which was my dinner and his breakfast, and then we would begin the evening's work, which was a strange combination of theoretical and experimental work. We were experimenters, if you can call us that. That is, we were capable of putting foils in front of a radon beryllium source and measuring transmissions through them and activations, like grabbing the foils, running down the hall of Pupin—it was on the top floor—running like crazy, putting the foil on a counter, and taking a reading. And then we would run back, put them up again, and start doing theoretical work. And we would work rather strange hours. It seemed to me that we would work usually to something like midnight or one a.m., and then go out and have a bite to eat. This would mean two or three hours during which I would get educated on some new subject. I learned group theory from Julian, and I must admit I forgot it all immediately, but as I recall, I had all of Wigner's book given to me, plus a lot more at the time and this was a regular process we went through and I think this must have gone on for a year or so and we started doing calculations of ortho-paradeuterium and on ortho-parahydrogen, scattering of neutrons, and this involved just an unbelievable amount of computation.'[12]

Feld was also involved in this experimental work. 'Probably when Morty and the other people working with him at Columbia had gotten pretty tired of running up and down the hall with the foils, I was recruited, as a sophomore then, to do the running. I guess Morty doesn't remember but I spent six months at Columbia doing the sprinting. I was a pretty good sprinter. I didn't know anything else but they were studying resonances in rhodium—I've forgotten what the mean life is now, but it's really very short [44 s]. You had to take these foils and sprint the 40 yards from the irradiation to the Geiger counter and I was the fastest sprinter they could find. I was a real good sprinter then, so I made out real well. As a result of that I not only got to hang around at Columbia at night but even when they went up to see Julian to consult on the theory or when something had gone wrong with the experiment or they got bored and just went up to talk with Julian, I was allowed to go with them, and so I got to listen.'[4] When all the measurements were done, Schwinger computed the proton–neutron cross section and obtained a magnitude of 20 barns, substantially larger than the then accepted value of 13 barns calculated earlier by Fermi and given in the Bethe 'bible.'[14] This result 'remained valid over the years'[29] and was cited as a benchmark value well into the 1950s [11]. The increase in the cross section affected the singlet neutron–proton interaction Schwinger had calculated with Teller [8]. The experiment was a diversion for Schwinger from several other undertakings, directed mostly towards a better understanding of the character of nuclear forces.

The contemporary theories of the neutron–proton interactions were based on the Schrödinger equation for a two-particle wavefunction depending on the coordinates and spins of participating nucleons. The potential energy was a function of the distance between the nucleons; in addition, there was an exchange operator, the action of which interchanged the variables within the wavefunction. There were four types of such operators, including the case of no exchange at all, known as the Wigner force. Another possibility was an operator that exchanged only the spins of the interacting nucleons, known as the Bartlett force. The Heisenberg force exchanged both spin and coordinate variables, and the fourth type of interaction, known as the Majorana force, took place by the exchange of coordinates alone. No single such exchange process was able to describe all the properties of the neutron–proton interaction. For example, under the coordinate-switching Majorana operator all eigenstates of odd angular momentum quantum number changed their signs, making the interaction angular-momentum dependent. On the other hand, the sign of the Bartlett potential alternates with increasing values of the total spin. These changes of sign make the force oscillate between being repulsive and attractive, which was against experimental evidence. Therefore it was believed that the interaction involved a mixture of all kinds of exchanges with the Heisenberg or Bartlett forces contributing to about one-quarter and the Wigner or Majorana forces to three-quarters of the total interaction potential.[40]

In order to find more about these forces, Schwinger decided to turn to more advanced applications of the interaction of neutrons on light nuclei. He still continued his friendship and collaboration with Lloyd Motz, and together they started to work on the interaction of thermal neutrons on deuterons. A letter and a *Physical Review* article [16, 17] appeared sometime later, in 1940, and was completed by an exchange of correspondence, for by then Schwinger had left Columbia for Berkeley. This work was interesting in certain respects: it again demonstrated that Schwinger had achieved maturity in handling extensive, complex calculations. It was mostly a computational piece of work, conceptually straightforward but very complex in execution. The point of departure was the interaction potential of the most general form which involved both position and spin exchange operators, assuming equal forces between both kinds of nucleons. The inclusion of polarizations would have been exceedingly difficult and cumbersome, so Schwinger and Motz decided to neglect them; they also replaced the deuteron's exact ground state wavefunction by a superposition of two Gaussian functions whose height and width were determined from graphical fitting. Despite these simplifications, the calculation was still a complex quantum three-body problem, the handling of which required considerable technical virtuosity. The challenge lay mostly in the ingenious mixing of approximations based on physical intuition with the mathematical methods of solving integral equations, so that the problem could be simplified enough to be reduced to a system of 20 linear algebraic equations solvable with the help of mechanical crank calculators. (They thank a Jerome Rothenstein for help on the numerical work.) Schwinger and Motz had access to the recent, still unpublished, accurate results of Dunning and his Columbia student Carroll. After comparing them with their own calculations they had no doubt that they agreed best with the mixture of the coordinate-exchanging Heisenberg and Majorana forces, without any admixture of Bartlett or Wigner interactions.

Schwinger also worked on similar subjects with Morton Hamermesh, his good friend with whom he had studied together and occasionally played chess in the past. As we have noted, they had also interspersed experimental work with their theoretical calculations. Together they generalized the Schwinger–Teller theory of scattering by ortho- and parahydrogen to the more complex case of deuterium and to a wider range of neutron energies. It was good phenomenological work, aimed at finding the cross sections for transitions from the ground state to other low-level states of ortho- and paradeuterium, which in conjunction with experiment could be useful for determination of the spin dependence of the nuclear force. This research was completed in 1939, and presented at the APS meeting at Columbia in February [14], but Schwinger's work on new projects delayed the publication of the detailed article. Hamermesh describes the agony of writing this paper vividly. 'Well, this work went on for a while and we got all these computations done, except that this was a period, as I recall it was around 1938, beginning of 1939, and I think Julian was getting ready to go off to Berkeley, and the paper was done and we were going to write it up and I looked upon this as my magnum opus. You know, I was going to be doing a thesis with Halpern, but who cared about that. This was really great stuff. Then we started to write the paper.* The only trouble is that at this time Julian was already very much interested in the tensor forces and I remember very well helping him with some calculation involving the coupled differential equations that you get; [moreover,] I was a great reader of the literature and I was always telling him about interesting problems and unfortunately one day I mentioned the absorption of sound in gases and that started him off on an enormous amount of work which I don't think he ever published, as far as I can tell. But he did all sorts of calculations on this and there I was, trying to get him to write a paper and he's a rather finicky writer—maybe he isn't so finicky any more—but I can recall that there were only a few weeks before he was to leave and there was the paper and we were still in the first paragraph and every night we would start, we would write six or seven lines, and we wouldn't get it done, and here I could see the time slipping and I would go home and I would cuss hell out of him—to myself. And at one point I contemplated murdering him, but I didn't. He went off to Berkeley, paper not done.[12]

'The next time I saw Julian was at Cambridge. I came to the Harvard Radio Research Lab in '43 and Julian arrived there about the same time, at the radiation lab, and we saw each other and he said to me, well, you know, we really ought to write that paper. That's a great idea. It turned out, of course, he really had a point. He had found a very neat trick for reducing all this unbelievable amount of calculation that we had to do to what then amounted to four days of work, and so we did it all over again very, very quickly and the paper was finished in about two weeks, I think, of writing. He had improved his style by then and it was published I think in '46 [33] and another one in '47 [38]. Well, essentially what I'm trying to say is that I think I should claim that I'm Julian's first student. I believe I learned more from him than I learned from anybody else. In fact, I think he's the only one from whom I ever learned anything.'[12]

Julian's incredible productivity always made it difficult for him to find time for polishing up the details and writing papers. On this occasion, the delay was extremely long because of the war; the paper, under the title 'The scattering of slow neutrons by ortho- and paradeuterium' [33], did not appear until the end of 1945, more than six years after the original calculations had been completed. Schwinger's attitude towards writing papers, to say it mildly, was rather hesitant. He was so full of ideas that he assigned low priority to putting finishing touches to essentially completed work. He also often felt he could improve the paper if he waited a bit to come up with a better idea for doing the calculation more elegantly, which was certainly true in this case. Nothing illustrates Julian's

* Elsewhere Hamermesh recalled, 'I remember that at one point when we were trying to write up our result for publication we worked steadily for several days with little sleep. We went to a seminar of Fermi's and both fell asleep during the whole seminar.'[41]

attitude in these matters better than his work on the theory of nuclear tensor forces.

Recall that Schwinger developed the concept of tensor forces during his stay in Wisconsin. He was frustrated by the fact that the existing theory, while capable of providing reasonable agreement with the experimental data on nuclear binding energies or total cross sections, could do it only with persistent discrepancies. He hoped that the gap between the experiment and theory could be narrowed or eliminated with an admixture of yet another type of force. If, like all other fundamental forces, it were invariant under rotation and space inversion it could, in principle, be proportional to any even power of the product $(\boldsymbol{\sigma}_i \cdot \mathbf{r})$ of spin and position operators. Here $\frac{1}{2}\boldsymbol{\sigma}_i$ is the spin of the $i$th nucleon, and $\mathbf{r}$ is the relative position of the two nucleons. However, for spin one-half particles, all higher powers of this product reduce to the lowest order ones, leaving only two candidates [13, 22, 23, 24],

$$S_{12} \quad \text{and} \quad \boldsymbol{\tau}_1 \cdot \boldsymbol{\tau}_2 S_{12}, \tag{2.5}$$

where

$$S_{12} = \frac{3(\boldsymbol{\sigma}_1 \cdot \mathbf{r})(\boldsymbol{\sigma}_2 \cdot \mathbf{r})}{r^2} - \boldsymbol{\sigma}_1 \cdot \boldsymbol{\sigma}_2, \tag{2.6}$$

and where $\frac{1}{2}\boldsymbol{\tau}$ is the isospin of the nucleon, with $\tau_z = \pm 1$ for the proton or neutron, respectively. Interactions not involving $S_{12}$ were a linear combination of the conventional Majorana, Heisenberg, Wigner, and Bartlett forces described above.

Schwinger chose this particular linear combination in order to have zero spatial average over all directions in space. Despite its resemblance to the classical expression for the magnetic coupling between two magnetic dipoles of magnetic moment $\boldsymbol{\sigma}$, he expected that the strength of this new interaction must be characteristic of the other nuclear forces, submerging any corrections due to the electromagnetic spin coupling.

The introduction of the tensor force was Schwinger's first significant and truly original contribution to nuclear physics. It did not merely add yet another phenomenological term to obtain somewhat better agreement with experimental data; the tensor term had a profound effect on the symmetry properties of the distribution of nuclear matter inside neutron–proton bound states, and even changed the quantum number structure of nuclear energy levels.

Firstly, the ground state wavefunctions of two-particle bound states created by central forces are always spherically symmetric. This would preclude deuterons from having electric quadrupole moments. On the other hand, by their very nature tensor forces endow the deuteron with a non-zero quadrupole moment. In 1938, Schwinger had not yet heard about any experimental indication in support of such a claim, in spite of the ongoing experiment in Rabi's group.[38] His prediction was made without any basis of quantitative information, and he could not yet even say whether the quadrupole moment was negative or positive. No wonder he was cautious and somewhat apprehensive about announcing the new idea publicly. When he went to the Chicago meeting in November 1938, he learned, to his astonishment, that Rabi's group was just at the same time discovering the quadrupole moment by using his molecular beam techniques, as described above. Soon afterwards Rabi's group indeed measured the quadrupole moment, consistent with the distribution of charge in the shape of a spheroid prolate 14% along the direction of the deuteron's spin axis.[42]

The second major departure from established theory was that while all central forces were invariant under rotations of space and spin coordinates separately, the Hamiltonian of the tensor force was not; it required a coupled rotation of space and spin reference frames. In other words, the Hamiltonian operator was invariant only under those rotations in which the observer's point of view turned simultaneously with the space coordinates.

Therefore, with central forces alone, the operators of orbital angular momentum and spin commute with the Hamiltonian and the quantum numbers of two nuclei comprise of the values $L$ and $S$ of the angular momentum and spin, and their respective projections $m_L$ and $m_S$. Incidentally, these were the same quantum numbers as used in atomic spectroscopy, and the lives of early nuclear theorists were made easier because the language and many useful techniques of special functions developed for atomic physics were readily adaptable for new applications in nuclear physics.

Just as in atomic physics, the situation changes when spin–orbit forces are considered. With even the smallest admixture of a tensor interaction, the energy eigenstates of nuclei must be described by a different set of quantum numbers because the total angular momentum $\mathbf{J} = \mathbf{L} + \mathbf{S}$, rather than $\mathbf{L}$ or $\mathbf{S}$ separately, commutes with the Hamiltonian. Although the eigenvalues $J$ of the total angular momentum and its projection $m_J$ still remain good quantum numbers, the total spin and its projection no longer do. In the case of deuterons, the Hamiltonian is symmetric in spin variables and the corresponding wavefunction is either symmetric or antisymmetric. This makes it possible to distinguish between the singlet and triplet states from the criterion of symmetry alone, which permits the use of total spin as a quantum number in this case. However, $m_S$ is not available; in its place, the fourth variable necessary to provide a complete set of quantum numbers of a neutron–proton bound state proposed by Schwinger was parity, the eigenvalue of the space reflection operator. In consequence, the energy eigenstates were mixtures of wavefunctions corresponding to either even or odd values of $L$, since these transformed differently under reflections. One important consequence of that was that even the stable ground state of the deuteron was different; in the spectroscopic notation, it was a combination of the states $^3S_1$ and $^3D_1$, while in the absence of the tensor force it was a pure $^3S_1$ state.

In order to investigate the amount of the admixture of the tensor potential, Schwinger wanted to compute the ground state wavefunction of the deuteron, the cross sections for radiative capture of thermal neutrons, scattering of neutrons by protons, and an especially interesting process—the photodisintegration of deuterons—which would provide accurate information about the deuteron's binding energy. For this he needed precise solutions of the Schrödinger equation with the tensor potential method. Unfortunately, despite using the simple square well potential, he could not find analytical solutions even for the lowest energy states. He realized that the equations must be solved numerically by power series expansion. Schwinger had done numerical calculations before, but this time the task was overwhelming and it would take him away from fundamental research. Therefore he decided to wait and look around for someone more adept in this art than himself. He abandoned the largely finished work, made a preliminary announcement of it at the APS meeting in Chicago in November 1938 [13], as noted above, but eventually published the entire work only in 1941, sharing the credit with William Rarita, who had done the numerical calculations, while on leave (from Brooklyn College) at Berkeley. The articles became known as the famous Rarita–Schwinger papers [23, 24], which had considerable impact on the development of theoretical nuclear physics. (It is interesting to note that in [23] Schwinger again thanks Breit and Wigner for the benefit of stimulating discussions at Wisconsin, where he began the investigation. Of course, the presentation improved with the passage of time, and he thanks J. R. Oppenheimer and R. Serber as well.)

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3

# Schwinger goes to Berkeley

## Arrival in Berkeley

In the spring of 1939 Julian Schwinger was 21 years old, a fresh PhD with a sizable number of articles already published in the *Physical Review* to his credit, several of which were quoted and considered significant. The time had come to find a postdoctoral appointment, and this meant leaving home and New York City. Julian was still greatly attached to his family, and had become accustomed to taking advantage of the special privileges he had earned at Columbia; therefore he was not at all eager to leave this environment. Indeed, this was relatively the freest period of his life, virtually devoid of all responsibilities, and he devoted every minute of it to physics.

After the disappointing experience of the winter trip to Wisconsin, where his contacts with his highly energetic hosts Gregory Breit and Eugene Wigner had been almost non-existent, Julian felt that it would be better to forgo any travel opportunities for a while. With Rabi's blessing he indefinitely postponed his visit to J. Robert Oppenheimer in Berkeley. Considering Oppenheimer's strong personality and domineering attitude towards his co-workers, it was probably a wise decision to stay close to Rabi, who had already shown his superior qualities as a mentor. Rabi was also a powerful personality, but in Julian's case he exercised a different type of influence; their talents complemented each other, and in such a relationship there was no room for domination. Rabi understood Schwinger's difficulty in making a premature transition to adulthood; he did not press any advice on Julian who, outside physics, was just a normal young man with the normal torments of his age and in need of more time to develop and mature.

Before World War II, job prospects for doctoral graduates in physics were generally bleak. Postdoctoral positions were rare and many young PhDs, while waiting for an opening, taught in high school or, if they could afford it, did unpaid work in research. Many young physicists engaged in scientific projects at Columbia were working *gratis* as guests through personal association with somebody from one or another research team. Unlike them, Julian had won an almost certain right to choose any research institution of his liking; he even received very prestigious unsolicited invitations. Hendrik Kramers invited him to spend a year in Leyden, and John H. Van Vleck offered him a coveted three-year fellowship at Harvard's Society of Fellows.[1] Schwinger was not interested in either of these offers. Scientifically they would not provide him with anything better than he had already been enjoying at Columbia, and the prospect of starting an independent life was still of no interest to him. 'My father's financial situation had begun to improve. Of course, after losing his company during the Depression, he never again acquired that level of affluence, but he was a very much sought after designer of women's apparel on Seventh Avenue—which meant a certain [amount of] mass production, but he was very skilled and I'm sure he got paid very well. There was a difficult period during which everybody was broke and he had to moonlight, I think. That was difficult. He worked for one company and then another one. I can't say affluence, but at least ease had returned. My brother got his degree in business from [Columbia] University and he worked for a bank. Somewhere along the line he decided to become a lawyer. He must have gone to law school. Could it have been Fordham law school? [It was.[2]] He was not yet married,* but probably still living at home, but surely gainfully employed. But I was very happy to leave home.'[3]

By 1939 there was no point in waiting any further and the time had come to part company with Rabi. Julian decided to make a move to Berkeley. Why did he choose to go to Oppenheimer? 'Oppenheimer was the name in American theoretical physics. Where else could I have gone? I had already tried Breit and Wigner and found them wanting. So, who else was there? The only real school of theoretical physics, outside Princeton, was Oppenheimer. So it was inevitable, and I still don't know why I didn't go to Berkeley in 1937. Very strange. ... Maybe I was afraid that the second half [of the year] would be just as bad as the first half. I had this example of not being particularly attracted to the gurus of the trade.'[3]

Julian took it for granted that all major transitions of his life happened by themselves, with a kind of invisible intervention from friends or superiors, without having to worry about the mechanism. This time was no exception. Still not convinced that Oppenheimer was the best match for Schwinger, Rabi at the last minute suggested that he go to Pauli instead, but to no avail.† Julian was firm in his decision, so Rabi just advised him what he must do and how to apply for a stipend, and offered to pull some strings for him if necessary. He vigorously recommended him for a National Research Council fellowship, and from then on everything worked perfectly smoothly. Schwinger was awarded

* Harold got married during the war, while in the Navy.[2]

† As Rabi recalled: 'I thought he should go to Pauli but [Schwinger] thought Oppenheimer was a more interesting physicist, and he went there.'[4]

an adequate $1500 stipend for one year, the value of which was comparable to postdoctoral fellowships of today, and he left for California.

Schwinger arrived in Berkeley on 1 September 1939, the fateful day of the German invasion of Poland. 'I remember stopping at a fleabag hotel on Telegraph Avenue and remember getting out in the morning, looking around and seeing a mountain. I had never seen a mountain next to a town before. Oppenheimer had heard of me, and my going to him had been cleared through Rabi. In any event, I had several years of published papers already. However, the only paper that would have interested him was the paper I had not published [0]. I was in nuclear physics by and large; he was not. He was in cosmic rays and aspects of electrodynamics. I don't remember in detail how we got together. [Oppenheimer] certainly said, "Well, you have to have a good place to stay," and I believe he took me around to International House on Bancroft Way. Anyway, he got me established and then said, "I have to find a comfortable chair here because I assume we're going to do a lot of talking." '[3] This, however, was not immediately possible in the limited comforts of Julian's new quarters, so they had to continue their conversation in Oppenheimer's office.

In 1939 Schwinger could be characterized as a nuclear physicist. He had a broad background in physics, but all his accomplishments were in the field of neutron and proton interactions. Therefore he surprised Oppenheimer with the choice of his latest interest, the anomalous sonic dispersion of gases, which became the subject of his first seminar. This work emerged from a rather obscure aspect of the kinetic theory of gases—molecular relaxation phenomena—which, as we recall from Chapter 2, was inspired by an article brought to Schwinger's attention by Morton Hamermesh, who was a promiscuous reader of all scientific literature. Julian devoted some time to this subject and produced a considerable amount of calculations on it, which later shared the fate of volumes of his never published research. Oppenheimer complimented Julian on his work, but remarked: 'You are interested in very strange things!'[3] It was a compliment, but it did not mean acceptance. Oppenheimer exerted a very stimulating influence on his younger co-workers, but he often enjoyed intellectually subduing his entourage, from which he expected respect and enthusiastic affirmation of his ideas. At first it seemed that Oppenheimer and Schwinger would not be able to communicate and interact with each other productively. In Julian's eyes, 'He was overwhelming. And you may appreciate the dilemma that put me in, as was already indicated by the way I behaved in Wisconsin, in which I did not want to be overwhelmed. Oppenheimer was not only impressive, he liked to impress. He was a showman. I was impressed, no question about it. But I also resisted him. Not at the beginning perhaps, but more and more.'[3]

Schwinger would accept no idea without prior gestation, remolding and rephrasing it in his own way, and Oppenheimer initially misinterpreted this reserved attitude as an ostensible manifestation of lack of interest. He allegedly reached the point of seriously thinking about getting rid of the strange newcomer altogether and requesting the National Research Council to transfer him elsewhere. 'I have heard rumors and I do not know the basis of it, that Oppenheimer in my first month was enough disappointed with me ... that he was thinking of having me sent back to Columbia. Someone mentioned this just in passing to me. It might have been Rabi.'[3] Schwinger had a frank explanation for Oppenheimer's feelings. 'At the early stage perhaps I didn't measure up in the sense of ritual, in which everybody would come into Oppenheimer's office at some early hour of the morning and they would sit around and talk. I presume I was still a late riser and so never came to these get-togethers. Maybe he didn't like my dissident ways at first. I never heard a direct statement, but it's very plausible that I was a strange fish to begin with until he appreciated that I could produce nevertheless. So perhaps in the first month he didn't quite like the "cut of my jib." '[3] Rabi corroborated this rocky beginning. 'I spoke to Oppenheimer later and he was terribly disappointed. He came to the point of writing a letter to the National Research Council suggesting that Julian go somewhere else, because it took a man like Oppenheimer quite a bit to get used to Julian. Pauli once referred to Oppenheimer's students as being *Zunicker*. Somebody who knows enough German knows what this means—people who nod heads—and Julian was not that way—that, and his hours. However, he thought better of it and soon learned not only to accept him but to love him.'[4]

The differences were quickly mended after Oppenheimer realized that despite appearances Schwinger was learning intensely from him and that he would become a productive research partner. 'After all, I was there to learn from him, which I did do because he introduced me to areas of physics I had not actively worked on, like aspects of cosmic rays. I did not know anything about cosmic rays, really.'[3]

Schwinger blended well with the dynamic group of young theoreticians associated with Oppenheimer, who included David Bohm, Herbert C. Corben, Sidney Dancoff, Edward Gerjuoy, Phillip Morrison, William Rarita, Leonard Schiff, Robert Serber, Harland Snyder, and George Volkov. He quickly began to collaborate on several projects and, even before the first two months were over, he and Oppenheimer sent their first joint paper for publication. It was a letter to the *Physical Review* 'On pair emission in the proton bombardment of fluorine.' [15]

This letter addressed a complication which had arisen in interpreting the data from an ongoing Berkeley cyclotron experiment of Fowler and Lauritsen,[5] in which the capture of a proton by fluorine produced an unstable neon nucleus which disintegrated into oxygen via the emission of a very low-energy alpha-particle. Some time after this emission, a substantial excess of energy was released in the form of monochromatic gamma radiation. The reason why the reaction had to go through a double-step process instead of the alpha-particle taking the entire available energy was explained by the selection rules that involved angular momentum and parity conservation. What was puzzling was that sometimes, instead of the gamma radiation process, 'it produced an excited oxygen nucleus which strangely decayed into [a stable oxygen nucleus accompanied by the emission of] an electron and positron, and of course everybody was theorizing about new forces and so forth.'[3] Why this reaction appeared to be strange was because the relative frequency of its outcome indicated that the pairs must have been produced directly, not by internal conversion. On the other hand, in order to forbid long-range alpha-particle emission, the excited oxygen level was believed to be of odd parity, while the ground state must be of even parity, which would bar any such pair production process. Oppenheimer believed that electrodynamics would break down at the nuclear scale and suggested that a new kind of non-electromagnetic exchange interaction coupling of the electrons to nucleons might be responsible for this effect. He gave the problem to Leonard Schiff, who could not find a satisfactory explanation for this dilemma. 'Schiff was then Oppenheimer's assistant in Berkeley,* and the problem got handed down from one to the next. Oppenheimer was interested in this, so Schiff said, "Hey, Schwinger, why don't you look into this?" So I did. And obviously it got done in a day or so.'[3]

Typically, Schwinger took a totally conservative approach and refused to engage in any speculation about a new force before ruling out all the more natural reasons on the grounds of existing theory. 'I think I realized that it just was something in the electromagnetic interaction of the forbidden transition that could not radiate light but could proceed by vacuum polarization,'[3] that is, through the production of electron–positron pairs. The simplest explanation was to assume that the initial oxygen level is of even, not odd, parity. Then the energy could be released only by the emission of two quanta via an intermediate energy level or directly through vacuum polarization. He calculated the probability amplitudes for pair production and double-photon emission via a virtual intermediate state of oxygen. The ratio of respective emission probabilities was

* Ed Gerjuoy, who by this point was a graduate student of Oppenheimer's, recalled that when Schiff gave one of his frequent talks at Oppenheimer's seminar, Oppenheimer was very mean and caustic in his remarks to him, often leaving the gentle Schiff on the verge of tears. However, Gerjuoy realized immediately that the same would not happen to Schwinger, who became Oppenheimer's assistant the following year, because from the first Schwinger could answer all of Oppenheimer's questions until the leader was forced to lapse into silence.[6]

greater than one, and the quanta from the two-photon process would form a weak continuous spectrum which would not have been observed, thus providing a very simple explanation of the phenomenon. An experimental prediction ensued: if the excited state of oxygen is even rather than odd, then the production of high-energy alpha-particles would also be large at the energies where pair production was large. 'We should expect the resonance yield of long-range alpha-particles to be comparable with, and probably considerably greater than, the yield of pairs.' [15]

The paper on pair emission was written by Oppenheimer and, besides its physical importance in demonstrating the reality of vacuum polarization, is of historical interest as an illustration of the differences between Schwinger's and Oppenheimer's treatments of experimental information, which would create some friction if the two had to collaborate closely. Schwinger believed that no piece of data explainable by established theory should serve as the starting point for a search for departure from that theory. That would be speculation, or worse, a search of the last resort for a magical solution by a person lacking adequate mathematical skill. Therefore, when he was presented with the problem of explaining a strange energy level that decayed through electron–positron production, he first looked for a process of pair production consistent with the conventional electrodynamic interaction. Once the figures agreed, that was it. On the other hand, while writing about this result, Oppenheimer felt compelled to conclude that 'if [the predicted high yield of long-range alpha-particles] is not so, the pair emission itself would seem to provide strong evidence for non-electromagnetic coupling between electrons and heavy particles.' [15] Schwinger felt uneasy about Oppenheimer's unsupported comment about other possible explanations of this effect, and complained: 'He wrote that letter to the *Physical Review* incorporating whatever calculations and ideas I had but at the same time mentioning other possibilities. To me it was a purely electrodynamic process and exactly what was to be expected. On the other hand he, in the spirit of the time, was convinced that electrodynamics had broken down and so in the letter there is still a reference to the possibility of some new short-range force between electrons and protons, which I had no great stock in, but there it was.'[3]

Schwinger later recalled that the experience of this particular calculation dealing with the direct conversion of energy into a pair left him with a deep conviction that vacuum polarization was an entirely real, observable effect. 'Vacuum polarization did not occur to me [as a new phenomenon]. Out of the decaying nucleus there comes an electron–positron pair. Vacuum polarization is just a handy word meaning that there are phenomena in which electron–positron pairs are created. It is just a catchword for indicating that class of phenomena. You can't get rid of it. The phrase vacuum polarization means no more than the fact that an electron–positron combination is coupled to the electromagnetic field and it may show itself as real or virtual.'[3]

Years later, while working on the foundations of quantum electrodynamics, the understanding of this fact would give him a definite edge over Richard Feynman who chose to ignore such processes entirely in the first version of his theory. On the other hand, Schwinger felt compelled to include them in spite of all the problems with the divergent calculations of this effect. This would be an important step in his invention of mass renormalization.

Schwinger could have arrived at this discovery as early as 1939. That year Oppenheimer suggested to Sidney Dancoff to try to compute the second-order relativistic electrodynamic correction to electron scattering in the electrostatic field of a nucleus. Dancoff made a fatal, but at the same time quite understandable, mistake in omitting the contribution due to vacuum polarization in which a virtual electron–positron pair is created or annihilated with the necessary energy borrowed and then returned to the field. In such a process the pair creation appears as an effect disconnected from the scattering.* Effects like this were then difficult to visualize, but their omission violated relativistic invariance, leaving some otherwise cancelling terms intact, and Dancoff could not notice that the divergent electrodynamic corrections could be incorporated together into a united electromagnetic and mechanical mass. It is somewhat surprising that he made this error, because his paper includes a footnote in which he notes that Serber pointed out the importance of including 'the Coulomb interaction with the virtual pairs in the field of the scattering potential,' which 'results also follow directly from formulae for "polarization of the vacuum." ' But Dancoff erroneously stated that 'the conclusions drawn below are unaffected by the presence of the Coulomb interaction.'[7] After Dancoff published his results, remarkably the error went unnoticed until after the war, when in 1947 Oppenheimer asked H. W. Lewis to redo Dancoff's calculation, who found that Dancoff had omitted 'certain electrostatic transitions ... essential to the covariance of the scheme.'[8] Lewis, and Schwinger shortly thereafter [43], found a finite radiative correction, thereby providing 'a satisfactory termination to a subject that has been beset with much confusion.' [43] In a different context, Ito, Koba, and Tomonaga repeated Dancoff's error as late as November 1947 and had to rescind it two months later.[9]

It is paradoxical that although Schwinger knew Dancoff well and the two interacted socially at the time one of them was already including vacuum polarization in his calculations while the other was ignoring similar processes. History might

* For electron scattering, the relevant process omitted was one in which the positron and photon are virtual, rather than the positron and electron, but as Schwinger noted, all such processes are part of a whole.

have developed differently if the two of them had had more time to discuss their respective research interests in greater detail.

Schwinger found Oppenheimer to be dazzling and erudite. Despite some conscious dissident refusal to be influenced, during his two-year stay in Berkeley he became fascinated with Oppenheimer and even picked up certain elements of his lifestyle. Julian had always been much younger than his academic peers and had been left behind when others were undergoing the usual rituals of transition to adulthood. Now was the right time to catch up. He was on his own, earning a small salary which nonetheless seemed to be a small fortune to him, but in spite of this sudden independence he was totally inexperienced in worldly matters and needed a role model. Who could fit that role better than Oppenheimer—elegant, attractive to the opposite sex, given to driving impressive automobiles, connoisseur of good food and potent Martinis, and on top of all that an accomplished theoretician, surrounded like a saintly scholar by a circle of doting disciples?

It took no time for a real friendship to develop between these two such dissimilar characters who, to the end, remained quite different from each other; in particular, Julian's shyness and inexperience in social interactions made him feel somewhat clumsy and act accordingly. 'Oppenheimer was immensely stimulating and clever, learned. I liked his style and elegance. I responded to his taste in automobiles and women, should I say.'[3] As for food, they went out together in groups on many occasions. 'To my surprise, he would often ask me out to lunch at an elegant restaurant, which left me very embarrassed because I never knew whether I ought to pay my fair share, or what. But he would pick up the tab until one day, in trying to indicate to me that I was not really doing the right thing, he said, "Oh, by the way, I think I left my wallet at home. Could you possibly . . . ." And so that was a signal. I mean I didn't think of these outings as master and pupil being together so he gently informed me that a little reciprocity would be in order, which made me feel much better.'[3]

Although Schwinger, following Oppenheimer, was becoming a connoisseur of fine foods, at Berkeley he still remained largely a 'steak and potato' person, which he had been before. As to Oppenheimer's tastes in food, those rather gourmet habits of Mexican food and chili and so on, 'it took me quite a while to respond to that. I'm not sure that my delight with Mexican and Southwest food goes directly back to Oppenheimer, but it certainly was the beginning.'[3] He had not tasted Mexican food in New York, 'and it took me a while to get accustomed to the hotness of it. The Martinis were sort of an eye-opener followed by an eye-closer, but I did my best to keep up. It was a different style of life and I appreciated it.'[3]

Throughout his life, Schwinger was known as a man with an uncompromising taste for fast and spectacular automobiles (his last car was a brilliant red

Maserati). His fondness for cars certainly began in Berkeley where the very first thing he decided was to go first class and bought 'a red LaSalle, a charming car, just a notch below Cadillac. It was a similar car. I'm sure that Oppenheimer's interest in Cadillacs had its effect on me when I realized that this LaSalle was available. It was, of course, a used car. My income did not allow me to buy new cars, but it was in very good shape. I enjoyed it; it was a marvelous way to begin.'[3] Julian must have been infatuated with this car because he kept it even after, three years later, he had saved enough to acquire a Cadillac of his own.

Schwinger thrived in the stimulating atmosphere created by Oppenheimer and got along very well with the lively circle of young theorists surrounding him. Never before or later in life did he blend and fuse so tightly with any other group of people. Although he did not abandon his habit of working in seclusion and very late into the night, causing Oppenheimer to remark in a jokingly sarcastic manner that 'his wave function does not overlap' with others, in this case Corben,[1] he undertook intensive and productive collaborative projects with Oppenheimer, Gerjuoy, Corben, and Rarita. He would also at least once renounce his routine to take part in an automobile trek, sharing a ride with Oppenheimer, to go to Pasadena. These trips were institutionalized by Oppenheimer who wanted to cultivate contacts with the rest of the West Coast physics research community and arranged regular work-and-some-pleasure trips by several cars to Caltech. In such transitions from Berkeley to Caltech and back Schwinger went with Oppenheimer. 'We certainly left and arrived together. I don't think we always traveled in the same car.'[3]

Schwinger was quickly consolidating his already considerable confidence in himself. In his career he never made a major mistake or misjudgment that he would have to rectify.* He trusted his opinions and calculations and was even becoming confrontational in the face of experimental evidence that did not agree with his predictions. During 1940, Luis W. Alvarez and Kenneth S. Pitzer at the Radiation Laboratory in Berkeley conducted a carefully prepared experiment, involving 50 000 counts, on the scattering of slow neutrons with the thermal energy corresponding to a temperature of 20.4 K on ortho- and parahydrogen.[10] They expected to obtain more precise values of the cross sections than were already available, but instead obtained magnitudes that were substantially different from what Schwinger thought to be true based on his earlier theoretical work. [8] He corrected them by pointing out that they had not properly taken into account the effects of the thermal motion of molecules in the gaseous target they had used. But still the inferred

* A possible exception was his initially incorrect, but unpublished, first relativistic Lamb shift calculation which we will describe in Chapter 8. His foray into cold fusion, late in life, was perhaps a misjudgement, but not a technical mistake.

experimental cross section for the scattering of neutrons on protons was 15% lower than the directly measured value of 20 barns. [11] 'The whole idea of that scattering experiment was to measure the scattering length of neutron–proton scattering in the triplet and singlet states. Previously only the orders of magnitudes were available. There was a quantitative experiment coming on line and, as a matter of fact, I got Alvarez very angry at me because I thought that the results he got were quite improbable in the light of what one anticipated and I suggested that the experiments be repeated. I don't know if they ever were, but he was quite angry. Instead of being appreciative of the experiments, I suggested that there must be a flaw somewhere, as the theorist always does, and I must have been arrogant about such things, but I think I was right.'[3]

We recall that when Schwinger left for Berkeley, Morton Hamermesh was quite upset because their joint paper on neutron scattering by ortho- and parahydrogen and deuterium was left unfinished, only to appear a decade later [33, 38]. Only a brief abstract appeared in the *Physical Review* [14]. But the Alvarez–Pitzer experiment provoked Schwinger to retrieve the research notes on his work from Hamermesh. 'Suddenly there comes a telegram, please send all the calculations, and I packed up a pile of stuff about this high, and shipped it off, heard nothing till suddenly some letters appeared in the *Physical Review.* There were some experiments by Alvarez and Pitzer,[10] and a short note by Julian [20] with these calculations. I just gave up on him and did a thesis quick.'[11] In fact, Schwinger modestly acknowledged Hamermesh's contributions: 'The cross section curves necessary for the evaluation ... have been computed by Schwinger and Teller [8], with extensions and improvements by Hamermesh [14].'

Schwinger wrote a letter to the *Physical Review* [20], explaining his concern and the two letters of the disagreeing parties were published side by side. Schwinger's letter ended with the words: 'The consequences of these ortho–para measurements are in such variance with present theoretical concepts that it would be highly desirable to repeat these measurements and search for systematic errors.' Alvarez and Pitzer declined to do that and politely stated that they had other priorities: 'The theoretical implications of these data will be discussed in a companion note by Dr. Schwinger. We had planned to repeat the work, to improve the statistical accuracy and to search for possible systematic errors, but pressure of other work now makes that impossible for some time.'[10]

## Mesotrons

Schwinger had arrived to work with Oppenheimer two years after what was believed to be the hard experimental evidence for the existence of Yukawa particles,[12] then still called 'mesotrons,' had been established.[13] Oppenheimer and Serber were the first physicists in the Western hemisphere who published on the Yukawa $U$-field, although not as proponents of his idea. They published some strongly worded criticism of Yukawa's theory, pointing out that it was effectively equivalent to Heisenberg's exchange model,[14, 15] but by the time Schwinger joined Oppenheimer, the latter had already changed his mind. Everybody in Berkeley was talking about mesons, and for the second time in his young life Schwinger had the good fortune to find himself at the right place at the right time.

The essence of Yukawa's new ideas was his explanation of the nuclear force through the exchange of mesons, which in relativistic quantum mechanics led to the concept of mesonic fields. The original Yukawa scalar field had already, in the minds of many, been abandoned in favor of a vector field quantized in a manner similar to electrodynamics. Being massive and charged, it also possessed longitudinal degrees of freedom and was complex. These were unwelcome complications at a time when the methods of quantum electrodynamics were crude and the divergence problem was still unresolved. Still, in a very limited class of problems it was possible to obtain certain quantitative predictions in the lowest order of perturbation theory without totally renouncing the requirements of rigor. Schwinger was familiar with the existing literature on the subject, including the still generally unnoticed publications (in the USA) of the Bristol group of refugee physicists which included Herbert Fröhlich, Walter Heitler, and Nicholas Kemmer. It is remarkable that some of this work was devoted to the explanation—on the grounds of nuclear theory—of two effects that would play a very special role in Schwinger's discoveries in quantum electrodynamics. One was an incorrect explanation of a small anomaly in the fine structure of hydrogen, now known as the Lamb shift, then freshly discovered experimentally by R. C. Williams.[16] Fröhlich, Heitler, and Kahn hypothesized that this effect could be the result of a long-distance remnant of a short-range force associated with the virtual emission of a meson by a proton in the hydrogen nucleus.[17] Earlier the group of Fröhlich *et al.* had suggested that this effect might also affect the magnetic moments of a proton and a neutron. They calculated the self-energy correction to the nucleon energy due to the virtual emission of a vector meson. It diverged, but in an external magnetic field the divergent self-energy could be expanded in a power series in the field strength, and the coefficient of the term linear in the field turned out to be finite. Fröhlich, Heitler, and Kemmer risked the interpretation that it represented an anomalous correction to the magnetic moment.[18]

Schwinger was familiar with the developments in meson theory, but he did not trust them. He felt uncomfortable with 'subtraction physics' and was always reluctant to use any procedures that he could not fully understand. Therefore he chose to concentrate on what he knew how to calculate rigorously with the tested methods of quantum mechanics. He returned to the old project on which he had worked at the University of Wisconsin and during his last year at Columbia, which was the inclusion of the tensor component in the potential of the nuclear force. He was prompted to return to this unfinished task by the publication in England of new results with which he did not fully agree.

In 1940, Fröhlich, Heitler, and Kahn published an article[19] in which they applied the new meson theory to photoelectric nuclear processes. In particular, the deuteron photodisintegration experiments contained a wealth of useful clues about the nature of the neutron–proton interaction. Heitler, Fröhlich, and Kahn concluded that the exchange currents related to the strong tensor coupling, derived from the vector-meson model, were predominantly responsible for the emission of hard gamma radiation. In this particular application, they managed to arrive at finite probability amplitudes by circumventing the divergence problems arising from the short-range singularity of the tensor potential of the form $1/r^3$ which was inherent in all single-meson theories. They did not do it in an entirely consistent manner, including the tensor forces in some while ignoring them in other parts of their calculations. The value of the photoproduction cross section found by Fröhlich, Heitler, and Kahn was large and Schwinger suspected that this was a concealed but straightforward consequence of that very singularity and later commented that 'although this perturbation calculation gives convergent results, it is obviously a dubious procedure to include singularities which, in other aspects of the theory, imply infinities only lent significance by arbitrary methods.' [22] Therefore he finally decided to deploy his own techniques for tensor forces, which had been awaiting practical applications.

## Collaboration with William Rarita

The methods used by the Bristol group had evolved from the work of Nicholas Kemmer.[20] The exact nature of the meson field was still unknown, but there existed two possible candidates to understand it: the spinless Yukawa and the spin-1 Proca fields. The interaction part of the Lagrangian density was supposed to be made up as a product of the quantized meson field and two Dirac spinors representing proton and neutron fields. Since the Lagrangian density is a relativistic scalar, the product necessarily had to be constructed in a relativistically covariant manner from the Dirac matrices in such a way that the entire expression transformed as a scalar. The algebra of Dirac matrices produces five such linearly independent products, none of which could be arbitrarily excluded. Therefore, in principle, one could have five different meson theories, with meson fields transforming under the Lorentz and reflection groups as scalars, pseudoscalars, vectors, pseudovectors, or a tensor field constructed from a vector. The actual interaction Lagrangian was believed to be a combination of several kinds of such terms and their respective coupling constants had to be determined empirically from the scattering data.

However, because of the ever-present divergences, the calculations of the scattering cross sections could only be conducted in the lowest order of approximation, including only the basic processes that involved the virtual exchange of a meson between a proton and a neutron, or the virtual creation and disintegration of a meson by a pair of nucleons. In this lowest order of approximation, the general form of solutions was essentially determined by the conservation laws and the algebraic form of the multiplier terms introduced in the interaction to make it a scalar quantity. Very similar expressions could also be derived on the grounds of conventional quantum mechanics. With a proper combination of tensor and other types of nuclear potentials, involving the operators for the exchange of positions and spins of the neutron and proton, one could emulate the position and spin dependence of the leading order perturbative results characteristic of all kinds of postulated meson theories, be they scalar, vector, charged, neutral, or mixed. The only important difference was that in the quantum-mechanical approach the results depended on the overall volume of the potential well, but were relatively insensitive to the actual radial dependence of the interaction potential. Therefore the potential could be almost arbitrary. On the other hand, in vector meson theory the radial dependence was predetermined by dimensional considerations and had an unavoidable and very strong $1/r^3$ singularity at the origin. Therefore Schwinger decided to compute the scattering probabilities on the basis of quantum mechanics, without explicitly engaging himself in the still murky intricacies associated with the quantization of meson fields. He was not willing to become a field theorist yet. He had tried to work on a similar problem in Wisconsin and found that, by using only the exchange potentials and central forces, it was not possible to reproduce the structure of all possible expressions obtained in field-theoretical solutions. Something was missing, and the only possible addition was a non-central force. 'I had picked up the idea of tensor forces, following it from field theory and then ignoring the field theory background.'[3]

In the exchange theories of nuclear forces the calculations were based on the standard Schrödinger equation,

$$\left(-\frac{\hbar^2}{M}\nabla^2 - E\right)\Psi(\mathbf{r}_1, \mathbf{r}_2, s_1, s_2) = J(r)O\Psi(\mathbf{r}_1, \mathbf{r}_2, s_1, s_2), \tag{3.1}$$

where $M$ is the nucleon mass ($M/2$ is the reduced mass of the neutron–proton system), and in which the interaction potential on the right-hand side is a product of $J(r)$, a function of the distance $r$ separating the nucleons, and $O$ is the exchange operator. The role of the latter is to introduce couplings between the particles in different spin (or isospin) states by switching the position or spin variables within the wavefunction $\Psi(\mathrm{r}_1, \mathrm{r}_2, \mathrm{s}_1, \mathrm{s}_2)$. The arguments $\mathrm{r}_i$ and $\mathrm{s}_i$ are the coordinates and spins of the interacting nucleons, respectively. One possible exchange operator $O$ was the product of Pauli spin matrices $\sigma_1$, $\sigma_2$, which exchanged the spins of the two interacting nucleons. Such an interaction was called the Bartlett force. The other type was the mathematically identical operator, being a product of the isospin matrices $\tau_1$, $\tau_2$, which exchanged isospins, thus effectively switching the respective positions of the proton and neutron. The latter interaction was known as the Majorana force. The operator $O$ could also be an identity operator, corresponding to no exchange, and this force was called the Wigner force. The last possibility included the product of $\sigma_1$, $\sigma_2$ and $\tau_1$, $\tau_2$, and the interaction, known as the Heisenberg force, was associated with the exchange of both spins and positions.

No single type of the above exchange potentials satisfactorily described even the basic properties of nuclear forces, and it was recognized that combinations of at least two of them were needed. However, no combination of the four central forces would produce the structure of terms obtained in the calculations of cross-sections in meson theories. The central force operators did not form a complete set invariant under rotation and inversion (simultaneous reversal of all spatial coordinates) which was needed to describe fundamental forces, because such a set also contained even powers of $\sigma_i \cdot \mathrm{r}/r$, where r is the vector distance between the particles. For spin-$\frac{1}{2}$ particles all higher even powers reduced to the quadratic term $(\sigma_1 \cdot \mathrm{r})(\sigma_2 \cdot \mathrm{r})/r^2$ or the identity. This non-central type of exchange potential was the tensor force that Schwinger had invented in Wisconsin.

With tensor forces, it was possible to emulate all kinds of probability amplitudes produced in the leading order of perturbative calculations in meson theories. For example, the combination of Wigner, Bartlett, and tensor forces led to solutions similar to those of the neutral meson theory. On the other hand, the combination of Heisenberg, Majorana, and tensor interactions would reproduce the results of charged meson theory, without encountering the difficulties with the divergences due to the singularity of the potential at $r = 0$. As Schwinger noted, 'We have avoided these difficulties by employing simplified potentials which permit exact solutions for the pertinent states of the deuteron [13] and thus allow a consistent solution of the problem. Although the choice of these simplified interactions has been guided by the current mesotron theories, we have disregarded the detailed radial dependence of the nuclear forces, obtained from these theories by a highly questionable application of perturbation theory. Two typical forms of the interaction potential are:

$$V = \frac{1}{3}(\boldsymbol{\tau}_1 \cdot \boldsymbol{\tau}_2)\{A + B(\boldsymbol{\sigma}_1 \cdot \boldsymbol{\sigma}_2) + CS_{12}\}\, J(\mathrm{r}), \tag{3.2}$$

$$V = -\left\{A' + B'\,\boldsymbol{\sigma}_1 \cdot \boldsymbol{\sigma}_2 + C'S_{12}\right\} J(\mathrm{r}), \tag{3.3}$$

$$S_{12} = 3\frac{(\boldsymbol{\sigma}_1 \cdot \mathrm{r})\,(\boldsymbol{\sigma}_2 \cdot \mathbf{r})}{r^2} - \boldsymbol{\sigma}_1 \cdot \boldsymbol{\sigma}_2, \tag{3.4}$$

written in terms of the isotopic spin operators $\boldsymbol{\tau}_1$, $\boldsymbol{\tau}_2$; the spin operators $\boldsymbol{\sigma}_1$, $\boldsymbol{\sigma}_2$; and Eqns (3.2) and (3.3) are respectively analogous to the "symmetrical" and "neutral" potential now in vogue.' [22] The tensor interactions given by Eqn (3.4) are precisely those given by Schwinger in [13], and discussed in Chapter 2—see Eqns (2.5) and (2.6).

The potentials (3.2) and (3.3) included a total of six coupling constants, but Schwinger reduced their number with the help of a phenomenological analysis. He fixed the relative magnitudes of the coupling constants $A$ and $B$ in Eqn (3.2) and $A'$ and $B'$ in Eqn (3.3) by using the relationships between the interaction strengths in the singlet and triplet states of given parity. Therefore there was only one coupling constant associated with the central force and one with the tensor force. The former constant was determined by the known neutron–proton cross section of 20 barns.

The relative strength of the coupling constant of the tensor force was determined from the magnitude of the deuteron quadrupole moment, which, in the absence of tensor interaction, would be zero. The postulate of the 'tensor force was not entirely speculative. The tensor force predicted that the deuteron had a quadrupole moment. ... I was conscious of the fact that while I had predicted the quadrupole moment, I was predicting it on the basis of no quantitative information, so I could not tell whether the quadrupole moment was positive or negative. To my astonishment, when in 1938 I went to talk* about this theoretical prediction, Rabi at the same time was experimentally discovering the quadrupole moment. [We recounted this story in the previous chapter.] Then Rabi measured it and it was positive in some nominal sense. And so one wanted to incorporate as much quantitative information as possible, particularly how the existence of the quadrupole moment would alter the magnetic moment of

* At the same APS meeting in Chicago, Schwinger also gave an experimental talk for Willis Lamb, who 'couldn't go for some reason.' It 'was about the scattering of neutrons that was the anticipation of the later Mössbauer effect.'[1] Schwinger had overlapped with Lamb in his last year at Columbia. Lamb was impressed: 'Schwinger knew Dirac's book on *The Principles of Quantum Mechanics* very well, and could solve problems on the basis of having mastered it, which I could not, and I greatly admired him for that' [Telephone interview of Willis E. Lamb, Jr. by Jagdish Mehra, 12 March 2000.]

the neutron as inferred from that of the proton and deuteron. This was the beginning of quantitative implications to be tested experimentally, so we were just looking around for what other implications were there.'[3]

Thus in 1938, Schwinger already recognized that the calculations of the problems of the bound state, partial wave scattering, and radiative capture of neutrons, were reduced to solving the standard Schrödinger equation and did not present any fundamental difficulty. In order to solve the quantum-mechanical two-body problem, Schwinger developed a technique which reduced the calculations to solving systems of simultaneous differential equations that could then be solved perturbatively by iteration. Schwinger's technique was to expand the wavefunctions into spherical harmonics and, simultaneously, in a double power series in integer powers of the distance multiplied by the logarithm of the distance variable. Once properly set up, these computations were not too sophisticated mathematically, but were extremely cumbersome. Doing them on the available mechanical calculators demanded inordinate amounts of time. Schwinger had very little time to spare, so he temporarily shelved the problem. 'I'm surprised at the very slow pace of this, but I felt no great urge to publish rapidly. It was not publish or perish in those days. In any case, I was publishing a lot anyway. But when I came to Berkeley, I came with the feeling that I wanted to do some more elaborate calculations with the more realistic models of forces and so forth. Now that came down to numerical work, and while I had done some numerical work on ancient calculators of the time, I looked around for somebody who was a little more adept at this than myself.'[3]

As we noted at the end of the last chapter, the required help came in the person of a fellow physicist from New York, William Rarita. Rarita had also been a student of Rabi's, but only for a short time. Because of personality differences their relationship had been short-lived. After an unsuccessful attempt to find a problem suitable for a doctoral dissertation, Rarita turned to Gerald Feenberg, who had just arrived from Harvard and was actively recruiting doctoral students. This relationship proved to be much more fruitful and Rarita successfully completed his thesis without any undue difficulty. He eventually obtained a hard-to-get teaching position at Brooklyn College, and taught physics there until the late 1940s.

Rarita came to Berkeley in 1940 for a one-year sabbatical visit. Upon his arrival in California he asked Oppenheimer for guidance in finding a promising research project. Rarita's interests were mainly in nuclear physics and, to his credit, he had published an article with Richard Present on proton–proton scattering in which they had independently arrived at the conclusion that central forces alone were incapable of describing accurately the experimental proton–proton scattering data.[21] Quite naturally, Oppenheimer suggested to Rarita that he should concentrate on tensor forces, and then said it might be interesting to investigate the problem of photodisintegration of the deuteron and relations between tensor forces and cross sections for nuclear reactions involving photodisintegration.[1]

Rarita began to read the literature, had some exchange of ideas with H. A. Nye, and then met Schwinger, who invited him to compare notes. Soon they were working together. In the initial stage of this collaboration, Rarita just did numerical work under Schwinger's strict guidance. 'Somebody told me that he [Rarita] was pretty good at calculating, and I went around and said, "Would you like to help me out?" and he did. . . . He became my calculating arm. I wrote the paper and told him what equations to solve and so forth, and he was very happy doing this, for in the process he learned what was going on.'[3]

Eventually, this uneven distribution of responsibilities became more level and a true friendship developed between Rarita and Schwinger, who spent long hours working together every day, although of course, in Rarita's words, 'Schwinger worked the night shift.' 'He got up about two in the afternoon and went to the seminar at 4 p.m. After dinner we talked and worked until 10 p.m., when I went to bed. He continued to work until five in the morning.'[1]

Certain outsiders initially had unfavorable impressions about the nature of this collaboration. 'Interestingly enough, [Schwinger's old friend] Joe Weinberg was there [working on his thesis with Oppenheimer]. Joe had a high sense of justice, an overly keen sense in some respects. He came to me and said, "Why are you exploiting Rarita?" Exploiting, meaning I was using him. I said, "I'm not exploiting him. His name is going to be on the paper." In fact, I said, "I'm probably making him." Which is exactly true.* But Joe Weinberg did not see it that way. He was very conscious of labor and capital and—well, you know that before the war, shall we say, very idealistic communist sympathies were widespread. That's not news of course, but he was a rather rabid person of that type. He saw my collaboration with Rarita as a class struggle, exploitation of the masses. It was rather silly, because each of the two gave what he was best at and ended up with a collaboration. Actually, it suited well the left-wing ideology, "From each according to his best!" He and I were perfectly good friends and he wasn't upset.'[3]

Indeed, with the passage of time Rarita took over an ever-increasing share of responsibility for theoretical aspects of the work. Together they started from more general aspects of the tensor interaction and properties of the bound states in which Schwinger was principally interested, and then they progressed to the process of photodisintegration, solving which had been Rarita's objective. By then, Rarita had learned enough to deliver on his own. In their first publication together [22], Rarita and Schwinger computed the cross sections for the photodisintegration of the deuteron by 17.5 MeV gamma-rays and showed that 'no significant evidence regarding the tensor interaction may be expected

* Rarita obtained a full professorship at Brooklyn College after his sabbatical.

from rough measurements of the total cross section; the large value obtained by Fröhlich, Heitler, and Kahn[19] is illusory.'

Rarita and Schwinger published altogether four papers on tensor forces, the main one of which 'On the neutron–proton interaction' [23] included the formulation of the problem and applications to the calculations of the ground state of the deuteron, neutron–proton scattering, and then photodisintegration of the deuteron. In retrospect, the article's chief message was that pseudoscalar, not vector, meson theory which included the proper admixture of non-central force appeared best to describe the observed quantities related to these phenomena. The sequence of topics in this article reflected the actual order in which the calculations had been made; however, as we have noted, its publication was preceded by a shorter version devoted purely to photodisintegration [22], possibly to acknowledge the smaller participation of H. A. Nye. A few days before Christmas 1940, Schwinger and Rarita also drove to Pasadena to give a presentation of this paper at the meeting of the Pacific Section of the American Physical Society at Caltech [21]. It was an interesting conference involving physicists and astronomers, which included an illustrated lecture on the enormity of problems encountered during the construction of the world's then largest (200 inch) telescope on Mt Palomar.

One special consequence of the existence of tensor forces was that the deuteron's ground state, which was previously regarded as a spherically symmetric singlet state of zero orbital angular momentum, now emerged as a superposition of states that included eigenfunctions of angular momentum $l = 0$ and $l = 2$ or, in spectroscopic notation, a combination of $S$ and $D$ states. In other words, it possessed a non-zero quadrupole, but no electric dipole, moment. By using Rabi's data on the electric quadrupole moment and certain helpful spin sum-rule techniques that Schwinger had developed while working with Corben on the theory of spin-1 mesons (see Appendix 1 of [24]), Rarita and Schwinger found that the $l = 2$ admixture of the deuteron is about 3.9%. Another interesting result was an estimate of the magnetic moment of the neutron, which, because of the smallness of the $D$-state admixture, was hardly different from that given by the simple difference of the moments of the deuteron and the proton, and also in agreement with experiment. However, there were significant discrepancies between the magnetic moments of the deuteron and $^6$Li, which could be accounted for by the tensor force. Most of the tensor force consequences resulted from the small $D$-state content of the deuteron, and the magnitudes of tensor force corrections to other nuclear quantities turned out to be rather small, and this in itself Schwinger found rather surprising.

The other source of information on the nuclear force came from the data on the scattering of neutrons by protons and the radiative capture of neutrons. The inclusion of the tensor force, which violates conservation of spin, complicated the calculations of scattering cross sections by a partial wave expansion. In a partial wave expansion, the incoming and outgoing wave functions are expanded in eigenfunctions of the angular momentum. The incoming waves have zero orbital angular momentum in the direction of propagation, and therefore the total angular momentum in that direction is equal to the spin quantum number, both of which are conserved quantities in the case of purely central forces. Therefore the orbital angular momentum in the propagation direction is also conserved. In the presence of non-central forces, without spin conservation, the outgoing waves of a given angular momentum become superpositions of different spin states and corresponding states of non-zero orbital angular momentum. This proliferation of possible states complicated the application of perturbation techniques, making it necessary to use the device of spin averaging. The results of Rarita's numerical calculations related to neutron–proton interactions were very encouraging. 'This demonstrates that the spin forces of the type $S_{12}$ are capable of at least a partial explanation of the experimental data.' [23] Previous calculations of the total neutron–proton total cross section, based on a purely central potential, had invariably produced magnitudes larger than the experimentally observed ones. The reduction in magnitude due to the tensor force was less than hoped for (only 2%); not enough to rule out any inconsistency but sufficient for the authors to conclude that 'it is difficult to decide whether a definite discrepancy exists.' [23] They were also able to pronounce with a similar degree of confidence the agreement with less certain data on the radiative capture of slow neutrons (indirectly confirming again Schwinger's value of 20 barns for the neutron–proton cross section), but not for the reactions of photodisintegration of the deuteron.

Schwinger made the calculation of the probabilities of the dissociation of the ground state of the deuteron induced by the absorption of soft gamma-rays. The dominant mechanism for such transitions involved electric and magnetic dipole transitions to dissociated, respectively triplet and singlet, continuum states of the deuteron. Rarita carried out the numerical calculations for the case of disintegration by a well-defined, strong 2.62 MeV line of the gamma emission from ThC″, that is, $^{208}$Tl. For this energy, satisfactory experimental data on total and forward cross sections for the emission of neutrons and protons by photodisintegration were available. The theoretical cross section was 50% larger than experiment, and the forward scattering was predicted to be much larger than observed. Comparison with the data was a clear disappointment and a lesson that quantum mechanics alone was no longer an adequate tool for nuclear physics. Schwinger, who wrote the article, made a momentous statement that 'for the first time we meet a phenomenon whose explanation apparently demands a detailed application of a field theory.' [23]

The last article, 'On the exchange properties of the neutron–proton interaction', [24] contained a thorough numerical study of the chief experimental implications of the principal types of exchange potential. This included the calculation of the total cross section and the angular distributions of scattered particles in proton–neutron collisions, and the distributions of the nucleons produced in photodisintegration. The previous paper [23] was restricted to even parity states, while here, to study the exchange nature of the neutron–proton interaction, attention shifted to states of odd parity. These states could only be studied by high-energy processes—thus the photodisintegration considered in [23] had an energy of 2.62 MeV, as opposed to 17.5 MeV here. This paper contained considerably more pedagogical details of the calculations, including technical appendices on spin sum-rules and the details of the modified perturbation technique used in the calculations. It clearly lacked Schwinger's characteristic style. 'Rarita was more involved in this now. He had learned the ropes and it was less a calculating problem than a theoretical one. So I forwarded his knowledge of the subject. I don't remember who did the actual work; he may have done most of it. I've always been happy to have collaborators along with me even if I do the major portion of the paper; it doesn't bother me.' Schwinger did the conceptual work anyway, and he liked to think that those associated with him had learned and grown through the process. 'It was another aspect of teaching.'[3]

As the summer of 1941 approached, the interests of Schwinger and Rarita began to diverge.* Rarita was emerging from the collaboration with Schwinger with a command of techniques and a greatly increased experience in phenomenological nuclear physics. He certainly wanted to go on putting his new skills to work. For Schwinger, it meant a conclusion of earlier pursuits. After two years around Oppenheimer, his interest in nuclear phenomenology was fading. Schwinger did continue teaching nuclear physics after the war, and made further research contributions, but in 1941 he was somewhat disenchanted by the

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* After Schwinger's death, Gerjuoy recalled that 'Rarita did not understand much. He was just pounding the adding machine. Julian was trying to see if using the parameters which fit the quadrupole moment of the deuteron he could consistently understand the two-particle system. It became clear it was going to work. Rarita's year was coming to an end, and Julian had lost interest in the problem. The two-particle system was understood. In fact what I started working for Julian on was to see whether the parameters would fit the three-particle system—the triton, $^3$He, and the alpha-particle for good measure. One day we came in [to find that] Rarita had been chewing on this. Rarita—short and broad—said to Julian, "Is the paper going to get written?" Julian said, "Oh, yes, don't worry about it." Rarita just got mad and he muscled Julian right up against the wall and said, "Julian, if you don't get that paper written, I'm going to kill you!" Two or three weeks later that famous paper got written.'[5]

limited conceptual challenges of a fundamental nature that it could provide and therefore he began to turn toward the more fundamental aspects of quantum field theory. In part, he was motivated by the developments in meson theory, but the influence of and friendship with Oppenheimer played a catalytic role in this transition. Oppenheimer's involvement with field theory had always been immensely serious. He had contributed to it and made an impact on the subject, and always put some of his younger associates to work on the problems of field theory. Schwinger was now entering this new phase that redefined his life and work as a physicist.

## Transition to field theory

A short letter 'On a theory of particles with half-integral spin' [25] that was submitted to the *Physical Review* in the early summer of 1941 marked Schwinger's shift from purely quantum-mechanical methods applied to nuclear physics to field theory. It was the first paper that Schwinger published in which he did not refer to a single experimental 'number.' The paper was inspired by the earlier work of Fierz and Pauli[22] on a general theory of particles of arbitrary spin which Schwinger found interesting, but in need of improvement. The short article also turned out to be a parting gift to William Rarita, whom Schwinger included as a co-author, although he did not contribute much to its creation. 'As a matter of fact I was reverting to being a field theorist [from having been a nuclear physicist]. This goes back to the work of Pauli and Fierz, which I had read somewhere and found very clumsy. And so, as a sideline of the development, I had been thinking if one couldn't find a better way of presenting it. It's not clear to me how Rarita came into this, because he did not really contribute anything to the idea; I did it myself. But he was my satellite and I was just thanking him for his friendship, something I have done several times. I could not thank him in money, so I thanked him by saying, "Why don't you do a little thing that was not important and then you're on the paper." '[3]

In the Fierz–Pauli theory, the particles of integral spin were described by tensors of corresponding rank, while the particles of fractional spin greater than one-half were spinors of appropriate multiple order. For example, wavefunctions of spin-2 bosons were tensors of the second rank, while spin-$\frac{3}{2}$ fermions were described by wavefunctions with three spinor indices. In order to avoid problems of indefinite energy, additional conditions on the fields were necessary. For a wavefunction $\psi_{\nu_1\ldots\nu_k}$ of a particle of integral spin $k$, these conditions had the form

$$\partial_\alpha \psi_{\alpha\nu_2\ldots\nu_k} = 0, \quad \psi_{\alpha\alpha\nu_3\ldots\nu_k} = 0. \tag{3.5}$$

Here the summation convention had been employed; repeated Greek indices are summed over, taking the values 0 through 3. The first condition was a direct generalization of the auxiliary condition $\partial_\alpha \psi_\alpha = 0$ imposed on vector fields in the Proca theory. In order to derive these supplementary conditions from a variational principle, Fierz and Pauli had to introduce auxiliary fields into the Lagrangian. Schwinger disliked the complications of the formalism associated with multiple-order spinors and auxiliary fields. He proposed an alternative, more elegant approach so simple that he was able to present it in a one-, compared with Fierz and Pauli's 22-page article. (However, he was later to see the virtues of the multispinor formalism—see, for example [153, 190].)

Schwinger proposed to describe higher fractional spin particles by fundamental quantities of mixed transformation properties of spinors and tensors. Fractional ($s = k + \frac{1}{2}$)-spin particles of mass $m$ would then be ordinary Dirac four-component spinors and symmetric Lorentz tensors of rank $k$. As ordinary spinors, they would satisfy the Dirac equation

$$(\gamma_\alpha \partial_\alpha + m)\psi_{\nu_1 \ldots \nu_k} = 0. \tag{3.6}$$

In Eqn (3.6) the spinor indices of Dirac gamma matrices $\gamma_\alpha$ and the wave-function $\psi_{\nu_1 \ldots \nu_k}$ are suppressed. The indices $\nu_1 \ldots \nu_k$ represent Lorentz tensor components. The positive definiteness of the energy still demanded that the conditions (3.5) are met, but there was no need to impose them. Instead, Schwinger postulated an algebraic rather than a differential condition

$$\gamma_\alpha \psi_{\alpha \nu_2 \ldots \nu_k} = 0, \tag{3.7}$$

and then the conditions (3.5) followed as a straightforward consequence of Eqn (3.6). He showed that the number of independent components is properly $2(k + 1)$.

Schwinger also proposed a Lagrangian for a free field of spin-$\frac{3}{2}$,

$$\mathcal{L} = \overline{\psi}_\mu(\gamma_\nu \partial_\nu + m)\psi_\mu - \frac{1}{3}\overline{\psi}_\mu(\gamma_\mu \partial_\nu + \gamma_\nu \partial_\mu)\psi_\nu + \frac{1}{3}\overline{\psi}_\mu \gamma_\mu(\gamma_\tau \partial_\tau - m)\gamma_\nu \psi_\nu, \tag{3.8}$$

resembling the Lagrangian of spin one-half theory, which was extremely simple compared with the artificially complex expressions in the Fierz–Pauli formulation.*

* Although not as simple as it might have been. Many years later, Stanley Deser expressed surprise[23] that Schwinger had not pointed out that this Lagrangian could have been expressed much more simply using the four-dimensional Levi–Civita symbol:

$$\mathcal{L} = \frac{i}{2}\epsilon^{\mu\nu\kappa\lambda}\overline{\psi}_\mu \gamma_\kappa \gamma_5 \partial_\lambda \psi_\nu, \tag{3.9}$$

where $\gamma_5$ is the chirality operator, $\gamma_5 = \gamma^0\gamma^1\gamma^2\gamma^3$.

Although the Rarita–Schwinger Lagrangian was not unique, it possessed great advantages when interactions were included. In the absence of the external electromagnetic field the expression for the current had the usual form,

$$j_\mu = \overline{\psi}_\nu \gamma_\mu \psi_\nu, \tag{3.10}$$

and this permitted the incorporation of the interaction with electromagnetic potentials in the ordinary way reminiscent of quantum electrodynamics. Pauli and Fierz needed as many as eight auxiliary conditions to accomplish this. Moreover, in the massless case the Lagrangian (3.8) was invariant under a gauge transformation. The Rarita–Schwinger theory was to become fashionable nearly forty years later, when supergravity necessitated the appearance of the spin-$\frac{3}{2}$ gravitino.[24]

This paper dealing with the higher fractional spin was Schwinger's first article without an explicit and immediate application to an experimental problem, but not his first paper of a predominantly field-theoretical scope. A year earlier Schwinger had published an article with Herbert C. Corben on 'The electromagnetic properties of mesotrons' [18, 19]. Corben, a fresh PhD, was an Australian who, like Schwinger, had arrived in Berkeley to work with Oppenheimer with the help of a fellowship. He was a Commonwealth Fund Fellow. Before coming to Berkeley, Corben had studied meson fields with H. J. Bhabha in Cambridge, England, and published with H. S. W. Massey on the penetration properties of charged spin-1 cosmic ray mesons passing through the atmosphere.[25] Oppenheimer recognized that Corben's and Schwinger's respective experiences complemented each other and he suggested that they start working together.

Schwinger described Corben as 'a very smart and cheerful fellow. We had no problem getting together and working and collaborating.'[3] Soon they combined their strengths, Schwinger in quantum mechanics and Corben in meson theory, and decided to investigate the interaction of spin-1 mesons with arbitrary magnetic moments with Coulomb fields. (Massey and Corben had already considered the case of the magnetic moment being unity, the Proca equation. So had Oppenheimer, Serber, and Snyder.[26]) The motivation for this particular subject was the still unresolved problem of substantial discrepancies between the observed interaction properties of cosmic rays and the values obtained from the standard field theoretic calculations which worked very well for electrons and protons. We now know that the confusion had its origin in misidentifying the abundant cosmic ray mu-mesons (or muons) as the nuclear binding 'mesotrons,' the Yukawa particles, which we now call pions. What was really happening in the upper atmosphere was the decay of pions into muons and neutrinos, and it was the (non-strongly interacting) muons that penetrated to sea level. At the time, this was still a completely unsolved mystery, and Corben and Schwinger tried to find an explanation of this discrepancy by exploring the consequences of assuming that the mesons had anomalous magnetic properties which affected their ionizing power. Therefore they decided to calculate the cross sections for the electromagnetic interactions of mesons of spin-0, 1/2, or 1, and having an arbitrary magnetic moment, with a static external Coulomb field. 'Whether that makes sense I do not know . . . . We just wanted to explore what would happen if you added a magnetic moment. It would obviously strengthen the electromagnetic interactions and shorten the penetration [length].'[3]

The preferred theory of mesotrons at the time was that they were spin-1 particles. Therefore, they considered the general form of a Lagrangian for a vector meson field $\Phi_\mu$ of mass $m$ coupled to the electromagnetic potential $A_\nu$,

$$\mathcal{L} = a^{\mu\nu}_{\sigma\tau}(\overline{D}_\mu\overline{\Phi}^\sigma)(D_\nu\Phi^\tau) + m^2\overline{\Phi}^\sigma\Phi_\sigma, \tag{3.11}$$

where $D_\nu$ is a gauge-covariant derivative, $D_\nu\Phi_\mu = (\partial_\nu + ieA_\nu)\Phi_\mu$. The exact form of the Lagrangian was then dictated only by the requirement that it is a scalar quantity, therefore it was not unique; this was reflected by the presence of a numerical tensor, $a^{\mu\nu}_{\sigma\tau}$, which could be an arbitrary combination of three possible bilinear forms that could be constructed from metric tensors. Using the freedom to define the magnitudes of these constants, Corben and Schwinger wrote the solutions of the equations of motion as a sum of two otherwise unrelated fields, $\Phi_\alpha = \Psi_\alpha + \partial_\alpha\phi$, of which one was a vector and the other a scalar. They noted that the two could transform into each other under an electromagnetic perturbation. This was pure speculation and there was no evidence for any such phenomenon, but nevertheless the proposition was very intriguing. It was still an open question whether the meson fields were scalars or vectors, and the possibility of having both fields mixed and emerging from a single Lagrangian was worth mentioning.

After deriving the expressions for the symmetric stress–energy–momentum tensor, Corben and Schwinger proceeded to find stationary solutions of the equations of motion for the field in the presence of a Coulomb field. The rather complex calculation was handled efficiently and elegantly, thanks to Schwinger's mastery of spherical harmonics and spin techniques. Unfortunately, the divergent behavior of certain wavefunctions near the origin prevented them from producing a complete set of solutions for the Proca mesons in the electrostatic field of a point charge. While this was then a common occurrence in this kind of calculation, Schwinger's first foray into meson theory ended in a mild disappointment.

Without a complete set of finite wavefunctions it was not possible to achieve any meaningful results through a perturbation expansion, and therefore Corben and Schwinger were forced to turn to the Born approximation, which also could not produce a conclusive answer regarding the values of the spin or magnetic moment. Despite this drawback, they were still able to obtain some definite results for the scattering cross sections. They concentrated on the meson–electron scattering because of its purely electromagnetic nature. The comparison of their results with the experimental data seemed to speak in favor of the theories of spin-1 mesons with the magnetic moment equal to one nuclear magneton (that is, Proca mesons) or, to a lesser degree, a theory of spin-$\frac{1}{2}$ mesons possessing the magnetic moment of an undetermined value, but other than one magneton. This work was described in a talk at the APS meeting [18] and in a *Physical Review* paper [19], having essentially the same abstract. Of course, we now know that the muon is a spin-$\frac{1}{2}$ particle with magnetic moment nearly equal to one.*

## The good days are over

After Schwinger's one-year fellowship expired, Oppenheimer made him his own assistant for another year (replacing Leonard Schiff), but no further offer was forthcoming after this extension came to an end. No particular reason for the end of the partnership had to be given, but the pattern had repeated itself again; it took Julian two years of apprenticeship to grow up and match, even surpass, his master. Since Julian could be an assistant only in title, not exactly a helper of the kind Oppenheimer needed, the time had come for him to end the tutelage, move out, and establish his own territory. The assistant's job was in the meantime offered to Schwinger's future lifelong friend Robert Sachs.[28]

Again, characteristically, the decision regarding where to go next was not Julian's. In his eyes, his career still presented itself as a chain of small miracles; only good things had happened to him before, and there was always somebody out there who was available to take care of the details. This time it was no different. Some consultation between Rabi and Oppenheimer took place, and afterwards Julian was told that a suitable opening at Purdue University existed for him, to which he agreed, and that was that. Later on he would describe the process as 'I was shipped out to Purdue.' He recalled: 'One has to look at what was happening in the summer of 1941. I have no doubt that the planning for the uranium [atomic bomb] project had begun.' Although the Manhattan Project began in Los Alamos only in April 1943, Oppenheimer was involved in it from the beginning. 'I was not privy to all that was going on, but after all there was

* Joseph Weinberg, then Oppenheimer's student at Berkeley, remembered dropping around Julian's 'digs' at Berkeley—'a magnificently appointed suite'—and while snooping around noticed three or four different versions of a typed manuscript for *Physical Review*. It was the manuscript for the paper with Corben [19]. When asked why so many versions, Julian explained that he was trying to find the 'most compact and elegant presentation.'[27] This is a striking example of Schwinger's perfectionistic style.

Oppenheimer in California, Rabi in New York, who I'm sure had his eye on the long range, and I suspect that they decided that maybe I had had enough of shall we say the coddled life and had to get out in the real world, because I don't recall how this happened. In effect I was told that "You're going to leave and we have a job for you as an instructor at Purdue University." Now why Purdue? It turned out that at that time Purdue had one of the best departments of theoretical physics in the country. There were many bright young people there and that was not a bad choice.'[3]

In the summer of 1941 Schwinger's happy stay in California was coming to an end, as was Oppenheimer's creative involvement with theoretical physics. June was a particularly busy month for Julian. Maybe sensing the impending turmoils of America joining the World War, maybe out of fear that regular faculty responsibilities would temporarily take him away from research work, Schwinger submitted one short paper and three abstracts before leaving for Purdue, all of which may be regarded as 'patent applications,' or progress reports of the unfinished work that could be interrupted by his departure to a new place and situation.

Three of these were brief communications delivered during the APS meeting that again took place at Caltech from 18 to 20 June. Coincidentally, the meeting was addressed by George B. Pegram, at that time the President of the Society, who had admitted Julian to Columbia with a scholarship while he was chairman of the physics department there. Out of the 35 communications presented at the meeting, three were Schwinger's. They were on nuclear phenomenology and gave a good sampling of what his research topics had been. In one [29], he attempted to estimate the range of nuclear forces from the value of the quadrupole moment of the deuteron based on the observation that its existence directly implied a lower limit to the range of the forces. In another [28], he discussed the stationary nucleonic states produced by a charged scalar meson field. The stated purpose of this calculation was to explain the 'anomalously large theoretical scattering of charged mesotrons by nuclear particles.' A possible mechanism was the formation of heavier states of nucleons through strong coupling of nuclei with mesons. The idea that this might be the case came from Gregor Wentzel,[29] who had envisioned states in which the nucleons became 'dressed' in clouds of mesons which produced a shielding effect that modified the strength of the effective nuclear force. Bound together, nucleons and mesons would then form atom-like stationary states of a mass somewhat larger than the known nucleons and of arbitrary charge. Wentzel's paper 'fascinated me enormously and I had begun to work on that and I discovered by doing it in my own way that Wentzel had made mistakes. I should have published the paper in which I wrote all that, but in fact I never did until it was sent to the Wentzel *Festschrift* 25 years later [28a]. So it was published too late to be of any use.'[3]

Schwinger knew that his calculation was more than a technical improvement over Wentzel's work and, although he never did fully publish the results of this research, nevertheless he made a very conscious effort to establish credit for his results. The abstract [28] was written simultaneously with the article [26] with Oppenheimer, which touched upon essentially the same subject, though it was enriched by an analysis of the implications on scattering. That paper was received by the journal on 19 June, the day after he delivered his talk. 'You know it was so easy to do it then that you got lazy about writing the full papers.' Oppenheimer's contribution to this was in part his interest in cosmic rays and 'that, after all, was the underlying stimulus. As to the quantum ideas, Oppenheimer certainly was adequate technically to deal with the semi-classical treatment of spin. . . . He was not adequate, or at least he never attempted to follow or join in, with the quantum treatment, which was more elaborate. But he was contributing, and when I told him about Wentzel making a mistake, he certainly did not question it. Well, he was trying to keep his hands in lots of different topics and it is very difficult to work intensively on all these subjects.' Schwinger wrote the paper essentially, but Oppenheimer was glad to put his name on it too.[3]

After the Pasadena meeting, Schwinger wrote a long technical letter to Oppenheimer, in which he first apologized for 'misunderstanding our writing agreement.' Apparently this had to do with the failure to complete the long article on the subject. He said 'he had worked out the quantum theory of the pseudoscalar fairly completely,' which agreed with the classical theory. He said that he had started looking at the charged pseudoscalar problem, but had not gotten very far. He then described technical conversations he had with Pauli and Weisskopf, presumably at the meeting.[30]

Schwinger began to write the sequel [28a] which he, in the first reference of the article with Oppenheimer, promised 'to be published soon,' but the plans for polishing it for publication never materialized. 'In fact, it was never finished, because I remember that when I sent it to the Wentzel *Festschrift* it ended unfinished and somebody commented at that time that it was like a manuscript with the last page torn off, and it was all rather mysterious.'[3] The Wentzel *Festschrift* article ends with a parenthetical comment: 'The 1941 manuscript stops with this equation left incomplete, although there are sketches of the rest of the argument.' At the time of the *Festschrift*, Wentzel wrote Schwinger a letter of thanks for his contribution: 'It was very gratifying to me to see, at last, your unpublished paper in 1941 which no doubt was the basis of the Oppenheimer–Schwinger note in *Phys. Rev.* [26] and presumably known to Dancoff, Serber, and Pauli in their development and generalization of the strong-coupling method. It was only through brief letters from Pauli that I heard of these developments before the correspondence between Princeton and Switzerland was stopped early in

1942. Later, starting in 1943 I felt I had to reconstruct what you had done because I needed problems for my doctoral students Coester, Houriet, Villars, Jost, and others. So you will appreciate how pleased I am to see your paper in my Festschrift.'[31]

The results of these articles helped in accommodating the puzzling paradox of the early meson theory, which was how mesons could be the agents of the extremely strong nuclear binding force, and yet deeply penetrate all kinds of absorbing media with only relatively feeble scattering effects caused by their interaction with atomic nuclei. The satisfactory explanation on the grounds of the two-meson theory, with the pi-meson being responsible for nuclear binding and the muon (with no strong interaction) abundantly present in sea-level cosmic rays, was still years away, and Schwinger and Oppenheimer [26] followed a path similar to several earlier attempts to explain this strange property.

First, in 1939 Heisenberg, working on the neutral Proca vector mesons, had discovered that a substantial part of their interaction energy might be used to increase the internal energy of a nucleon through reaction effects that converted the self-field of a nucleon into the increased inertia associated with the spin motion.[32] 'Oppenheimer became interested and was looking for a quantum way of doing whatever Heisenberg had suggested, . . . but he never got beyond the classical way of looking at it, which is what Heisenberg had developed.'[3] By 1940, the popular feeling among theoreticians had changed and the dominant belief was that some peculiar mechanism of quantum interference weakened the nuclear force in some, and strengthened it in other, physical situations. There existed theories supporting this idea as evidence. Bhabha[33] and Heitler[34] independently suggested that the weakening of the nuclear force could indeed happen in a theory of charged scalar mesons if slightly excited states of charge 2 and $-1$ existed side-by-side with the ordinary nucleons (neutrons and protons) of charge zero and one. The superposition of scattering effects on the ordinary and excited 'isobar' states would then lead to almost complete cancellation of interactions with the nuclei and near-zero scattering cross sections in the high-momentum limit. In contradistinction, in the low-momentum region the cross sections were not significantly affected, and therefore mesons could be responsible for nuclear binding inside the nucleus while at the same time still having very high penetration power in the atmosphere when arriving as components of highly energetic cosmic radiation. As we have noted, in 1940 and 1941, Wentzel[29] developed a model in which he explained the production of isobaric states of non-standard charge by a process in which the nuclei emitted or absorbed a charged meson. He used a scalar meson field; thus such emissions changed only the charge but not the spin of the nucleon emitting a meson.

Schwinger thought that Wentzel's calculation of the shielding effect of meson clouds was overly simplified on account of several avoidable assumptions which could possibly have a prejudicial effect on the character of expected solutions. First, Wentzel employed the perturbation scheme he had developed in inverse, rather than positive, powers of the coupling constant (thus it was a strong coupling rather than a weak coupling expansion); then he positioned the nucleons in a rigid, cubic periodic lattice, effectively assuming that they were infinitely heavy. He calculated the self-energy of the nucleons due to meson exchange in the limit of strong coupling. His results were finite only due to non-zero lattice spacing and finite lattice size. The correction to the self-energy of a single nucleon included a large negative constant term, a positive term due to the mass of the mesons present, and finally a small positive correction that was proportional to the square of the charge of the nucleon, implying that the isobars became increasingly heavy as their charge increased. However, Wentzel's results predicted much too large an energy gap between the isobaric states to produce the cancellations necessary to match the inferred observed value of the scattering cross section, and Schwinger expected that the discrepancy might be an artifact of Wentzel's approximations.

Therefore he decided to revisit the problem, first from the classical and then from the quantum point of view. He was able to carry out the calculations and rigorously solve the problem of a classical meson field coupled strongly to a continuous extended source, and then also produced a quantum calculation which also used an extended source rather than Wentzel's cubic lattice.* This calculation had to be approximate, and Schwinger conducted it in the strong-coupling limit. He also extended the calculations of Wentzel's charged scalar meson field to a neutral pseudoscalar field. However, he failed in generalizing the quantum calculations a step further to a charged pseudoscalar field. Interestingly, the formulae for strong-coupling cross-sections obtained in the classical and the quantum case turned out to agree exactly. These results permitted Oppenheimer and Schwinger to conclude that 'these methods are sufficient to decide in favor of a pseudoscalar, rather than a scalar or vector, field to fix roughly the values of the coupling constant and source size needed to make the model definite.' [26] This marked the second time that Schwinger concluded that the meson was a pseudoscalar; a correct conclusion, although here based on a false premise.

Although related to the incorrect conception of a single 'meson,' these calculations represented an important piece of research for Schwinger, who then fully mastered the technique of unitary canonical transformations. It marked an important step in the development of strong coupling theory. In this particular application he used the canonical transformation for separating the

* This appears to be Schwinger's first use of a source function, a concept which became increasingly important throughout his career.

wavefunctions of individual isobaric states, but later it would play a crucial role in many formal applications, eventually including the renormalization method of quantum electrodynamics.

The first of the three papers presented by Schwinger at the 1941 Pasadena meeting of the American Physical Society was a progress report on research with Edward Gerjuoy [27]. Recall that Gerjuoy had been an undergraduate with Schwinger at City College in 1934. Now he was one of Oppenheimer's graduate students. He recalled an amusing incident which happened one day while he, Schwinger, and Oppenheimer were talking in Oppenheimer's long office in LeConte Hall. Two other students, Chaim Richman and Bernard Peters, came in seeking a suggestion for a research problem from Oppenheimer. Schwinger listened with interest while Oppenheimer proposed calculating the cross section for the electron disintegration of the deuteron. That midnight, when Gerjuoy came to pick up Schwinger for the latter's breakfast before their all-night work session, he noted that Schwinger, while waiting for him in the lobby of the International House, had filled the backs of several telegram blanks with calculations on this problem. Schwinger stuffed the sheets in his pocket and they went to work. Six months later, Gerjuoy and Schwinger were again in Oppenheimer's office when Richman and Peters returned, beaming. They had solved the problem, and they covered the whole board with the elaborate solution. Oppenheimer looked at it, said it looked reasonable, and then said, 'Julian, didn't you tell me you worked this cross section out?' Schwinger pulled the yellowed, crumpled blanks from his pocket, stared at them a moment, and then pronounced the students' solution was okay apart from a factor of two. Oppenheimer told them to find their error, and they shuffled out, dispirited. Indeed, Schwinger was right; they found they had made a mistake, and published the paper,[35] but they were sufficiently crushed that both switched to experimental physics.[5]

After their midnight repast, Gerjuoy and Schwinger would work till 3 a.m., when they would stop for lunch; then they worked in LeConte Hall until 7:30 in the morning, when Gerjuoy would have to stop to get ready for his duties as a teaching assistant. Evidently, Gerjuoy got little sleep at this time, having also been recently married. For their problem they had to evaluate some 200 spin sums; to check their results, they decided to compute them separately, and compare the results. They disagreed on only 20 terms, but in each case Schwinger was right and Gerjuoy had made a mistake. But, unlike Peters and Richman, Gerjuoy had enough faith in his own abilities, and recognition that Schwinger possessed another class of intellect, that he did not give up theoretical physics.[5] Moreover, never 'did Julian gloat about it or in any way put me down.'[36] (At some point during their collaboration Gerjuoy taught Schwinger to play pool.[5])

They continued their work through the summer and by the end of the year submitted as a more comprehensive paper 'On tensor forces and the theory of light nuclei' [30].* Gerjuoy stepped into the project on tensor forces where Rarita had left it, and understood the physics much better.[5] At that time, the magnitudes of the tensor and non-tensor coupling constants as well as the parameters describing the radial shape of the interaction potential were already available from the calculation of the quadrupole moment of the deuteron and other properties of the neutron–proton system. The next step in the investigation obviously had to be to reach beyond the two-body problem of the deuteron to the calculation of the wavefunctions of light nuclei composed of three or four nucleons. The binding energies of the nucleons in light nuclei have an interesting pattern which at that time had no theoretical explanation: the alpha-particles $^4$He are very strongly bound, while the three-nucleon nuclei of $^3$He and $^3$H are considerably less so, and the deuteron binding energy is practically zero on the nuclear scale. Schwinger suspected that this must be an effect due to the admixture of tensor forces in the interaction potential, but he could not prove it directly by an explicit calculation because the technical aspects of the nuclear three-body problem presented an immensely more difficult challenge and were much different from the deuteron problem.

First of all, the spin–orbit coupling brought in by the tensor coupling changed the classification of the energy eigenstates. The total spin of a nucleus was no longer a constant of motion and its value was not a good quantum number for identifying the state of the nucleon. Therefore the traditional spectroscopic classification developed for atomic optical spectra had to be replaced. Although this was true as well in the case of the deuteron, the situation was simpler there because the symmetry or antisymmetry of the wavefunction permitted the conservation of the total spin quantum number. Furthermore, for the two-body system parity could be used to eliminate certain angular momentum classifications from the ground state. These special features, of course, could not be applied to larger systems. Matters were further complicated by the greatly increased complexity of the variables needed to describe the spatial structure of the many-nucleon system. Therefore the numerical techniques that worked sufficiently well for Rarita's computations were now entirely inadequate. Instead, Gerjuoy and Schwinger turned to the variational method, which from then on became Schwinger's preferred technique for many years to come. He perfected such techniques in his war work at the MIT Radiation Laboratory a few years later, but nonetheless the paper with Gerjuoy represented an important step in the application of this technique to nuclear physics.

* Gerjuoy actually wrote this paper after Schwinger left Berkeley, and received only minor comments from Schwinger.[5]

The technique was not much different from the one that had been in use for a while for estimating the ground state energy of a quantum system. In such applications, one starts by making a reasonable guess on the general form of a trial wavefunction $\phi$, leaving in it a free parameter, say the rate at which it decreases with the distance, and then varies the parameter in order to minimize the value of the energy,

$$E = \frac{\int \phi^* H \phi \, d^3 r}{\int \phi^* \phi \, d^3 r}, \tag{3.12}$$

where $H$ is the interaction Hamiltonian. The lowest value of $E$ generally provides a good estimate of the ground state energy.

In 1937, L. H. Thomas had pointed out[37] that if the binding energy of a nucleus is known, this procedure can be reversed and modified so that it could be comfortably used, through a series of iterations, for finding wavefunctions and the parameters defining the shape of the nuclear potential. Schwinger recognized the power of the variational methods and adopted them as his favorite workhorse, especially for many-body nuclear calculations. He learned how to exploit this technique in its full capacity in his work on waveguides, and later advocated its use in his lectures on nuclear theory that he gave after World War II. It was to play a major role in his later developments of quantum electrodynamics.

As was the case with the Rarita–Schwinger papers on the deuteron, the key aspect of the work on light nuclei was to figure out the exact percentage composition of angular momentum states of the ground state. For example, the ground state of $^4$He was an unknown mixture $^1S_0$, $^3P_0$, and $^5D_0$ states. As the trial wavefunctions for the variational method, Gerjuoy and Schwinger chose the products of the exponentials of the negative sum of squares of mutual distances between nucleons and the expressions, built of Pauli spin matrices and vector distances between nuclei, that were necessary to provide correct symmetry properties. For the shape of the interaction potential, which was less important, they substituted simple square-wells of the size and depth previously determined from the deuteron data by Rarita and Schwinger [23, 24]. Variational calculations were then conducted by minimizing the energy with respect to multiple parameters: separate rates of exponential decrease for each spin state present in the mixture, and the coefficients describing the relative amounts of these spin eigenstates in the wave function.

The results of these calculations were that the $D$-states admixtures of $^4$He and $^3$H were only about 4%, but that only about 50% of the observed binding could be accounted for in this way. This discrepancy led Gerjuoy and Schwinger to conclude that 'the assumption that the ordinary and tensor forces have the same range is not adequate.' [30] Unfortunately the calculation was eventually found to be partly incorrect. An error had been made in the choice of algebraic terms defining the spin and angular momentum of the wave functions. The tensor interaction potential introduces a spin–orbit interference which violates the conservation of total spin, while leaving the isospin degrees of freedom intact. Therefore, for describing the state of the nucleus the eigenvalues of the orbital momentum must be used together with isospin quantum numbers. The awareness of this fact came only gradually, and at the time the paper was written Gerjuoy and Schwinger did not know about it. Hence their classification of states was incomplete.[38] The error affected the wavefunctions of the $^4D$ state of $^3H$. Actually there are only three independent components of the $^4D$ state, but Gerjuoy and Schwinger represented it as a combination of four states, which were therefore linearly dependent and not mutually orthogonal. In any event, Schwinger continued this work with Robert Sachs when he went to Purdue, presenting calculations of the magnetic moments of $^3H$ and $^3He$ at the 1942 Baltimore APS meeting [32] and a full paper after the war [36].

## Departure for Purdue University

Oppenheimer and Schwinger parted as good friends, 'I do have the feeling that Oppie appreciated me particularly. First of all it was clear from our conversations that were rather friendly and intimate even though I still did not quite know how to act in the face of His Majesty.'[3] Oppenheimer's high regard and respect for Schwinger continued forever. 'When [in 1947] he finally decided to leave Berkeley to go to the Institute [as Director of the Institute for Advanced Study in Princeton], he very delicately explored with me the possibility of my coming to Berkeley to take over. I don't know what exactly [to take over], his professorship, his chair, or at least come to Berkeley. That didn't work out, but it certainly indicated a fairly high regard for me.'[3] However, as we shall see later, this offer, which Schwinger regarded as duplicitous, left a bitter taste in Schwinger's mouth.

There was another reason why Schwinger did not regret much when he parted company with Oppenheimer after two years despite his great respect for Oppie as a scientist. 'I would have enjoyed staying on at Berkeley,'[3] but he increasingly felt that Oppenheimer was losing his creativity because he chose to become an organizer, a manager unwilling to be burdened with demanding details. Throughout his career, Schwinger had a deep respect for the 'theorist's manual labor,' which he thought of as a key to success; he would even redo a seemingly routine calculation as pure exercise, for the purpose of speeding it up and developing a better command of the techniques. Thanks to this attitude and constant practice, he would go through monumentally complex calculations with ease, without making even the slightest error. Deep respect for detail had become a characteristic trait of Schwinger's entire career as a scientist. He had a rare talent for making the details actually work for him, be it as a source of approximation in a phenomenological calculation or a decisive criterion of validity and internal harmony of the theoretical logical structure. He had little respect for colleagues who would abandon the details in pursuit of more grandiose plans.

Asked how he coped with Oppenheimer's pervasive influence, Schwinger explained: 'This is not easy to answer. The resistance did come but it took a little longer. I had a feeling that I had found something more interesting in Oppenheimer than in these other people, so I wanted very much to get the feel of him, and learn something from him, of course. But ... he was constantly on stage himself, which was a little difficult to cope with at first, but I gradually got used to his mannerisms, although I must say that his manner of speaking always left me baffled as to what he was actually saying.' The mannerisms that struck Schwinger in Oppenheimer as the actor, public figure and teacher, were 'certainly the quickness, sharpness, and acuity, plus of course his attitude of putting people off.* One got this feeling that he very much insisted on displaying that he was on top of everything, which he very often was. But as I grew to know him more and more it became clear that since he no longer concerned himself—I'm now speaking scientifically—with the details of things, it became more and more superficial, which I regretted very much. It was a lesson to me, never to lose completely your touch with the subject, otherwise it's all over.' Oppenheimer continued to act even later in life as if he was on top of everything. 'Well, he could pull it off better than most people. He did have a quick brain. There was no question about that, but I think the brain must be supplemented by long hours of practice that go into the fluidity and ease. Without the technical practice sooner or later you get lost.'[3]

Schwinger left for Purdue with a sense of expectation. On the way to Indiana he briefly retraced the path of his earlier journeys westward from Columbia University,† and went to participate in the Michigan Summer Symposium in Physics. Often referred to simply as the University of Michigan Summer School, the Symposium was a two-month long learning workshop intended to bring together the cream of active theoretical physicists with bright graduate students

* Gerjuoy recalled that he asked Schwinger for help on another part of his thesis one day while both were in Oppenheimer's office. Julian responded by putting the entire multipole expansion formalism on the board, and patiently explained matters to the bright student and co-worker. Just then Oppenheimer came in, glanced at the board, and put both down for wasting time on such elementary matters.[5]

† Schwinger drove across the country with Rarita. Of course, in accord with Schwinger's habits, they traveled mostly at night. They had only one near accident.[39]

from all over the American continent. He had attended it as a graduate student in 1937. This time Schwinger arrived as an invited lecturer, which in itself was an unusual distinction, especially since he joined at so young an age the élite company of Wolfgang Pauli, Frederick Seitz, and Victor Weisskopf. At Ann Arbor Weisskopf and Schwinger became good friends.[1] Weisskopf had built his career on electron and early radiation theories. After the war, their continuing friendship and frequent contacts actively influenced Schwinger when he set as his goal the quantization of relativistic electrodynamics.

At Purdue University, Schwinger was given a salary of approximately $2000 per annum, only slightly higher than what he was earning as Oppenheimer's assistant. Thanks to his earlier experiences in Wisconsin and trips to Ann Arbor he was no stranger to the new surroundings, even though 'Purdue was a strange place to have [a stimulating environment], in the middle of nowhere, particularly an engineering school by and large. So in effect I accepted. I had gotten used to the idea that somebody was guiding my life and that I didn't have to worry about myself. Indeed, Lark-Horowitz had become chairman of the department of physics, and he set out to organize the collection of bright youngsters who were available cheap in the United States.'[3] There was in fact nothing to worry about.

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# 4

# During the Second World War

## A job at Purdue University

In the late summer of 1941 Julian Schwinger arrived in West Lafayette, Indiana, to assume his first regular teaching position as an instructor of physics at Purdue University. His research work continued uninterrupted. Purdue was primarily an engineering school and in the early stages of the war, after the fall of France, ever-increasing numbers of its faculty, including those in physics, had begun to leave to join various defense-related research and development projects. At the same time, the numbers of students enrolled in technical fields greatly increased, creating heavy teaching loads for the remaining faculty. This suddenly opened unexpected employment opportunities for fresh PhDs. The atmosphere in the Physics Department, with Karl Lark-Horowitz as the energetic Chairman, was very stimulating; Lark-Horowitz had recruited many bright members of the faculty and a body of promising graduate students. Very soon many of the staff would have to leave, but for a brief period of time they were still able to enjoy their academic life in peace.

Although he had no particular desire to leave Berkeley, Schwinger recalled that the transition from Berkeley to Purdue was really not that horrendous, 'because I had had that previous experience at Wisconsin and Ann Arbor. And the Middle West didn't frighten me, but now I was actually teaching.' The salary was in the range of about $2000 per annum. 'I presume it was greater than the National Research Council fellowship, but not much; it was adequate for the time. [It even brought one article of luxury which Schwinger coveted very much, a sleek large black Cadillac.] Well, the research continued and I would give these courses. I think my errant ways were still in evidence, because I do recall once oversleeping for a lecture course, and the students went in high dungeon to complain to some dean or another, who didn't do very much to me.'[1]

After this incident the departmental secretary was given instructions to call Schwinger early enough, around noon, to make sure that he arrived on time for his lectures. The driving force behind this was Frank Carlson, who made sure Schwinger met his classes, and that his teaching schedule would fit with his habits.[2] Julian had never taught a regular course before, although he had occasionally substituted for Rabi in his lectures on quantum mechanics at Columbia.* He had a natural talent for lecturing, but the practice of teaching was not only new, but entirely foreign to him, considering his experiences as a student when he had managed to earn his degrees without attending classes. He had probably never even sat through a college course in physics or mathematics in its entirety. He had nothing to emulate, everything to invent, and many ordinary things that happened in the classroom surprised him. Apart from a few guest lectures at Columbia, he had only given research seminars at City College, Columbia, and Berkeley. He was not much older than his students. 'Well, from what I experienced it was a disaster. I had to learn to adjust to the audience, which was not easy. It's still not easy.' Schwinger did all the things he was required to do in teaching, 'I am conscientious when I have to be,' but he did not enjoy the mechanics of record-keeping and exams. 'Teaching, of course, came naturally to me, but it gave me pleasure only when the audience was appropriately responsive. When they were indifferent or downright stupid, I regarded it as a waste of time; I still do.

'I gave a course to freshmen and sophomores, then alongside I gave a course in quantum mechanics. And there were a few people in the latter who were quite good. ... I had good feedback on the graduate level, but the undergraduate level, I think, was a mutual disaster. I had never confronted an audience like that before; I had no idea how to handle it.

'I have only one memory of that course, which was explaining to them about parabolic motion of projectiles (it was a course in general mechanics). I described the Big Bertha cannon in World War I and we picked out some numbers and worked out the effective range which the cannon ball would travel, and it turned out to be an enormous distance and they said, "But that's not what Big Bertha did," and I said, "But remember that these calculations ignore air friction," at which the whole class broke up. I had fooled them. I hadn't told the truth and they were totally unaware of that important detail. That was a shock to them, ... that the laws of mechanics operate as an idealization and that the real world is much more complicated. I had entirely neglected to mention this. My fault!'[1]

* Even while an undergraduate at Columbia, Schwinger gave lectures. From the beginning, he never learned to end a colloquium on time. In Chapter 13 we will recount a story he told in 1967 of how Rabi had to bring him up short, with an unwelcome but humbling question, so that the audience members could make their commuter trains.[3]

When he gave the course in quantum mechanics, it was mostly based on Schwinger's ideas and the book of Dirac, 'with a little bit of Pauli thrown in. I remember I took great pleasure in giving Pauli's matrix-mechanical solution of the hydrogen atom, which I always thought was much deeper than solving the Schrödinger equation, but I'm not sure how many followed it. I would never, even at the beginning, bring the discussion down to the level that the audience expected. I wanted to raise the level, not descend below it. But I'm afraid that with the sophomores the gap was just too much.'[1]

In describing his experience, Schwinger was probably being too harsh on himself, but the fact is that after the Purdue venture he never again taught an undergraduate course until he taught senior-level quantum mechanics at UCLA in the 1970s, which he felt was quite successful, and even led to his research on the Thomas–Fermi model which we will describe in Chapter 15. The frustrations related to teaching did not in the least distract him from steady research work. For a while he cultivated close contacts with his Berkeley friends and continued by correspondence several joint undertakings he had initiated at Berkeley. At first he remained in contact with Oppenheimer. 'As a matter of fact I remember at first writing—you may know that I am not a letter writer—but I remember dropping a sort of research letter to Oppenheimer every week. Then it began to tail off, because our interests were clearly not on the same wavelength. I don't remember that I ever in my life sent a letter to Rabi. After all, Rabi and I were not co-researchers.' It was a different relationship with Oppenheimer. 'And research was my lifeblood. The social aspect did not interest me particularly.'[1]

One unfortunate consequence of this was that after Julian gained independence, his relations with his family also began to suffer. 'My mother and father were back in New York. I was neither a great letter writer nor telephonist. There was the war on, and my brother was drafted to work on something technical which had to do with radar. The memory I have is of him telephoning me at Purdue from somewhere, asking if I wouldn't come home to visit him because he was due to be shipped somewhere. I don't know where. And that was really terrible, because I had these classes to teach and nobody to take over from me. As a result I had to turn him down. I doubt if he ever forgave me for that, and rightly so. My brother was in the Navy all through the war. I doubt if he ever saw dangerous action.* My last memory of him in this connection is that he was at a Naval base somewhere in Virginia. Then he was shipped off someplace. I can't remember where. So we didn't really have any direct contact. The family

* Actually, Harold first worked for the Navy as a civilian, and then enlisted. He worked as a radar technician. He inadvertently avoided being killed in the Battle of Midway because he fell asleep on the beach in Hawaii, missing his transport to action.[4]

was dispersed. I kept in touch with my parents in a nominal way.'[1] There was no strong interaction.

Schwinger's first scientific endeavors at Purdue were understandably devoted to completing the outstanding cooperative projects from Berkeley. He finished a paper with Gerjuoy on the binding energies of light nuclei [30] (almost entirely written by Gerjuoy[5]), which was received by the editor of the *Physical Review* just six days before the declaration of war by the United States. It turned out to be the last regular article Schwinger would publish before devoting himself to work in support of the war effort. With the exception of two abstracts of communications to APS meetings in late December 1941 [31] and late May of next year [32] he did not publish anything until after the war was over.

The latter of these two communications was a joint paper with Robert G. Sachs. Sachs arrived at Purdue from a postdoc position with Edward Teller, and was appointed as an instructor starting in the spring semester of 1942.[2] Their professional relationship quickly grew into a lifelong friendship. The topic on which they collaborated was the calculation of the magnetic moments of three-nucleon nuclei of $^3$H and $^3$He. Although it represented a natural continuation of the earlier computations of Schwinger and Gerjuoy on the binding energies of $^3$H and $^3$He [30], this time it was Sachs who proposed the calculation of the magnetic moment of the triton. He suspected that it might be a better probe for examining the quantum composition of light nuclei than their binding energies which, as Gerjuoy and Schwinger had found out, produced somewhat ambiguous results. Sachs thought that it was very likely that Schwinger might already have done such an investigation, or had obtained at least some partial results and, like so many times before, had not gotten around to writing them up. Not willing to duplicate someone else's work Sachs asked Schwinger about it. Schwinger said that he had not done so, but found the idea appealing enough that he was willing to sit down and immediately start to work on the calculations. Everything went quickly and uneventfully and Sachs and Schwinger managed to complete the entire work by May 1942. The complete article was not published until mid-1946 [36]. After the war there were many belated pre-war publications like this, and others produced during the war under very unusual circumstances; therefore the *Physical Review* published such papers without specifying their actual submission dates. The 1946 article, which is only three pages long, concludes that if the (plausible) assumption is made that only *S* and *D* wavefunctions contribute significantly to the ground state, and if these wavefunctions are particularly simple, then the 4% *D*-state admixture found by Gerjuoy and Schwinger [30] leads to the following values: $\mu_{\mathrm{H}} = 2.71$, $\mu_{\mathrm{He}} = -1.86$, in the units of nuclear magnetons. The current experimental values are 2.98 and $-2.13$, indicating that the various assumptions made in this early theory were not especially accurate.

At the time of their collaboration in the early months of 1942, Schwinger and Sachs developed quite a close personal relationship that continued through their entire lives. Schwinger became a frequent visitor at Sachs' house, where his hosts were very tolerant of his unusual work and living habits. These visits were important for him as the main opportunity of having social contacts with other people, and the meals cooked by Jean Sachs provided a welcome departure from his usual daily staple of steak and French fries before going to work and ice cream on the way back home.[2] Sachs recalled celebrating Schwinger's birthday in 1942: 'We had to spend the whole time trying to cheer him up because he had already reached the grand old age of 24 and not yet made the required great discovery expected of him.'[6]

While the research with Robert Sachs involved only quantum-mechanical calculations, Schwinger's paper [31] contributed to the December 1941 APS meeting, was already 'On a field theory of nuclear forces.' In it, he addressed the same two frustrating problems of meson theory which Schwinger had already discussed earlier, the unacceptable $1/r^3$ singularity of the tensor force, and (peripherally) the unexplained penetration range of cosmic-ray mesons. This time, however, Schwinger approached the subject from a purely field-theoretical point of view. He pursued the idea, which had originated in the work of Christian Møller and Léon Rosenfeld,[7] who proposed a two-meson theory combining the vector and the pseudoscalar fields. The hope was that in the case of these two fields, their respective infinities might cancel, and thus the problem of divergences would be resolved or at least alleviated. Schwinger did not present the details of his work in [31] since it was simply a brief abstract, but the results were readily replicable and influenced the subject for some time. In fact, someone (perhaps one of the discoverers) sent Schwinger a congratulatory telegram on the discovery[8] of the rho meson in 1961.[1] The main claim of the paper was that if 'in addition [to a pseudoscalar meson] a vector mesotron field is postulated which possesses the same nuclear coupling constant as a pseudoscalar field, but whose particles differ in mass from the pseudoscalar mesotrons observed in cosmic rays, the inadmissible singularities are removed. The sign and magnitude of the resultant tensor interaction, which behaves as $1/r$ at small distances, is determined by the mass difference of the two mesotrons.' [31] Of course, these calculations could not possibly extend beyond the semi-classical approximation to higher orders of quantum perturbation theory.

Schwinger used the deuteron data to estimate the mass difference between the two mesons, and found that in order for the quadrupole moment to have the correct sign the vector meson must be heavier than the pseudoscalar one. Thus he suggested that both vector and pseudoscalar mesons are responsible for nuclear binding; the former, as the heavier of the two, is unstable with respect to a decay into a gamma-ray and the pseudoscalar meson. Such decays would take place in the higher atmosphere, and then only the softer, penetrating, pseudoscalar component would reach the ground level. Schwinger highly regarded this short communication, and it was included in the collection of his selected papers,[9] with the annotation: 'The prediction of the rho meson.' Indeed the spin-1 meson is the next most important contributor to the strong nuclear force after the pseudoscalar pion. However, the actual dominant decay mode is $\rho \rightarrow \pi\pi$; the decay mode $\rho \rightarrow \pi\gamma$ occurs less than 0.1% of the time. The reason for this disparity is that Schwinger never imagined that the vector mesotron was so heavy as to be able to decay into two pseudoscalar mesotrons.

In the background of this note,* and in most of Schwinger's and others' mesotron papers before the war, was the confusion between the 'mesotron' which was observed at sea level and the strongly interacting Yukawa particle responsible for nuclear binding. It was not until 1947 that Robert Marshak and Hans Bethe[10] came up with the hypothesis† that actually there were two kinds of 'mesons,' now named pi and mu; the pi-meson (or pion) was responsible for nuclear binding and interacted strongly with particles of the atmosphere triggering the production of a series of secondary particles, while the mu-meson interacted only weakly and this explained its long penetrating power. (Nowadays, we reserve the term meson for strongly interacting bosons; the spin-$\frac{1}{2}$ 'mu-meson' became the muon.) Before then, physicists were extremely hesitant to postulate the existence of any new particles, and it was conceptually easier to believe that a single kind of meson produced two strikingly different kinds of effects. The pion, decaying into a muon, was first experimentally seen somewhat before the Marshak–Bethe proposal.[13] Of course, this 'two meson' theory ($\pi$, $\mu$) had nothing to do with Schwinger's two-meson hypothesis ($\pi$, $\rho$). Schwinger first heard about the Marshak–Bethe theory at the Shelter Island conference in June 1947, but commented: 'I don't recall as being particularly struck by what Marshak considers his finest hour, in which he suggested the two-meson theory.'[1]

## The war and the dilemma as to how to contribute to the cause

The fateful day in December 1941 when the Japanese bombed Pearl Harbor was a Sunday, and Schwinger was sound asleep until late in the day, unaware of the momentous events taking place. He was awakened by an unexpected

* An expanded version of this abstract was started but not completed, presumably because of the pressure of war.[3]

† This idea was anticipated by nearly a year by S. Sakata and T. Inoue,[11] who proposed the process $\pi \rightarrow \mu + \nu$, where $\nu$ is the spin-$\frac{1}{2}$ neutrino. For more fascinating details of the mesotron–Yukawa meson confusion, see Ref. 12.

call: 'Somebody telephoned me sometime in the afternoon when I was sound asleep, and said, "Have you heard that Pearl Harbor has been attacked?" I asked, "Didn't they first attack the Philippines?" [which the Japanese did within 10 hours of the attack on Pearl Harbor]. Or if "not the Philippines, but somewhere in Southeast Asia?" It was clear that the Japanese were moving down the coast. And the guy who called me said, "Yes, they did that too!" My reaction was fury. "How dare they?" I remember saying something childish, "We must smash them!" as though it were a comic opera or something.' For one who has come to love Japan, this was an extreme sentiment. At that time Schwinger felt rage toward them. But history alters things, 'so does wisdom that comes with age, I guess. At that time no other reaction was possible.'[1]

Schwinger did not have to wait long for an opportunity to contribute to the war effort. There was an ongoing war-related research program at Purdue coordinated by Lark-Horowitz, but it was in the area of semiconductors needed for radar detection devices,[2] which was not Schwinger's field of expertise. Then a recruiter showed up at Purdue in the guise of Hans Bethe himself. At the outbreak of the Second World War in 1939, Bethe, at Cornell in Ithaca, New York, was still not an American citizen, and thus not allowed to work on classified projects. However, he was so eager to contribute that he invented and then developed his own project, which was a theory of armor penetration projectiles based on the theory of elasticity. In another project, in part with Edward Teller, he solved the problem of the detachment of the shock waves from projectiles as they reach supersonic speeds. After Bethe became a naturalized US citizen in March 1941, he was swiftly given the top security clearance, which incidentally arrived on the day the United States declared war on Japan.[2] He eventually ended up as Head of the Theory Division at the Manhattan Project in Los Alamos.

Bethe arrived at Purdue in the spring of 1942 on a different mission: to enlist physicists to help in perfecting the generation and detection of microwave radiation in the 40-centimeter waveband. He remembered Schwinger from Columbia, where in 1935, at Rabi's request, he had evaluated Julian's academic potential,* and was of course familiar with his later accomplishments in nuclear physics. Schwinger agreed to join with the same enthusiasm as the rest of the Purdue team, consisting of E. S. Akeley, J. Frank Carlson, and J. K. Knipp, all of whom were personally invited by Bethe to join.[2] The initial team also included Robert E. Marshak, who at that time was at the University of Rochester. They and other talented young people working on

* Recall the glowing letter Bethe wrote on behalf of Schwinger which we quoted in Chapter 2.

different projects went to the MIT Radiation Laboratory for exploratory visits in the summer of 1942. 'I think we were all interested in doing something and so we came to the Radiation Lab during the summer *en masse* to see what was going on. Marshak had rented a house and we all lived in portions of it. I was in the hot attic.'[1] It was not yet clear how the logistics of the radar development would work. Through 1940 and 1941 it was presumed that the scientists and engineers from academe would stay at their home universities for as long as possible, teaching the young generation and training graduate students while contributing their research services to national defense. There were no national laboratories specifically designated for supporting this type of work. People and equipment were scattered all over the country and it was judged that the cost of bringing them together at one place would be too great.

Therefore Bethe's initial plan, which he proposed as the coordinator, assumed that research would be conducted at home institutions and that the participants would meet together only periodically for consultations. Commuting was then done by train, and even though Bethe did not plan more than two or three conferences per year, with additional bimonthly visits to Ithaca of one of the representatives from each participating center, relative geographical proximity was essential for such an arrangement. In spite of its being some 500 miles from Ithaca, Purdue University contributed a disproportionate share to the project in its early stages. The participants took turns in going on relatively short trips from Lafayette, Indiana, to Ithaca, New York, to confer with Bethe who distributed tasks, synchronized the research and communicated with technical people in the development branches, for example at MIT. Schwinger also took a few trips and participated in solving some preliminary problems of applications together with Bethe, J. F. Carlson, and L. J. Chu.[14]

This was the rather typical mechanism of low-profile war programs, which had been secretly created since 1939. Thousands of scientists would be gradually absorbed into defense ventures. The migration was coordinated by the National Defense Research Committee, which was created in June 1940, with the active participation of the National Research Council and the National Academy of Sciences. Although invisible to the general public, it was a potent effort. By the fall of 1941 fully one-half of the chemists and three-quarters of the physicists were already working for national defense.[15] To fulfill future manpower demands, in the forthcoming semester the engineering schools in the United States would accept 120 000 new students, over ten times the typical number that graduated in any given year.[15] The pressures of teaching such large numbers and doing war research ruled out any other pursuits. Only a handful of prominent physicists, without exception foreign nationals, chose not to or were not allowed to get involved in war-related projects.

Schwinger remembered one visit of Wolfgang and Franca Pauli to Purdue University in the fall of 1942 when people were about to move to the MIT Radiation Laboratory. 'Somebody said to me, "Why don't you ask Pauli if he would be willing to help out on these electromagnetic problems." I went around to Pauli's office and I said, "You know, a lot of us here are in the war effort working on electromagnetic problems and we were wondering if you would be interested and possibly help?" He looked at me and said, 'These are well-defined problems, are they not?" I said, "Yes." At which he shrugged and turned away.'[1,*]

The pressure of the war machine was fast building up and the comparatively leisurely mode of operation patterned on independent scientific investigation turned out to be much too slow to be of any significant immediate use to the military. The Purdue people learned that they would have to take a leave of absence and settle down in Cambridge, Massachusetts, where Karl T. Compton was organizing the specialized MIT Radiation Laboratory devoted to research and development in the field of radar and microwave technology. The name 'Radiation Laboratory' was a diversion, camouflaging its real purpose and pretending that it was an academic nuclear research facility. In the early years of World War II, few people had even heard about nuclear fission, nuclear physics was considered harmless and esoteric, and the research on radar was considered to be the most crucial and first in line for capital and human resources. And indeed radar, not the atomic bomb, turned the tide of the war.

The first operational radar installations were built in 1938 by the British for protection of the approach to London harbor. They worked in the 10-meter wavelength radio band and utilized large, fixed land-based installations. Later, smaller ship- and aircraft-borne 1.5-meter wavelength devices were also developed, but they lacked sufficient power and desired resolution. By 1940, Henry A. Boot and John T. Randall from Mark Oliphant's laboratory at the University of Birmingham had constructed a powerful source of 10-centimeter wavelength radiation. It was not a vacuum tube, but a truly revolutionary cavity magnetron, capable of emitting spurts of power up to 10 kilowatts. The device contained several (in the original design six) resonator chambers in which a microwave frequency field was applied to a space charge. The resulting charge oscillations generated further radiation, part of which fed the process and the other part was transmitted to the antenna. It made all other available sources of microwave radiation obsolete, including the two existing traditional American vacuum tube designs, known as the klystrons and resonatrons, which had low power

* Clarice Schwinger recalled that during the Pauli's visit to Purdue, Franca was so bored that when she heard Julian was giving a talk, she attended; she was enraptured by his presentation. She thought Julian was absolutely the most marvelous lecturer she had ever heard.[16]

and operated on the 40 cm band. A prototype was brought to the United States for demonstration by a special mission headed by Sir Henry Tizard. It not only proved the superiority of the British design, but also the concept of organization based on high concentration of all scientific and engineering resources at one location. British resources were already overstretched and it was decided that future work on the device ought to be concentrated at a single such center on the North American continent. MIT was chosen for the site of the project and the Radiation Laboratory was born. Its mission was straightforward, designing and building radar devices for three purposes: aircraft-borne radar for night-fighters, long-range radar for navigation, and a radar for automatic anti-aircraft gun laying. Although its ultimate purpose was technical, the project was headed from the beginning by physicists, not engineers. Lee A. DuBridge, Chairman of the Physics Department at the University of Rochester, was made the Director of the Radiation Laboratory, and F. Wheeler Loomis, Chairman of the Physics Department at the University of Illinois, was appointed Associate Director. I. I. Rabi became Director of Research. He temporarily renounced the principles of curiosity-driven academic investigation and made it clear to all newcomers that every piece of research had to serve the ultimate goal of making a better radar and no departures from that goal would be tolerated.[2]

The Radiation Laboratory turned out to be a great organizational success and the speed of progress was astounding by any standards. In just a few months the microwave wavelength was shortened to centimeter bands and, before the war was over, the emission power reached the magnitude of several megawatts. By the end of the war the Laboratory dwarfed everything with the exception of the Manhattan Project at Los Alamos. Even though large numbers of its workers transferred to Los Alamos, it still employed 4000 people on 15 acres of floor space and maintained spin-off auxiliary operations in the United States and overseas in Great Britain, in liberated France and in Australia.[2]

Julian Schwinger and his friends first arrived in Cambridge in early 1943 to stay there indefinitely. Schwinger rented for himself a room in an austere place with the improbable name of 'Hotel for Refined Gentlemen' where he lived until 1945. The days he spent there were uneventful and solitary. 'I lived a miserable life there, but it was nevertheless very convenient. I never met any of the other "refined gentlemen."'[1] However, the location was perfect, right on Commonwealth Avenue, just half a block from Massachusetts Avenue, and all he had to do to get to work was to take a pleasant stroll across the bridge towards the entrance to MIT. 'As you faced the main entrance, you walked a little bit to the left and down a side street. There, in some buildings left over from World War I, was where the Radiation Lab was put. It was called Building 22, made out of wood, and probably survives to this day. [It no longer exists.] It was very ugly and depressing, especially at night.'[1]

Building 22 became the headquarters of the Theory Division of the Radiation Laboratory. The initial direction of the mission of the Division was to develop a theory of waveguides. Hans Bethe was its unquestionable inspiration, but he transferred to Los Alamos at a very early stage and the Division was headed by George E. Uhlenbeck. By then additional theoretical problems related to radar arose and the scope of the Theory Division was broadened beyond classical electrodynamics to include any theoretical field relevant to radar technology. One of these new subjects was the theory of noise which helped in the discrimination of weak signals from background radiation. Uhlenbeck's personal contribution was developing a statistical theory of random noise and noise reduction through statistical averaging, which resulted in greatly improved methods of signal detection, and benefited experimental technique enormously after the war.[2]

David Saxon recalled the day Schwinger arrived at the Radiation Lab: 'There was a lot of excitement about that. Electricity in the air. I remember the day he arrived. There was a seminar, then a lot of whispering. I looked around. There he was, standing in the very back of the room, quietly.'[17]

Schwinger inherited after Bethe's departure the task of assuming the leading role in the waveguide theory group. The group was not large by Radiation Lab standards: its most active members were A. Baños, J. F. Carlson, A. E. Heins, H. Levine, P. M. Marcus, and D. Saxon. Schwinger assumed this assignment and decided not to become a part of the Manhattan Project at Los Alamos when it was organized there early in 1943. Schwinger was Oppenheimer's friend and, in addition, a nuclear physicist; yet he was probably the only nuclear physicist of consequence who never took part in making the bomb. It was a conscious decision, made after careful exploration of the goals and method of operation of the Manhattan Project, the possible role he could play in it and, ultimately, the moral aspects it would involve.

When Bethe approached Schwinger in the spring of 1942, the recruitment for the atomic bomb project had been long under way, even though Enrico Fermi produced the first nuclear chain reaction only at the beginning of December 1942. Robert R. Wilson, at Princeton, had already begun many months previously to invite young physicists (Richard Feynman included) to join his group which, under the auspices of the Office of Scientific Research and Development, worked on the magnetic separation of uranium ($^{235}$U and $^{238}$U) isotopes. Wilson used a special device at Princeton, called the isotron, which was a kind of linear accelerator that bunched the ions of different mass into individual clusters within the accelerated beam.[18] This method, while quite good for obtaining small amounts of isotopes for research purposes, lacked the efficiency necessary for producing the very large amount of fissionable material needed for an atomic bomb. Therefore, also at Princeton, a committee of eminent physicists was established in 1942 under the chairmanship of Richard Chace Tolman to find better methods for large-scale isotope separation. Rabi and Oppenheimer sat on this evaluation committee and it is quite inconceivable that they would not have discussed whether to include Schwinger in the nuclear project. While the committee included experienced scientists like Karl T. Compton, Arthur H. Compton, R. C. Tolman, and Harold Urey, they routinely sought advice from their younger colleagues. Feynman, for instance, who was then still much less experienced than Schwinger in nuclear matters, was invited to attend and answer their questions. Schwinger had the impression that Rabi preferred to keep him at the Radiation Laboratory, with a niche of his own.[1] It was also Schwinger's preference, but he made the final decision only after an exploratory visit in the summer of 1943 to the Metallurgical Laboratory in Chicago. Saxon recalled that 'there was a period when it was unclear Schwinger would stay at the Radiation Lab or go to Los Alamos—they were trying to get him—after all, his expertise was in nuclear physics. He never talked to me about why he didn't go to Los Alamos.'[17]

The Metallurgical Laboratory was another innocently named center for war-related research of the Manhattan Project. Its main mission was nuclear reactor (called 'pile' in those days) design (chiefly with the projected Hanford, Washington, reactor in mind) and Eugene Wigner and Enrico Fermi played a leading role in this endeavor. This meant that interactions with them would be inevitable. Schwinger was fascinated by the prospect of getting to know Fermi, but after the not-so-perfect experiences from his visit with Wigner at Wisconsin he was not certain what to do.* However, rumors about unspecified and fascinating things going on there were reaching Schwinger, so he decided to visit Chicago for a non-committal reconnaissance. He went there at a time when people were already starting to leave the Radiation Lab *en masse*, principally for Los Alamos. Many, young Richard Feynman among them, heading to Los Alamos were also first temporarily directed to visit the Metallurgical Laboratory while their workplaces were being prepared in Los Alamos. 'People would come out and say, "Hey, come out to Los Alamos. Big things are going on. You'll find it interesting." They were not explicit, but I was interested enough. First it would be necessary for Fermi at Chicago to do something. Wigner was there, . . . and Sachs . . . . They would say, "Come out and just see what it's like." So, I

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* Schwinger recalled: 'Wigner was also recruiting, I suppose for the Metallurgical Lab, and the story is told that in trying to discover whether the people he was approaching were already spoken for, he would ask them, "Can you write Maxwell's equations?" And if they got the sign right, then he knew they were working for the Radiation Lab, working out radar. It's not a very entertaining story. At least, any physicist would never get the signs wrong, I don't think.'[1]

said, "Well, all right." Don't ask me how I could do that during the war; I don't understand it. To blithely pick myself up, get the transportation, and leave! But I ended up in Chicago, where various people I knew were there, which made it pleasant. In particular [Hyman] Goldsmith was there and I stayed with him [for a while and then moved to a hotel]. Bernard Feld [an experimentalist working with Fermi] was also there; he was somebody I knew from New York.'[1] (Recall that he was involved in the experimental 'runs' at Columbia.)

At the Metallurgical Laboratory Schwinger first went to see Eugene Wigner and his friend Robert Sachs. Enrico Fermi was away doing experiments; thus a meeting with him had to be arranged at a later date. After his arrival, Hyman Goldsmith introduced Schwinger to the general scope of the work at the Laboratory and entertained him with a technical conversation 'about some discoveries that they had made. I must say that when one tells these stories and you hear about them, security is laughable. Everybody talked about everything. Goldsmith told me about some experiments in which slow neutrons had been refracted by crystals and he asked me, "Do you understand that?" So I sat down and worked out a theory for it, which he took to Fermi to impress him, and Fermi said: "Oh, yeah, trivial!" Obviously he had worked out the theory himself, he was not going to be impressed by some young kid.'[1]

Schwinger spent about two months in Chicago, where he did many things, mostly classified work connected with the development of reactors. This of course brought him again together with Wigner. 'Wigner was always a mystery to me. When I first encountered him in 1937 in Wisconsin, I was obviously not attracted since I did not follow him around or anything of that kind. I think we interacted reasonably well in Chicago; at least Wigner gave me some problems to work on: a variety of problems like neutron scattering in crystals and the temperature effect, and I worked out something and left the answer behind. I was later told that nobody was ever able to duplicate that, which means that either I was wrong or they were stupid. I never found out. I did some calculations on reactor effects, temperature dependence and so forth, and when I returned to the Radiation Lab, I wrote back finishing up the details of whatever I was doing and sent it to Wigner. He was a strange little man with all these stories one tells about him, but we talked and there was no problem.'[1]

Bernard Feld, Schwinger's old acquaintance from Columbia, spoke at UCLA on the occasion of Schwinger's sixtieth birthday celebration and reminisced about their meeting at the Metallurgical Laboratory; 'I hadn't realized that he'd been doing engineering already at the Radiation Lab, but Julian is a pretty good engineer and he demonstrated it that summer. I guess there were a number of problems that remained to be solved, mainly to help the design at Hanford, and these were in transport theory and some of them were more or less difficult and Julian was working on some of them.

'... I had known Julian from Columbia, slightly, but at least well enough I figured that I might be able to help in making a match between Julian the project. It was really a very interesting process because what would happen was that I would go around in the afternoon—not every afternoon, but occasionally—go around to my friends in Wigner's group and sort of try to smell out what were the problems with which they were having trouble. Things that were giving them difficulty. And then sometime in the late evening, maybe 10 o'clock or 11 o'clock or something, I would wander ... [into Julian's office] and Julian would be sitting there at his desk, typically.

'This was a very hot summer in Chicago, and Julian was a very fastidious dresser in those days. He never took off his coat. In Chicago I never saw Julian without his jacket. He would be sitting there with his white shirt and tie, tie never loosened, jacket on, with pad and paper, he would be scribbling furiously, working on some problem on the pad, with his handkerchief, supersaturated handkerchief in the left hand, mopping the sweat off his brow as he worked, and I used to wander in and sit down and wait, and at some point Julian would pause to catch his breath and I would kind of interrupt him and try to get his attention away from whatever he was doing, and I usually succeeded not only because I was a pretty persistent guy, but because Julian is a nice guy and if you sort of bother him, he'll pay some attention to you, and after a while I would get him interested in the particular problem I had in mind.

'I'd start talking about it and Julian would get interested and then he would go to work on it. He'd get up to the blackboard and I would start making notes. As he worked on the problem, I would be taking notes and sometimes, you know, that could be pretty hectic. I don't know any of you who saw Julian work in those days. Julian is ambidextrous. He has a blackboard technique that uses two hands, and frequently, when he really got carried away, he would be solving two equations, one with each hand, and trying to take notes could be a hectic job. Well, at some point, either we would finish the problem or the dawn would start to break in the eastern horizon, and we would decide it was time to quit and then often, we would go to have breakfast together. We would get into Julian's sleek black Cadillac and go to the nearest all-night eatery, where we would both have breakfast of, I think it was steak, and then off we would go to our respective beds.'[19]

Talking to people at the Metallurgical Laboratory was equally illuminating as a frightening experience for Schwinger. 'I began to work on some of this stuff concerned with the development of reactors, and I began to appreciate the kinds of energies they were talking about and appreciate what Los Alamos was going to be: namely, dropping bombs of these energies. Fermi had already discovered the chain reaction in December 1942 [actually he developed the controlled fission reactor, or pile]; the idea now was to design reactors to produce plutonium for the atomic bomb. And I said no to that. It was a gut feeling with me. I looked at these energies, and I said that this was beyond human comprehension. I did not verbalize it. I withdrew. I felt this was the wrong thing to do without knowing in detail why. I appreciated that I was being rather unique, because after all I was really a nuclear physicist. Every nuclear physicist I knew was out there at Los Alamos, but I did not want it. I think it was the right choice; it was visceral. . . .

'My own reaction had nothing to do with scientific things. Part of my feeling was, "Look, I've been a nuclear physicist for a long time. These people are doing nuclear physics; there is no new nuclear physics; this is engineering. I'm not really interested in that." Whereas this electromagnetic stuff, while it was not fundamental, certainly was very challenging. I could be useful at the Radiation Lab, while any nuclear physicist could do what they were doing out there.*

'In addition, I had a very uneasy feeling that something unnatural was associated with Los Alamos. Something evil. It would have been so easy to have said yes. All my friends were out there and were obviously doing exciting things; well, not all my friends, but many of them .'[1]

The prospect of being able to participate in any kind of scientific research, even though it was not fundamental, was magnificent, considering the alternative. Schwinger had thought of joining one of the services if they would have him, and at the Radiation Lab he was even called up to take the military draft physical examination, although 'I was a rather sorry mess physically. I had let myself go to fat, and my teeth were in terrible shape. The examiner looked at me and said, "Well, we'll fix this. I think you'll make a good marine!" At which I almost fainted. That seemed a little improbable, because the Radiation Lab was defending its important people. But there was a period when they just simply couldn't stop this. I don't think anybody was drafted but it got as far as at least looking over the bodies. . . . I was standing naked and some guy in charge of all this was mumbling something to me. I said, "Excuse me, what did you say?" and I went forward and put my hand on the desk. In true military style he glared at me. How dare I intrude on his private space? It was clear that the military and I were not meant for each other.'[1]

This was as close as Schwinger ever came to military service. The draft orders never came and his future with the Radiation Lab, where he was more valuable to the country, was secure.

## Waveguides

Schwinger considered his work at the Radiation Lab as his patriotic duty and was curious and optimistic in accepting the challenge of working on a subject

* Nevertheless, Schwinger completed some substantial calculations of neutron scattering and pile heating at the Metallurgical Laboratory and subsequently.[3]

that soon turned out to be not an involuntary deviation from research at all, but rather an authentic and fascinating research opportunity, which had important consequences in his postwar research. 'The research direction was the theory of waveguides, but there were many things going on. There were antenna design problems. There was the theory of the magnetron, which was the generating device. But I was not interested in that. In a sense, I didn't want to learn too much of electrical engineering, but [the theory of] waveguides was clearly electromagnetic theory: Maxwell's equations and electromagnetic waves, they were like quantum mechanics, so that I was not being pushed too far away from familiar concepts.'[1]

What were waveguides and why had a special theoretical group been formed to study their properties? The core of the staff of the Radiation Laboratory consisted mostly of electrical engineers. They were the cream of the crop, but competent mainly in the theory of transmission lines, traditional circuits and radio technology. They had lifelong experience in working with devices that were several orders of magnitude smaller than the characteristic electromagnetic wavelength that they carried. In such conventional circuits there is generally no need to describe the flow of power in terms of detailed field quantities. Rather than working with the electromagnetic fields in and around conductors, it is sufficient to lump the electric and magnetic local aspects of the field, and replace them by global quantities like voltages and currents. The flow of electrical power through the discontinuities produced by the gaps of capacitors, turns of coils, etc., can be explained in terms of global 'lumped' characteristics, like capacitance, inductance, reactance or impedance.

Traditional circuits had some specific limitations in the aspects of transmitting power in large quantities or across large distances, but they were well understood and manageable. Nevertheless, there were additional problems associated with higher frequencies. For example, higher frequency currents travel on the surface, not through the volume of the conductor, which has an effect on the amount of power that the conductor can handle. Short-wavelength currents that are transmitted through circuit branches of unequal length can also interfere. These effects posed mostly technical problems, but for microwave frequencies the nature of the difficulties suddenly turned out to be fundamental. The wavelength of the radiation used in radar was shortened first to ten, then three centimeters, and eventually even 1.25-centimeter bands were explored. At such wavelengths ordinary circuits do not conduct electromagnetic energy, they diffract it.

Therefore, microwave energy must be carried not by a network of wires, but through metallic pipelines that confine the field to prevent energy loss. Such pipelines were given the name of waveguides. As the field propagates through a waveguide, the energy it transports and the harmonic composition of the modes of radiation are affected by the geometry of physical obstacles in its path: posts, holes, bends, and apertures. The theoretical aspects of waveguide design resemble more the theory of sound waves than electric currents. The knowledge and intuition of trained electrical engineers, stemming from the assumption that currents and voltages are constant along the wires, became useless. They needed a good working knowledge of the properties of solutions of Maxwell's equations in metallic cavities, and an understanding of how electromagnetic waves interfere and diffract as they propagate inside waveguides. To make things worse, literally in the heat of the battle, there was no time for subtleties or lengthy calculations; fast and effective algorithmic methods for finding such solutions for a variety of boundary conditions characteristic of magnetrons and waveguides were needed.

Physicists were better prepared for this kind of work. Schwinger felt very comfortable with his assignment, which, because of sheer complexity, was intellectually challenging and matched well his background in electromagnetic interactions in quantum physics. Methodologically, the diffraction of electromagnetic waves resembled the scattering problems in quantum mechanics, so he would not stray too far from conceptually familiar territory. He was happy to find an assignment which would leave him in full control. For a young man just 25 years old, this was an inconceivable luxury in wartime. The experiences of the Radiation Laboratory turned out to be very stimulating for his scientific career and there is no way of knowing what his life as a physicist would look like without them.

When Schwinger took over from Bethe the (informal) leadership of the waveguide theory team, he inherited essentially but a single solved problem of the diffraction of an electromagnetic wave from a small circular aperture in an infinite plane wall. For a while, even this solution of a classic academic problem had to be kept as a military secret, but by the end of the war Bethe published it* in the *Physical Review* as a 'Theory of diffraction by small holes.'[20] Bethe's principal conclusion was that the mathematical theory of diffraction, initially developed by Kirchhoff for the diffraction of light and based on Huygens' principle, does not apply to holes of dimension comparable to the wavelength. He had found an alternative method, valid for circular apertures, and taught it to Schwinger, Carlson, and Chu. Then, still from their respective institutions, they worked together on the applications of Bethe's solution, which they included in a classified technical report on the 'Transmission of irises in wave guides.'[14]

* The journal listed the receipt date as 26 January 1942, but it was not published until the October 1944 issue. The former date was presumably when it was submitted to Radiation Laboratory Director DuBridge.

How were the calculations of that kind done? The light or radiation can be described by a wave satisfying the scalar wave equation, for a given frequency $\omega$,

$$\nabla^2 U + k^2 U = 0, \quad k = \omega/c. \tag{4.1}$$

The fact that the electromagnetic field is a vector, not a scalar, is not essential for describing the principle of the method, which is very general. The function $U$ may represent a component of the electromagnetic potential or even the pressure of a sound wave in an acoustical problem. It can also be generalized to a Maxwell field. The standard technique for solving the partial differential equation (4.1) for given boundary conditions on a surface $S$ is based on Green's theorem. Without going into details of this textbook technique, let us write down the general integral form of the solution

$$U(\mathbf{r}) = \int_S \mathrm{d}S' \left[ -\frac{\partial U_0(\mathbf{r}')}{\partial n'} G(\mathbf{r}, \mathbf{r}') + U_0(\mathbf{r}') \frac{\partial G(\mathbf{r}, \mathbf{r}')}{\partial n'} \right], \tag{4.2}$$

where the integration extends over the surface $S$ (the derivatives are in the direction of the outward normal to the surface), $U_0(\mathbf{r}')$ and $\partial U_0(\mathbf{r}')/\partial n'$, the boundary value of the field, and of its normal derivative given on that surface, and $G(\mathbf{r}, \mathbf{r}')$ is the Green's function, an auxiliary function that satisfies the same differential equation as Eqn (4.1) with an inhomogeneous point-source term. If $G$ vanishes on the surface, $U$ is thereby determined by Eqn (4.2) in terms of its boundary values—this is the so-called Dirichlet boundary-value problem. For the problem of a plane wave arriving from the left towards an aperture in a screen in a plane, at $x = 0$, the solution to the right of the screen can be given in terms of a Green's function of the form $G(\mathbf{r}, \mathbf{r}') = \mathrm{e}^{\mathrm{i}k|\mathbf{r}-\mathbf{r}'|}/|\mathbf{r} - \mathbf{r}'|$.

As Pauli had correctly told Schwinger after being asked for help at Purdue, problems like this were 'well-defined' in the sense that the boundary values of $U_0(\mathbf{r}')$ on the screen uniquely determined the solution in the entire space. However, for diffraction problems nobody could tell what the correct boundary values should be. It was assumed that $U$ and $\partial U/\partial n$ vanish on the conducting surface; therefore the integration extended only over the aperture where the boundary values were unknown. The problem was well defined, but without more physical insight the solutions remained completely unobtainable. This was a new field of knowledge. 'The problems may have been well defined, as Pauli thought, but the solutions weren't. I think he missed that a little bit. But I think Pauli was responding as a proper physicist should. Is there anything new to be learned here? And, technically, in an engineering sense, yes; scientifically, no, except to the extent that waveguides and all the rest of it have been useful tools for further developments.'[1]

The only mathematically rigorous solution of the diffraction problem in existence was Sommerfeld's calculation of diffraction by a perfectly conducting half-plane.[21] Even though it was only a two-dimensional problem, this solution was revered as Sommerfeld's earliest substantial scientific accomplishment. Schwinger's remark that Pauli 'missed a little bit' by calling the diffraction problems 'well defined' was of course an intentional exaggeration. Pauli stated that he was not interested in the subject, but he had tried his hand on it, and even written a paper[22] for Sommerfeld's seventieth birthday *Festschrift* on the asymptotic form of the wave diffracted by two connected infinite half-planes forming the shape of a wedge, the problem solved exactly by Sommerfeld.[21]

When Bethe began to study waveguides, he first turned to the old solution, originating from Kirchhoff, of the problem of diffraction by a circular hole using Huygens' principle which assumed that in the aperture $U$ had exactly the values brought in by the incident wave. He noticed that, after one finds the solution and then computes the values of $U$ on the reverse side of the screen, it turns out that the diffracted radiation illuminates the dark side of the screen and there $U$ and $\partial U/\partial n$ are in fact non-zero on the screen. This discrepancy, which progressively worsens with longer wavelengths or smaller holes, means that in the zeroth approximation in the aperture the electromagnetic field is in fact discontinuous, even though the aperture is actually empty space.

Noticing that Huygens' principle was invalid, Bethe concentrated on small apertures (small in the sense that fields inside them are essentially constant) and effectively reversed the order in which the problem was solved. He solved it by looking for such discontinuities of the magnetic and the normal components of the electric field inside the hole that produced solutions which matched the boundary conditions for the field on the conducting plane. In this way, the theory of waveguides became the theory of field discontinuities within the waveguides. Under the assumption that the hole is small, finding the distribution of diffracted radiation was a straightforward matter (for Bethe, that is, who introduced fictitious magnetic charges and currents to solve the problem), yet the results were so surprising that Bethe spent considerable time looking for errors. There were no errors; it just turned out that a small hole in the screen reflected more radiation than it transmitted, in blatant violation of classical intuition. Bethe wrote: 'The result mentioned is of course exactly the opposite of that expected from any elementary considerations based on the Huygens' principle.'[20] The radiation intensity transmitted through the hole is reduced by a factor of the order $(a/\lambda)^2$, where $a$ is the radius of the hole and $\lambda$ the wavelength of the radiation. Solving the circular aperture problem was important for the theory of waveguides, which indeed often had a shape of rectangular enclosures interconnected by circular holes. But real designs involved far more complicated geometries. The challenge of finding a solution whose applicability would not be restricted to very small apertures and circular shapes was indeed formidable; it seemed that Bethe had reached the limit of what analytical calculus and Maxwell's theory could deliver for the theory of waveguides.

Schwinger did not have much time to accomplish his tasks. Time was of the essence in everything at the Radiation Lab. As a matter of fact, it was so critical that all the results of theoretical calculations were immediately turned over to the technical team for applications. If they were not ready, the construction had to proceed nevertheless, irrespective of cost; trial and error had to substitute for the missing theory. In such an environment, no progress was possible without regular and exhaustive interactions between the diverse mix of staff researchers and technicians. The organization of the Radiation Lab was less formal than the one at Los Alamos and the horizontal flow of information was not restrained. Contrary to what was going on in the Manhattan Project, radar made use of conventional science whose principles were no secret to either warring side. The competition was about better engineering and introducing innovations faster than the adversary. The military viewed radar as a tool of conventional warfare and instituted precautions comparable to those at classified industrial sites. There were guards everywhere and workers were supposed to display their ID badges and forbidden to talk about their work to outsiders, but otherwise contacts between the personnel were free.

Schwinger was not interested in the world beyond the circle of his immediate associates. The larger picture did not concern him too much. 'The Radiation Laboratory as a whole was a gigantic thing with all kinds of organizational structures. It had to be. But I knew very little about what was going on. I was happy in my own little niche; if I had been unable to do anything I would have been miserable, but it suddenly became possible to meet these challenges and contribute something. . . . We all had badges which we had to show while coming in or going out. I don't remember having any problem in getting access to classified documents. In fact there was no need. I was busy producing my own documents. The days went on routinely and there were developments of these ideas and then dissemination of techniques to various people, [and] the transfer [of computational] problems to girls who worked the machines that turned out numbers, which makes contact with Dick Feynman because [at Los Alamos] he was in charge of the computers [in the Theoretical Computation Group]. What a strange assignment for him! I'm sure he didn't confine himself to it.

'I got slightly involved with the people in the experimental group, in particular a guy called Nathan Marcuvitz, who became a very good friend of mine. [Marcuvitz belonged to the advanced development group under Ed Purcell.] We used to talk a lot; he would teach me engineering and I would teach him electromagnetism and, as a result, we got together and worked rather closely on things. Oh, there were so many people there! Bethe had brought this group in;

he was our mentor you might say. Uhlenbeck must have come in at a rather early stage also. David Saxon came in after a while; we later collaborated in writing a couple of books. It's hard to separate events. There was a young fellow called Harold Levine, whom I later took as my assistant to Harvard. The main nucleus of people, whom I knew, were the people who came from Purdue. Sachs was not there; he first went to some place in Maryland where they were doing some legal stuff, and afterwards showed up at the Metallurgical Laboratory. It was his Chicago orientation that left him in Chicago after the war. From amongst the early people who came, a good fraction left to go to Los Alamos and new people came in. Robert Marshak got himself involved with the Canadian Atomic Energy Project at Chalk River. Then there was Mark Kac, the Polish-American mathematician. Once we talked together and I told him about something I was doing in waveguides that he found useful.'[1]

Mark Kac recalled an incident toward the end of the War in which Julian Schwinger made an error. Kac requested help from his friend A.J.F. Siegert, who was at the Radiation Laboratory, in evaluating a complicated expression involving integrals of Bessel functions. Siegert left a note on Schwinger's desk and in the morning a 40 page manuscript appeared. Kac was impressed, but since he knew the origin of the problem (which Schwinger did not) he could check a limit, which did not work. Kac wrote Siegert again, who informed Kac that Schwinger was sure of his result. There was nothing to do but for Kac to learn Bessel functions. After weeks of effort he got the answer, which was the same as Schwinger's except for a constant term. It turned out Schwinger had interpreted a certain integral in Watson's treatise on Bessel functions as a definite integral rather than as an indefinite one. Although this marked one of Schwinger's few mathematical slips, Kac remained impressed: 'Julian's unmatched prowess as a classical analyst is, of course, too well known to require further collaboration; but the feat of solving my strange problem in a few hours (which also included a lengthy writeup) must surely command admiration.'* But as a result of this embarrassment, Schwinger never again copied formulas from books, but derived them *ab initio*.

Schwinger's difficult role at the Radiation Lab was not only to develop the theory but also to teach it to the engineering branch. The latter task was not easy because the language of localized fields and the mathematics used for solving problems of the diffraction of electromagnetic waves were too complex from the viewpoint of practical engineering design. The theory, despite all its complexity, was barely sufficient to describe the simplest geometries, and lacked intuitive patterns for making rough estimates of solutions. The dialog with

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* Mark Kac in *Mark Kac: Probability, Number Theory, and Statistical Physics—Selected Papers*, ed. K. Baclawski and M.D. Donsker (MIT Press, Cambridge, 1979).

the engineers was difficult because the theory lacked the proper language for discussing the myriad engineering aspects of radar. It was still a mathematical physicist's theory, far from becoming an applied science which its users demanded. Schwinger's role was to create a bridge between these two different worlds. In spite of his young age and no experience in a leadership role, during the following two years he fully succeeded and gradually accomplished the rare feat of converting a subdiscipline of physics into a branch of engineering. The progress record of this remarkable accomplishment exists in the form of systematic notes of lectures which Schwinger gave at the Radiation Lab to members of the waveguide team.

The lectures were a necessity. The Radiation Laboratory had all the trappings of a research institution, but as a matter of fact it was a fully fledged war production facility. The research and development teams came to the Laboratory from different backgrounds and disrupted careers to join a totally unfamiliar project. This experience created strong friendships and lasting professional relationships, but Schwinger was upset because he had to waste inordinate amounts of time just to overcome communication barriers with one co-worker at a time. For him, to spend days in crowded meeting rooms was not an acceptable way of being productive. He could not just withdraw into his errant nightly work patterns and the custom of leaving work at seven in the morning to run away from the very people whom his presence was expected to support and who relied on him for answers to very specific questions. A mutually acceptable mode of communication had to be worked out. First, Schwinger's solution was to be around but not much visible. He communicated daily with his closest collaborators, with whom he worked on the immediate projects at hand. Others were assured that he would dutifully show up in the late afternoon or in the evening, so they would leave him notes or write questions and calculational problems on blackboards in their offices. Legend has it that they were never disappointed. In the morning they would always find their work places ready to start a new day, the offices cleaned by the janitorial staff, and stalled calculations worked out by the single-handed Theory Division night shift.*

However, as the work advanced, the increasing body of new knowledge had to be communicated to the team. The easiest way to do it was in the form of an organized series of lectures.† 'A whole group had been working on the

* Schwinger's Nobel Prize autobiographical note contains this description of his role at the Radiation Laboratory: 'Being a confirmed solitary worker, I became the night research staff.'

† Harold Levine, who became his first, and longest-term, assistant at Harvard, recalled that he lectured two or three times a week. 'His lectures were in a class apart, and displayed Julian as a master of technique.'[23]

theory of waveguides. The lectures were my way of forcing myself to develop new things and at the same time transmitting what I had learned to them so that they could use everything directly, instead of talking to each separately and saying, "Hey, you know there's this method," and then doing it all over again.'[1] The lectures provided the means for a two-way communication; in the discussions following the lectures the audience had unrestricted opportunity to talk about their specific problems with Schwinger and suggest a direction for the next developments. All the participants contributed their respective shares, but Schwinger played a dominant role. 'I would lay down a line of a certain technique and apply it to a few things and people would pick up more elaborate applications; it's not that I did it all singlehandedly. But I suppose I must admit that I set the general line of development. The lectures touched on many things and there was always, of course, contact between these problems and the problems of physics that were closely related. Thus you would find discussions early in the game on S-matrix theory.'[1]

David Saxon took notes of Schwinger's lectures and, usually within a few days distributed the edited version among the participants. Saxon recalled: 'My first formal and significant continuing interaction with him came when Uhlenbeck asked me to write up lectures he was starting to give on waveguides. Because he was orthogonal to so many people, Uhlenbeck had the idea of having him come late in the afternoon and he would give the theory of things he had solved and techniques he had developed, all extremely ingenious stuff. Uhlenbeck asked me to write it up. It was very demanding to try to keep up with Julian. (I was trying to do other things too.) The most difficult thing was getting clearance from Julian to actually put [the notes] out. That would involve a huge time delay. Eventually I'd get the notes back. The changes were never trivial or modest. Finally, Uhlenbeck and I threatened him: "Julian, what you're doing is important. If you don't get this stuff back in a timely way, we'll send it out without your permission." Then he really responded, and I got the stuff back very quickly with moderate changes, and then I used to see him frequently.'[17] These hastily prepared notes were necessarily an imperfect product, merely a reflection of work in progress, with occasional errors and redundancies. Yet the mimeographed handouts became something of a standard text and a steady stream of requests for more copies kept coming to Schwinger or Saxon until the late 1960s. These notes got to be widely circulated. A portion was published as *Discontinuities in waveguides* by Gordon & Breach as part of the Documents of Modern Physics series [148], unedited and unfinished in order to preserve the flavor of the original lectures, and which even included Saxon's 1945 memo containing a list of an additional 20 topics covered in the lectures, but still unwritten.

Saxon recalled: 'Once the war ended, my willingness to do this went to zero. Marcuvitz did continue. Julian did start to write [his volume for the Radiation

Lab series]—a chapter or two ... It was beautiful.'[17] Copies of these chapters exist in the Schwinger archive at UCLA.[3] Saxon continued: 'During that period we became quite close. He came to my house many times and listened to chamber music. We ate quite a few meals together—his dinner and my breakfast, at 5:00 in the morning. I quickly discovered he was quite a gracious guy. Although the difference between our abilities was staggering, he was never condescending. Julian was surprisingly accessible. If you knew what his constraints were, you could see him indefinitely.'[17]

At the Radiation Lab, Schwinger's closest collaborators were Harold Levine and Nathan Marcuvitz. Levine, who arrived from Cornell, where he had earned his PhD degree in early 1944, was the first of Schwinger's collaborators who was actually younger than he was. He owed his professional career to Schwinger and after the war he followed him to Harvard as his assistant. Marcuvitz was five years Schwinger's senior; he had earned his degree in electrical engineering from Brooklyn Polytechnic Institute, to which he returned after the war. In the early stage of the work he assumed the difficult role of an interpreter, teaching Schwinger the language and thinking patterns of electrical engineering. By the end of the war, when they had more time to spare, he also taught Schwinger dancing and dating. On one such occasion in 1944 he even presented Julian to Clarice Carrol, the future Clarice Schwinger.[16, 24]

Marcuvitz and Schwinger shared a common New York Jewish background and became close friends. A good part of their discussions took place at restaurant tables. They regularly had supper together and Schwinger would teach Marcuvitz electrodynamics and perturbation techniques used in physics and, in turn, learn the theory of electrical networks and power transmission. The two friends shared an inclination to dine on rather hearty meals and Schwinger's waistline suffered. 'I recall that every evening we went to Durgin Park in Boston, a place near the waterfront, which specialized in steaks and tough waitresses. I do remember that when I staggered out at seven o'clock in the morning from the Radiation Lab and walked across the bridge, there was a restaurant that I came to immediately on Massachusetts Avenue before I got to the corner where my hotel was, and I would settle down to breakfast, which as I recall, was peaches and vanilla ice cream invariably. So you can imagine the shape I was in in those days. I put on weight. There was no exercise except that walk across the bridge and there was that kind of food. I'm sure we also went to other kinds of places, but in those days I was essentially a steak and potato kind of man!'[1]

How was the theory of waveguides developed? The key observation was that it was not necessary to seek the most general electrodynamical solutions of the boundary conditions, because the waveguides were built to favor the generation of one particular type of solution. In a certain way, waveguides are built like musical instruments which favor the propagation of one dominant acoustic harmonic. In waveguides, even though the field excitations near the discontinuities are superpositions of many possible complex radiation modes, the non-dominant modes are localized in the immediate area of posts, apertures, etc., and quickly weaken with distance. They cannot be ignored, and must be calculated; however, only their space-averaged properties affect the outgoing wave. The task of Schwinger's team was to calculate these averaged effects due to the various standard types of discontinuities and devise a method for using such partial results to construct the solutions for larger systems of combined discontinuities found in the actual waveguides.

While the calculation of the composition and distribution of the mix of decaying harmonics was necessary for a complete description of the phenomenon of propagation of the field through a waveguide, the details were completely immaterial for radar designers interested only in the transmission of power through the device. Similarly, when diffraction occurs at the open end of a waveguide, part of the wave is reflected back from some distance beyond the actual opening of the pipe, where it has an antinode, and returns to sustain the standing wave in the pipe. Calculation of the position of the antinode and the portion of the reflected energy is extremely difficult, but the result is immaterial for a radar builder who does not even know whether the antinode in question is real. The wave might as well be reflected from the orifice of the pipe, retarded with a phase shift $\phi$ corresponding to the extra distance traveled. Also the amount of reflected energy can be given by a single coefficient describing the change of intensity due to reflection. The propagation mode of the field is therefore a harmonic wave of dominant angular frequency $\omega$ whose dependence can be described by the real part of the exponential $e^{-i(\omega t+\phi)}$.

Schwinger was expertly familiar with quantum-mechanical perturbative calculations of thermal neutron scattering, for example, where a similar mathematical structure of solutions was encountered in the partial wave expansion. Asymptotically, the effects of very complex nuclear interactions were described by the amplitude and phase shift of one partial wave corresponding to zero angular momentum scattered particles. For Marcuvitz, on the other hand, all this struck a chord with the classical theory of circuits, where the energy transmission or dissipation could be described by reactances, the impedance of the circuit and the accompanying shift of phase of the alternating current. Now the methods of two so unlikely disciplines were to be merged for the purpose of radar technology. Working with Bethe (while still at Purdue), Schwinger had learned a technical trick we noted in passing which Bethe used for the problem of diffraction by small holes. The trick was to rewrite Maxwell's equations in a manner that treated the oscillating electric, $E(t) = E_0 e^{-i(\omega t+\phi)}$, and magnetic, $H(t) = H_0 e^{-i(\omega t+\phi)}$, fields in a completely symmetric way.

The electric charge density $\rho$ determines the divergence of the electric field E, so that one can introduce a (ficticious) magnetic charge density $\rho^*$, and similarly relate it to the divergence of the magnetic field H,

$$\nabla \cdot \mathbf{E} = 4\pi\rho, \quad \nabla \cdot \mathbf{H} = 4\pi\rho^*. \tag{4.3}$$

Further, in the same spirit, in addition to the electric current density J one can introduce the magnetic current density J*, each determined by the curl of the respective field ($k = \omega/c$),

$$\nabla \times \mathbf{H} = -ik\mathbf{E} + \frac{4\pi}{c}\mathbf{J}, \quad -\nabla \times \mathbf{E} = -ik\mathbf{H} + \frac{4\pi}{c}\mathbf{J}^*. \tag{4.4}$$

There was no physical motivation behind this modification of the laws of Maxwell's electrodynamics, other than that it was easier to solve the equations in the general, symmetric, case and then select physical solutions by imposing proper boundary conditions consistent with the nature of both fields. (Later, Schwinger was to take the possible existence of magnetic charges and currents seriously; see Chapter 11.)

In the absence of obstacles, solutions for the field in metallic guides have a very simple form. For a rectangular waveguide of width $a$ and extending in the direction of the $z$-axis, a radiation mode polarized so that its electric field points in the $y$-direction and the magnetic field points in the $x$-direction, could be written as

$$E_y(x, z) = \sqrt{\frac{2}{a}} \sin \frac{n\pi x}{a} V_n(z), \quad H_x(x, z) = \sqrt{\frac{2}{a}} \sin \frac{n\pi x}{a} I_n(z), \tag{4.5}$$

where $\sqrt{2/a}\sin(n\pi x/a)$ is the $n$th harmonic of a standing wave in a box of length $a$, which describes the changes of the field strength in the direction perpendicular to the axis of the waveguide. This is because the tangential component of **E** and the normal component of H, must vanish on the surface of the guide. The behavior of the $n$th harmonic of the electric and magnetic fields along the $z$-axis is given by the functions $V_n(z)$ and $I_n(z)$, respectively. They were so named deliberately in order that certain final expressions invoked associations with voltages and currents in power-network theory.

If these particular solutions of the form typical for radiation in the waveguide are substituted into Maxwell's equations, the problem reduces to a much simpler case of coupled differential equations of first order,

$$\frac{\partial V_n(z)}{\partial z} = i\kappa_n Z_n I_n(z), \quad \frac{\partial I_n(z)}{\partial z} = i\kappa_n \frac{1}{Z_n} V_n(z). \tag{4.6}$$

Here $Z_n$ and $\kappa_n$ are constants, expressible in terms of other constants in Eqn (4.5), and again deliberately named to look like the quantities used by electrical engineers to parameterize the characteristics of transmission lines, namely, the impedance and propagation constant, respectively. In vacuum, for these modes, which are in the class of TE (what Schwinger called H) modes, $Z_n$, and $\kappa_n$ are related by $Z_n = \omega/(\kappa_n c)$, where $\kappa_n = \sqrt{\omega^2/c^2 - (n^2\pi^2)/a^2}$. Written in this way, Maxwell's equations take the appearance of precisely the equations used by electrical engineers to relate the voltage and the current carried by a uniform transmission line.

The difference was, of course, in the wavelength; ordinary 60 Hz alternating current has a wavelength of 5000 kilometers and, over comparable distances, interference effects are significant and the engineers had methods of accounting for them in network design. Now, when it was legitimate to think about the electric component of the electromagnetic wave as voltage and about the magnetic component as the current of the microwave mode, much of the language barrier finally disappeared. There were also more powerful analogies. The obstacles along the path of propagation modify the propagation, but near the entrance or exit of the waveguide the wave again has the asymptotic form described by the above formulae. If one writes the initial and final 'currents' and 'voltages' as vectors,

$$\mathrm{I} = \begin{pmatrix} I_1 \\ I_2 \end{pmatrix}, \quad \mathrm{V} = \begin{pmatrix} V_1 \\ V_2 \end{pmatrix}, \tag{4.7}$$

then one can formally relate them by a matrix $\mathbf{Z}$,

$$\mathrm{V} = \mathbf{Z}\mathrm{I}. \tag{4.8}$$

One of Schwinger's earliest accomplishments, with Marcuvitz's help, in his new role at the MIT Radiation Laboratory was demonstrating that $\mathbf{Z}$ has all the properties that circuit theory requires of an impedance matrix of a four-terminal network. This work with Marcuvitz, which even used the engineering imaginary unit $\mathrm{j} = -\mathrm{i}$, finally appeared in print in 1951 [63], with the note 'although this paper has lain dormant for a number of years, the authors feel it is nevertheless of current interest.' Configurations of greater complexity were not conceptually different, but required an increased number of vector components and corresponded to larger networks. The impedance matrix provides information in a compact form about the discontinuities inside the waveguide, but how to find its elements? 'The point is, to put it as simply as possible, that one is interested in one number. What is the impedance of a certain complicated geometry? Now in principle, you could do it by solving Maxwell's equations and find all the fields and find this one number out. In practice, it is impossibly complicated.'[1]

Schwinger sought help where he could, mostly from quantum mechanics, from which he could adopt perturbative methods. For describing the scattering in the input–output engineering language, he developed a technique patterned directly after the Heisenberg–Wheeler scattering matrix. The scattering matrix was then a new and still relatively obscure theoretical concept. Wheeler had written his paper on it[25] when Schwinger was still a student at Columbia, but the crucial monumental articles by Heisenberg on 'Measurable quantities in the theory of elementary particies'[26] appeared in *Zeitschrift für Physik* in the hottest period of the war and could be immediately studied only by the most devoted and privileged few who could afford the time for academic studies and who had access to current German journals. Wolfgang Pauli had the good fortune of being a keeper of the traditional free research privileges throughout the war and traveled to share the news about the progress of physics with colleagues working for military enterprises. In the fall of 1944 Pauli visited MIT to lecture at the Radiation Laboratory on the latest developments in meson physics. He was then in the process of writing a book on *Meson theory of nuclear forces* which appeared shortly after the war.[27]

In his lectures, all of which Schwinger of course attended, Pauli gave a detailed account of Heisenberg's work. Pauli's lectures came at a very opportune time. The method of the scattering matrix could be easily generalized to describe the scattering of any kind of waves, not only the Schrödinger waves of quantum mechanics. A discontinuity in a waveguide actually generates a process that can be looked upon as scattering. An obstacle is hit on both sides by two traveling waves of different amplitudes. Part of each wave is reflected and part is transmitted; after the scattering the amplitudes on either side assume new values. If the amplitudes on either side before and after the scatering are represented by vectors

$$\boldsymbol{\alpha} = \begin{pmatrix} \alpha_1 \\ \alpha_2 \end{pmatrix} \text{ and } \boldsymbol{\beta} = \begin{pmatrix} \beta_1 \\ \beta_2 \end{pmatrix},$$

respectively, they can be linked by a linear transformation $\boldsymbol{\alpha} = \mathrm{S}\boldsymbol{\beta}$. The transformation matrix S, like the scattering matrix, is unitary. Schwinger found the analogy strikingly handy and used the concept of the scattering matrix for isolating those characteristics of microwave propagation through waveguides that are essentially independent of the detailed nature of discontinuities in the waveguides. 'Here you are trying to describe what is going on in a certain junction with various inputs from different waveguides. You send something in, something comes out. And there is the same question of how far can you go without knowing in detail what's inside. It was not the *S*-matrix theory as Heisenberg had developed, which would be to separate the *S*-matrix theory from everything else, but it was a computational tool. That was, of course, the amusing thing, because physicists naturally will talk about not so much the *S*-matrix but reflection and transmission amplitudes. The engineers will talk about impedance. And we had to go back and forth between these languages; so, in the end, I wound up finally talking about impedances rather than something else. It was so much easier to join their language than it was to change their methods. But it represented a rather interesting development of approximation techniques. This is the period in which I developed for practical purposes the variational method that I transferred to scattering theory.'[1] There was another reason for which it was better to use the engineering language of impedances; it made it much easier to combine the effects of multiple discontinuities. The scattering matrix method by its very nature essentially applied to individual obstacles.

The microwaves in the waveguides are not truly monochromatic, as we have noted before; the field excitations near the discontinuities are superpositions of many possible complex radiation modes, but they are localized in the immediate areas of posts, apertures, etc. They quickly weaken with distance so that effectively only one harmonic propagates. To Schwinger, the decaying higher modes of propagation resembled electrostatics, where, 'in a sense there are no modes of propagation, hence no propagation. So the higher modes which complicated everything would very rapidly become the same as they would in an electrostatic problem. So [there were] techniques like conformal mapping and all the things that were available, and then finding how to correct [the solution] so that it represented the solution of the electromagnetic problem. One had to get very ingenious to solve what were really very complicated things by every possible input you could make use of.'[1]

The best way of correcting the solutions was by using the variational technique. In fact, without it probably only very few solutions could have been found in time to be of any use before the end of the war. The other tool that Schwinger developed at the Radiation Laboratory was the modem theory of Green's functions as we know it today. These two tools, originally designed for the solution of problems in waveguides, later became something much more than tools; they became the foundation of the functional methods in quantum field theory, so widely used in fact that students often do not even associate them with Schwinger's name.

## The magic tools

In a lecture in Nottingham, England, on the occasion of the 200th anniversary of George Green's birth, under the title 'The Greening of quantum field theory: George and I' [229], Schwinger reminisced: 'Through those years in Cambridge (Massachusetts, that is), I gave a series of lectures on microwave propagation. A small percentage of them is preserved in a slim volume entitled *Discontinuities in Waveguides* [148]. The word propagation will have alerted you to the presence of George Green. Indeed, on pages 10 and 18 of the introduction there are applications of two different forms of Green's identity. Then, on the first page of Chapter 1, there is Green's function, symbolized by $G$. In the subsequent 138 pages the references to Green by name or symbol are more than 200 in number.' [229].

Green's identities relate volume integrals to integrals over the boundary surface of that volume and the so-called Green's functions were always wonderfully helpful in all kinds of electromagnetic applications. We have seen Green's theorem and Green's functions already in Eqn (4.2). However, the modern and most common interpretation of Green's functions was created, and first applications developed, by Schwinger, who turned them into a powerful mathematical tool which he used almost as a magic key throughout his later work. He introduced Green's functions as dyadic (later tensor) functional operators which establish a linear relation between a vector field inside a region of space and the boundary values of the vector field on the surface enclosing this region. The oldest record of this approach is contained in the 1943 *MIT Radiation Laboratory Report 43–44*. This subject was also included in Schwinger's lectures at the Radiation Lab, but David Saxon did not manage to write them up. In 1950, Levine and Schwinger included it [61], with a clear acknowledgment of Schwinger's sole authorship of *Report 43–44*.

Julian Schwinger made a lasting contribution to mathematical physics, much of it so fundamental and pervasive that it has become subsumed into the culture of physics, where nearly no one recognizes its origin. Thus a good deal of the technique presented in the classic treatise of Morse and Feshbach, *Mathematical methods of theoretical physics* (which includes a masterful chapter on Green's functions), benefited from his insights,* and, at the end of their Preface the authors state 'We are indebted to Professor J. Schwinger for many stimulating discussions and suggestions.'[29] Another classic text, written after the War at Harvard, Herbert Goldstein's *Classical mechanics*, expresses a similar sentiment.[30]

For the purpose at hand, which was solving the modified Maxwell's equations of the type (4.3) and (4.4), Schwinger defined Green's functions, $\boldsymbol{\Gamma}^1(\mathrm{r}, \mathrm{r}')$, for the electric field, and $\boldsymbol{\Gamma}^2(\mathrm{r}, \mathrm{r}')$, for the magnetic field, as operators corresponding to point electric and magnetic currents. Then the solutions for the electric and magnetic fields, respectively, can be written in the form

$$\mathbf{E}(\mathrm{r}) = \frac{4\pi i k}{c} \int \boldsymbol{\Gamma}^1(\mathrm{r}, \mathrm{r}') \cdot \mathrm{J}(\mathrm{r}')\, \mathrm{d}^3 r' \tag{4.9}$$

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* Feshbach stated that the sections on scattering, Green's functions, and iteration methods were largely based on what he learned from Schwinger.[28]

and

$$\mathrm{H}(\mathrm{r}) = \frac{4\pi ik}{c} \int \boldsymbol{\Gamma}^2(\mathrm{r}, \mathrm{r}') \cdot \mathrm{J}^*(\mathrm{r}')\, \mathrm{d}^3 r'. \tag{4.10}$$

Both solutions are required to satisfy the boundary condition that on the conducting surfaces the tangential components of the electric, and the normal component of the magnetic field vanish. The constants in front of the integrals in Eqns (4.9) and (4.10) reflect the harmonic $\mathrm{e}^{-ikct}$ time dependence assumed for the waveguide problem.

Once one had found the Green's functions for a given surface $S$, one could in principle find the fields corresponding to any boundary conditions on that surface. Skipping all the mathematical details that lead to them, we write the final expressions for the fields at a point r outside the conducting boundary $S$. They are given by the surface integrals

$$\mathrm{E}(\mathrm{r}) = -\int_S \left(\mathrm{n}' \times \mathrm{E}(\mathrm{r}')\right) \cdot \left(\nabla' \times \boldsymbol{\Gamma}^1(\mathrm{r}', \mathrm{r})\right) \mathrm{d}S' \tag{4.11}$$

and

$$\mathrm{H}(\mathrm{r}) = -ik \int_S \boldsymbol{\Gamma}^2(\mathrm{r}, \mathrm{r}') \cdot \left(\mathrm{n}' \times \mathrm{E}(\mathrm{r}')\right) \mathrm{d}S', \tag{4.12}$$

where $\mathbf{n}'$ is a unit vector normal to the surface and the values of all primed quantities are taken at the point of integration.

The horrifying part of the calculations involving discontinuities was that the boundary values of the fields on the obstacle and apertures within the waveguide were not known. In order to calculate the electromagnetic radiation emerging from a waveguide, one needed to know not only the radiation that entered the waveguide, but also the electric and magnetic currents on the obstacles and apertures within the waveguide, and these depended not only on geometry but also on the radiation input. Once again, even if Green's functions for a given geometry were known, so that the problem was 'well defined' by the Eqns (4.11) and (4.12), the solutions were not known. As in Bethe's calculation of diffraction, at one point or another trial-and-error methods were needed. Schwinger already knew how to turn trial-and-error methods into a systematic method based on the variational principle, which he had used before in the nuclear bound-state problems. 'And the whole idea was to find an expression in terms of the unknown fields for this impedance, which had the property of being stationary or, sometimes even better, either a minimum or a maximum when you found the right fields. Which meant that if you took a reasonable guess, you could come pretty close to the right answer, and it was a very powerful technique. It got developed and used and, of course, naturally had an implication back in terms of quantum mechanics and other things.'[1]

One beautiful feature of the variational method was that it treated two problems, which everybody thought needed to be solved in sequence, as one problem. There were integrals of the type of Eqns (4.9) and (4.10), which one could now rewrite in a slightly different notation, without making a distinction between the electric and magnetic quantities,

$$\Psi(\mathrm{r}) = \int_S G(\mathrm{r}, \mathrm{r}')K(\mathrm{r}')\,\mathrm{d}S', \tag{4.13}$$

which related the field $\Psi(\mathrm{r})$ to a distribution of its current $K(\mathrm{r}')$ given on the surface $S$. The notation in the above formula and the following equations is Schwinger's, as presented in one of the very first lectures at the Radiation Laboratory [148]. In Eqn (4.13) the Green's function $G(\mathrm{r}, \mathrm{r}')$ describes the process of propagation of the field from its source, which is the current. A very important point must now be emphasized. Without going into detail, it is enough to say that Schwinger rewrote the equations of the classical field theory in a form that later served him as a template for the future relativistic quantum field theory. Here the inspiration to solve the problem of the transmission of energy influenced the future development of the functional quantum field theory.

Ordinarily, in microwave applications neither $K$ nor $\Psi$ were known, but in a properly tuned waveguide $\Psi$ included just the dominant mode, and thus Eqn (4.13) could be treated as an integral equation for the current. The same input or output $\Psi$ resulted in different currents, depending on the geometry of the surface $S$.

In the calculations of the processes of steady transmission of power through the waveguide, typically one had to establish the relations not between the field and the currents at the boundary, but between the boundary conditions on the entrance and those at the exit from the waveguide. In other words, one had to calculate how the field propagates from one surface to another. Such calculations involved double integrals of the form

$$\int_{S'}\int_S K(\mathrm{r})G(\mathrm{r}, \mathrm{r}')K(\mathrm{r}')\,\mathrm{d}S\,\mathrm{d}S', \tag{4.14}$$

where the propagation function links the currents at the entry and exit surfaces, $S$ and $S'$, respectively.

In particular, the general form of expressions for the elements of the impedance (or admittance) matrix was [148]

$$X = \frac{\iint K(\mathrm{r})G(\mathrm{r}, \mathrm{r}')K(\mathrm{r}')\,\mathrm{d}S\,\mathrm{d}S'}{\left|\int \Psi(\mathrm{r})K(\mathrm{r})\,\mathrm{d}S\right|^2}. \tag{4.15}$$

In one of his early lectures Schwinger presented to the audience a proof that the function $K(\mathrm{r})$ about which the matrix (4.15) is stationary also satisfies the integral equation (4.13). Then he explained that, thanks to the stationary nature of this impedance expression, if one makes a small deviation in estimating currents or fields, the resulting error in the calculated impedance will be proportional to the square of that deviation. 'Thus if a field or current function is chosen judiciously, the variational principle can yield remarkably accurate results with relatively little labor. Unfortunately, the ability to choose good trial functions comes only with experience' [148].

The rest of Schwinger's published lectures are a record of how he was developing such experience. He started with relatively simple two-dimensional (in polar coordinates) Green's functions in infinite space that were useful in axially symmetric problems. He chose an application to a long cylindrical waveguide with a post running along the axis. The technique was to start with a known static solution corresponding to a constant electric current, and use it as a point of departure for the iterative variational procedure to obtain the solution to the dynamic problem with an arbitrary current distribution. Then he proceeded to the more general case of dielectric posts, metallic obstacles of zero thickness, and change of width of the waveguide.

In each case the solution was developed to a point that would satisfy the engineers. He started with a drawing explaining the geometry of the obstacle, and ended with a diagram of the equivalent circuit of a four-terminal (two input, two output) transmission network corresponding to these problems. Schwinger provided the general expressions for the elements of the impedance matrix (4.8) starting from the variational principle (4.15), but he converted them to an algorithmic form that involved sums of terms from the expansions in Fourier series and special functions appropriate for a given geometry. These were highly practical solutions, and the text of the lecture notes is full of reminders that they are approximate formulas derived on the assumptions that only one harmonic mode propagates, and that the waveguide geometry has a very high degree of symmetry. It was also noted that more complete and accurate results were given in the *Waveguide handbook*,[31] 'where the variational principle has been applied to higher approximations than those considered here.'

One night Schwinger made an unexpected discovery. He was working on a solution for a bifurcated guide in the shape of an infinite rectangular pipe divided by a semi-infinite conducting partition running along its length halfway between the walls. The propagation problem for such a waveguide was equivalent to a six-terminal network and mathematically it reduced to the Wiener–Hopf type of integral equations, that is, of the general form

$$\int_0^{\infty} K(z - z')\mathcal{E}(z')\,\mathrm{d}z' = \begin{cases} 0 & \text{for } z > 0, \\ H(z) & \text{for } z < 0. \end{cases} \tag{4.16}$$

We use the original notation of the lectures here, which requires some explanation. The waveguide extends along the $z$-axis. In Eqn (4.16), $H(z)$ is the discontinuity of the transverse magnetic field across the metallic partition inside the waveguide (positioned at $z < 0$), while $\mathcal{E}(z')$ is (apart from a constant factor) the longitudinal electric field along the continuation of that central plane, $z' > 0$. The kernel $K(z - z')$ is an expression (whose exact form can be omitted here) that involves the scalar Green's function for this problem. For this particular geometry, the Green's function was known exactly and its Fourier transform had a very simple form. Schwinger found it useful to take the Fourier transform of the entire equation and look for the solution in the complex domain.* Later on, with the progress in the theory of distributions and integral transforms, such techniques became common procedures involving other types of transforms, especially Laplace and Mellin transforms, but when Schwinger did this it was a non-standard method and he had numerous concerns, especially regarding the convergence of the solutions. However, the result was a breakthrough. 'I made a major discovery that for some simple geometries the problem could be solved exactly, and this was my technique of Fourier transforms of integral equations, which was an extension of the Wiener–Hopf technique; they had applied it only to things that decayed exponentially at large distances, whereas here one was applying it to waves and it was not clear that the technique still applied. I monkeyed around a little bit and found a way of doing it. Of course, there were very few geometries for which you could do something exactly; one example could let you test things for which there were only approximation methods before and see how valid things are. It was really quite exciting. In fact, when I finally realized I could do this, I well remember leaving on the desk of my friend, this fellow Marcuvitz, a little piece of paper [with the message] "a new age dawns" and I went home that night.† To solve something exactly was considered beyond possibility, and yet it had been done.'[1] The solution is given in the last chapter of [148]; in this case the notes were taken not by David Saxon but by A. E. Heins.

On more formal problems, Schwinger worked with Harold Levine who, being a physicist, was more interested in analytical methods than was Nathan Marcuvitz. At the Radiation Lab, Levine became interested in the problems of diffraction, and after the war he continued his involvement in this subject. He followed Schwinger as his assistant at Harvard, and thanks to his continued interest in diffraction their wartime contributions in this field were finally finished and published, with several years' delay, between 1947 and 1950, in a series of carefully crafted articles which were saturated with the mathematics of

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* This technique is similar to that given by Schwinger for Sommerfeld's problem of diffraction by a straight edge [231].

† Marcuvitz remembered the message as 'a new era has dawned.'[24]

analytic and special functions [41, 46, 48, 54, 55, 61, 71]. Levine was the writer of these papers, and therefore they contain only that portion of the total body of diffraction problems (on which Schwinger worked) in which Levine was in some way involved; Schwinger worked on many more fascinating diffraction problems which he did not publish.[1,*] The earliest of the Levine–Schwinger papers [41], preceded by a brief paper contributed to the APS meeting [39], was submitted to the *Physical Review* in October 1947. Schwinger described this work as follows: 'I published a paper "On the radiation of sound from an unflanged pipe" with Harold Levine. This was the Rayleigh problem. (By the way, among the gods we must include Rayleigh: Maxwell, Einstein, Dirac, Rayleigh. Among the classical greats were Galileo and Newton, they are sort of icons; throw in Leonardo da Vinci in the prehistory of science.) Now the Rayleigh problem was this. If you have a circular pipe and a sound wave comes out—it's an open pipe, so it's the reflected wave. The wave that is reflected inside the pipe looks as though it originates not from the end of the pipe. Rayleigh had invented an approximate theory of that. We, that is myself and Levine, had in our possession a technique for the exact solution of this problem, because it was of the class of exactly solvable problems which had been worked out for waveguides. But it was a sound waveguide, rather than an electromagnetic one, so we solved it exactly and ended up with an integral that had to be worked out numerically, but we discovered that Rayleigh, to the accuracy of his approximations, had done pretty well. We were very pleased with Lord Rayleigh.'[1] Levine and Schwinger found the rigorous value for the effective lengthening of the organ pipe due to diffraction that causes the antinode of the dominant harmonic mode to form beyond, rather than at the opening of the pipe. This value was 0.6133 of the pipe's radius. Rayleigh, despite using a somewhat simpler case of a pipe flanged at the opening by an infinite plane, could not obtain a specific number for the answer. His empirical estimate was 0.6 of the radius, better than those of following workers.

'This problem was important for the following reasons. First, we were thoroughly familiar with Rayleigh's book[32] because we were interested in approximation limits, so you look for approximation methods everywhere and acoustics was the natural place. So, for instance, Rayleigh had this famous Rayleigh's principle, which is a variational principle, and he had found these approximate

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* Levine regarded it as a pity that Schwinger did not publish more analysis of an applied mathematical nature, such as on the theory of aerodynamic noise. Because Schwinger was completely preoccupied with his work on nuclear physics and field theory, Levine wrote the joint papers, and was disappointed that Schwinger was not more involved in the work during the seven or eight years Levine was at Harvard before going to Stanford.[23]

techniques for dealing with waveguides. Of course we were familiar with them. And since the topology of an open-ended non-flanged pipe is just the topology for which we had this mathematical technique, it was just natural to say: "Why don't we solve it exactly?" And so we did. It was sheer entertainment. This problem was left over from the Radiation Lab days, doing something that Rayleigh couldn't do very well. We were very proud of it.'[1]

Except for their last paper, the remaining Levine–Schwinger papers were on the theory of diffraction by an aperture in an infinite plane screen, which in a sense was a return to the starting point of the calculation that Bethe had presented to the group of young physicists at Purdue.[20] However, the articles do not refer to Bethe's calculation. By the time they were written, Levine and Schwinger learned that during the war a Dutch doctoral student, C. J. Bouwkamp, had worked at the University of Groningen on his dissertation, whose title in English was *Theoretical and numerical calculation of diffraction by a circular aperture*, which was completed in 1941. Even though it would have been extremely useful to the waveguide group at the Radiation Lab, nobody in the United States knew about its existence. Bouwkamp's analysis was exact; although the final solution had a form of an infinite series of waves diverging from the aperture, it involved no approximations other than the assumption that the field possesses rotational symmetry due to strictly normal incidence of waves. For such symmetry, he could use oblate spheroidal coordinates in which the wave equation becomes separable. While Bethe's result applied only to very small apertures consistent with the assumption that the field is constant over the hole, Bouwkamp's solution worked for all radii, except that the series converged more slowly for larger apertures.

Levine and Schwinger's approach was based on the variational technique. First formulated for scalar waves [46, 54], it was subsequently formally generalized to electromagnetic waves [61]. The technique was similar to what we have outlined above except that in order to maintain full generality for accommodating the apertures of fully arbitrary shape, Levine and Schwinger worked in three-dimensional space, writing the integral equations for the scattered wave with the help of three-dimensional Green's functions. The boundary values in the aperture were of course unknown, and the solution to the integral equation in general impossible to find, but well suited to the variational method. The technique was to consider two parts of the solution separately (those symmetric and those antisymmetric on either side of the aperture) and then assume that both parts of the solution were stationary with respect to small variations in their values in the immediate proximity of the aperture. On the screen the solution had to vanish, with a discontinuous derivative in the direction normal to the surface. The second variational requirement assumed stationarity of the solution with respect to the variation of that derivative.

In order to test the method, Levine and Schwinger applied it to the case of a circular aperture in order to compare their results with those of Bouwkamp. The agreement appeared to be good but not perfect, although later it turned out that numerical errors had sneaked into both the Levine–Schwinger and Bouwkamp calculations of the transmission coefficients of the aperture. The transmission coefficient is a constant describing what portion of the wave illuminating the aperture actually passes through; in the limit of small wavelength or large aperture, it approaches a value equal to 1. After reading the Levine–Schwinger paper [46], Bouwkamp returned to his older calculation and found an error in it. He recalculated the transmission coefficient using Schwinger's variational principle, but different trial functions, and found that it reproduced the correct figure exactly. Thinking that he had improved the procedure, he communicated it in a letter to the *Physical Review*,[33] with the remark that Schwinger's first approximation was closer to the correct value than the second. This, in turn, alerted Schwinger who was confident that the variational method must yield progressively better results. The calculation was redone, the small numerical error in the Levine–Schwinger paper found and the corrected figures published next to Bouwkamp's letter [55].

The final Levine–Schwinger publication was an abstract entitled 'Radiation force and torque' [71], representing a paper presented (presumably by Levine) at the 1952 Washington APS meeting. They pointed out that the scattering of waves by an object implies a force and torque upon that object. For the case of a rigid disk, they gave a formula relating the torque on the disk to the derivative of the scattering amplitude in the forward scattering direction.

## Toward the peace

The theory of waveguides remained Schwinger's chief official preoccupation until 1944. By 1944, the intensity of theoretical research on microwave theory and applications would crest, the majority of fundamental problems would be resolved, most of the technological goals accomplished, and further work on radar could be comfortably handled by the engineering wing of the Radiation Laboratory. From the military point of view, by 1944 the Allies had won essentially full command of the air and the seas and the mission of the Laboratory had been fulfilled. The war was not over yet, and large numbers of personnel from the Radiation Laboratory were transferred to other projects, almost invariably connected with the atomic bomb program.

Schwinger remained at MIT, but in the new situation he could afford to pursue progressively more research that was unrelated to his official line of duties. Now that the experience of working on waveguides had paid back handsomely, Schwinger just could not resist the opportunity of putting the refined variational technique and the newly learned scattering matrix theory to work in the study of nuclear collisions. Throughout the entire three years spent at the Radiation Lab, he never quit working on physics. Judging from the number of articles published in the *Physical Review* that appeared shortly after Schwinger left the Radiation Laboratory, he had led there an immensely productive life.

First he obtained some interesting results on the polarization of fast neutrons through scattering on light nuclei. Schwinger announced the results of this wartime work only at the Cambridge APS meeting in 1946 [34]. Two years later he also published a short follow-up article in which he proposed an alternative mechanism of neutron polarization [42]. He retained a warm sentiment for these two short papers, which were the first ever published on the physics of fast neutrons, and included them in the collection of his selected papers.[9] They had some conceptual link with Schwinger's early undergraduate work on Bloch double-scattering on polarized ferromagnetic plates [3], described in Chapter 2.

By 1943 the mechanism of polarization of slow neutrons by purely magnetic scattering was already well understood, in part thanks to Schwinger's contributions, but it worked only for slow neutrons which stay in the magnetic field long enough to be affected by it. No equivalent process had yet been found for fast neutrons. Then, at the Radiation Lab, it occurred to Schwinger that polarization might also result from spin–orbit coupling. Strong effective coupling of this kind is created when neutrons are scattered off nuclear resonance levels. Schwinger had worked out in considerable detail, with Gerjuoy at Berkeley, the wavefunctions of the ground states and excited energy levels of $^3$H and $^4$He nuclei. He was also acquainted with the experimental data on neutron scattering and knew about the anomaly in the scattering cross-section on helium at the energy of approximately 1 MeV. He thought this anomaly could possibly be attributed to a resonance produced by the capture of a neutron by $^4$He, which led to the momentary creation of a highly unstable $^5$He nucleus.

This problem just presented itself as waiting to be solved, and of course it would be a pleasant diversion from the work on radar. 'I was mindful of the importance of these developing techniques to problems of more immediate physics. I was also thinking of more physical problems. For example, surely during this period was the application of the polarization of neutrons by resonance scattering in helium. It was a natural line of development. I was putting together a lot of things. The polarization of slow neutrons in magnetic materials was a very early thing I did. How do you polarize fast neutrons? How about using scattering, because I think it was known that there was a strong spin–orbit coupling in helium?'[1]

This calculation was another example of Schwinger's talents in phenomenological analysis. First he noticed that the energy-dependence of the back-scattering cross section indicated that the $P$-resonance most likely splits into two levels, of angular momentum $\frac{1}{2}$ and $\frac{3}{2}$, and then made an estimate of the magnitude of the energy gap between them. The energy transfer and the scattering angle are kinematically related, therefore neutrons scattered off different resonance levels would not only have different spins but also different angular distributions. Schwinger analyzed those distributions for single- and double-scattering, and discovered that they could provide a feasible mechanism for polarizing fast neutrons. The scattering was so dependent on the spin state of the neutron that for certain scattering angles it would even yield the ratios of numbers of neutrons with the two different spin orientations as high as ten to one [34]. The second paper [42] proposed 'polarization by the spin–orbit interaction arising from the motion of the neutron magnetic moment in the nuclear Coulomb field.' Nearly 100% polarization could be achieved for small angle scattering of 1 MeV neutrons on Pb.

Rabi was concerned about the physicists returning to a fundamental research environment, so he started a series of biweekly seminars by Pauli, who came up from the Institute of Advanced Study in Princeton. The first lecture was crowded, but attendance plummeted as few were able to follow the highly technical presentations. (Of course, Schwinger, as we described above, benefited from Pauli's lectures.) Edward Purcell recalled that 'Rabi had the great idea to get Julian Schwinger to lecture in the weeks in between' Pauli's lectures. His lectures were marvelous. 'He reviewed the recent developments, where things had gone, what were the puzzles. Gradually the attendance at Julian's lectures went up and Pauli's went down.'[34]

During his last year at the Radiation Lab Schwinger also tried to compose a treatise entitled *The theory of wave guides*, but he 'reluctantly' abandoned it and went back to physics.* 'This was the first of my several books on electromagnetism, none of which were ever published.† But the point is that what was published was the so-called *Waveguide handbook*,[31] which was not theory but a collection of final results for engineers to use, all of which had been produced either by me directly or someone working in combination with me, using my techniques. It's a large mass of material indicating how much we did.'[1]

The initiative for writing books and monographs by people at the Radiation Lab came from Rabi, who understood the tremendous intellectual potential temporarily concentrated at the Lab. He feared that, as the end of the war would scatter the precious staff of the Radiation Laboratory, the accumulated

* Chapters 1 and 2 exist in mimeographed form in the Schwinger archive.[3] Titles of sections include 'Waveguides and equivalent transmission lines' and 'The theory of guided waves and impedance in wave guides.'

† However, this is no longer true. See [231].

knowledge would disperse together with the people who created it. He did not want to permit this to happen, and insisted that everything be put in writing and collected in book form. Louis Nicot Ridenour took charge of coordinating contacts with many writers and taking care of the monumental task of editing. It took 27 volumes to cover the major aspects of the new discoveries and inventions in microwave physics and engineering. They were eventually published over the period from 1947 to 1951 as the *MIT Radiation Laboratory Series* by McGraw-Hill. Nathan Marcuvitz finally edited the volume on waveguides.[31] It appeared in 1951, at a time when Schwinger had lost interest in applied classical electromagnetism.

Towards the end of his stay at MIT, Schwinger also developed the theory of synchrotron radiation in which he collaborated at first with David Saxon. He picked up the subject as a practical one, needed for the design of particle accelerators. This was a subject, work on which continued also after the war, which we shall discuss in the next chapter. In 1945 Schwinger gave several seminars on synchrotron radiation and on waveguides, including talks at Los Alamos, where he visited for the first time in the summer of 1945. 'I took a trip to Los Alamos. I forgot really why I went there. But I remember that Jerrold Zacharias, from Rabi's group, came around in July and said that he was flying to Los Alamos and thought that I would find it interesting to go along as well, so I went . . . .

'I remember flying continuously in a small plane for at least 24 hours. I was exhausted. Then, upon arrival, I was told that I had to give my talk on synchrotron radiation. I can't imagine how I ever got through it. So I must have been very impressed. The arranging of the talk was not up to me; after all I was a satellite of Jerrold Zacharias.

'I got there the week after the Trinity test and everybody was euphoric. Then I began to get some idea of it. I do remember coming back with a sample of what then was called Trinitite, a fused rock. Of course, nobody bothered to tell me that it was radioactive and I carried it around for quite a while. That was dangerous. I don't know what that trip did. While I was fascinated with New Mexico, I don't think it changed my opinion as to the correctness of what I had done during the war years.'[1]

One of the young physicists at Los Alamos was Roy Glauber, who worked in the theory division under Chaim Richman, who we recall had had an unfortunate interaction with Schwinger at Berkeley several years earlier; Glauber believed that Richman had done a thesis with Schwinger there on the deuteron, but if so, it was only in some nominal sense. In any event Richman, and Rarita, who was in the next office, spoke highly of Schwinger, although neither were strong theorists. When Schwinger arrived in Los Alamos in July 1945, Rarita insisted that Glauber must meet 'Julie,' so after lunch in Fuller Lodge at 1:30 p.m.

they went to the Big House where Schwinger was staying. Not surprisingly, Schwinger was embarrassed by the visit for he was still in his pajamas.

A day later Glauber went to hear Schwinger's talk. 'It was the most elegant talk I had ever heard—an hour and a half of the kind of intensity and perfection that absolutely bowled me over.' Schwinger's talk on synchrotron radiation and the microtron, which we shall describe below, convinced Glauber that Schwinger was the man he should work under for his PhD. He knew Feynman and Bethe from Los Alamos, and he knew that he couldn't learn anything from Feynman. So in January 1946 he returned to his undergraduate studies at Harvard, and would soon become one of Schwinger's first graduate students.[35]

At Los Alamos, Oppenheimer evidently did not hold a grudge against Schwinger for not joining the Manhattan Project and demonstrated to his younger colleague his continued friendship and respect in his usual way. 'I have a distinct memory of Oppenheimer pouring several of his potent Martinis, and that's about all I recall: getting very drunk. That was my special case; because I had never had such high potency liquor at such high altitude before. But I had a thoroughly good time, yes. In fact, I have a distinct memory that this was probably the only place in which I would finally go to bed and eight hours later wake up with no memory of anything in between, and it's strange because I felt good. I don't know why. I stayed at Los Alamos a week or ten days, maybe two weeks. I gave a number of lectures. So I lectured on the theory of waveguides and synchrotron radiation. It was all my electromagnetic experience being given to them.'[1]

The notes of Schwinger's lectures at Los Alamos in 1945 have survived and are preserved in the Schwinger archive at UCLA.[3] He lectured on the radiation produced by electrons accelerated in a betatron, on waveguides and aspects of waveguide junctions, on the 'microtron,' and on the excitation of a microwave cavity by an electron entering it. For betatron radiation, where the acceleration is produced by a magnetic field which increases sinusoidally to its maximum value, he displayed the formula for the radiated energy

$$\frac{E_{\text{rad}}}{E} = \frac{1}{4}\omega T \frac{e^2/mc^2}{R} \left(\frac{E}{mc^2}\right)^3, \tag{4.17}$$

for an electron of charge $e$ and mass $m$, moving in an orbit of radius $R$, $E$ and $\omega$ being the final energy and orbital frequency of the electron and $T$ the time required to build up the field. Schwinger concluded this lecture by stating: 'I shall be most anxious to learn whether these predictions are substantiated by experiment.' The waveguide lectures were a practical summary of what had been learned at the Radiation Laboratory. He also determined the excitation of a microwave cavity by successive bunches of electrons entering that cavity—even for a single electron in the bunch the small power produced ($2 \times 10^{-13}$ W for reasonable parameters) 'is probably detectable.'

The most provocative part of these lectures was 'Schwinger's idea for an accelerator,' as the notetaker called it. It took two possible forms: either a linear accelerator consisting of a succession of microwave cavities, or a single microwave cavity with a magnetic field 'recycling' the electrons through the same cavity. He gave tables of the microwave power required versus the resulting accelerating voltage, and concluded that to achieve an acceleration of $10^8$ eV about 100 cavities were required. For the circular scheme, it was envisaged that the accelerating cavity would occupy only a small part of the electron orbit. These schemes were dubbed by Schwinger the 'microtron,' not because it had a small size but for its use of microwaves. Of course, both species of accelerator were elaborated by Alvarez and others, and are now commonplace in high-energy physics. The two-mile long accelerator at the Stanford Linear Accelerator Center (SLAC) and one-mile diameter proton synchrotron at the Fermi National Accelerator Laboratory (Fermilab) are decades-old examples of this principle, as is the Large Hadron Collider (LHC) being built at CERN outside Geneva.

In the audience at these talks was another young physicist whose career path was just about to intersect with Schwinger's. 'I met Feynman on that occasion. To my knowledge, this was our first encounter. I don't have the memories associated with his scientific abilities; but I do recall that he looked at me and said, "Gee, you already have a number of papers published," or something of that sort. Of course, he had published one thing.[36] I didn't quite know how to take that at the time. He wasn't being jealous!'[1]

Feynman was not jealous, but he was very impressed by a man just three months older than himself who was an accomplished expert in several fields and whose list of publications bulged with 32 papers in the *Physical Review*! If he was jealous of anything, it was not of Schwinger's accomplishments but of his opportunities, especially Columbia. Feynman held a grudge against Columbia University for not having accepted him as a student. He had applied for admission in 1935 and, like anybody else, he was required to send in an entrance examination fee of $15. He knew that this deposit would not be refunded if he were not accepted. Finally he forfeited his fee. He could afford to lose the money, but deep inside this loss of the $15 payment became something of a symbol of his failure. He always felt that Columbia had cheated him, and suspected that it had something to do with Jewish admission quotas. Instead of Columbia, Feynman went to MIT, and recalled that if he had gone to Columbia he would have had an opportunity to become an expert theoretician at a much earlier time of his life.[18] Schwinger concurred. 'That would have been interesting, if we had been at the same place at the same time. That would have changed things.'[1]

The fact that Schwinger made a trip to Los Alamos together with Zacharias was probably not a coincidence. About that time, John Slater approached Schwinger on behalf of MIT to recruit him for a position there after the war was over. Shortly earlier Jerrold Zacharias had made up his mind about staying on at MIT, and the invitation to join him on the trip may have been an attempt to entice Schwinger to accept the offer. Schwinger did not either turn down or immediately accept the offer from MIT, and for a while he seriously contemplated it as an attractive proposition and 'a very strong possibility.'[1] He did not have to rush with the decision, but it became clear to him that this was high time to think seriously about the future.

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# Winding up at the Radiation Lab, going to Harvard, and marriage

## Enter Clarice Carrol, the future Mrs. Julian Schwinger

Throughout the period spent at the Radiation Laboratory, Schwinger did not socialize much beyond the immediate circle of his co-workers and old friends from Purdue or Columbia, and how could he? He was a social newcomer in a busy and sophisticated city, which could make anybody disappear, not only an overworked young man of eccentric nocturnal habits who resided at a place with the Kafkaesque name of 'Hotel for Refined Gentlemen.' He endured much teasing on that score, but it was convenient, and he was lucky to have found it.[1]

Even at this stage of life Schwinger had not become an independent professional person. Physics was the only exception. People knew him as a fastidious dresser at that time, but nobody knew that it was his mother who kept him properly attired. When he ran out of clothes, he would just set off for a short trip to New York. There, his mother would take him in hand and with a discerning eye create a complete wardrobe that would keep him suitably attired for the coming year.[1, 2]

In this period of his life, Schwinger's closest friend was Nathan Marcuvitz (popularly nicknamed Mark), an engineer in Edward Purcell's group at the Radiation Laboratory. Marcuvitz was concerned with Schwinger's solitary ways and now and then tried to help him meet a girl or arrange a date for him. Sometime in 1944 Marcuvitz met Clarice Carrol and decided she and Schwinger should meet (he preferred blondes). She was reluctant, but Marcuvitz insisted. 'You really ought to meet Julian; he is such a brilliant physicist.' That was hardly an attraction to Clarice. She worried what she would talk about with a physicist; she had never met one and knew no physics. She needn't have worried, for physics never entered their conversation.[1, 2]

On their first date they went dancing with Mark and his friend. The second, some weeks later, was probably their only daytime date; 3:30 in the afternoon seemed a reasonable time to Clarice, but she had no idea of the compliment being paid her. He usually got up at 4:30, and to get to her house at 3:30 he went without breakfast. They had a lovely walk in a park. Then Schwinger suggested that they have dinner in town. She agreed, but said that if they went into Boston she would have to change her clothes, expecting him to say that she looked just fine as she was. Instead, he said 'yes,' and she did indeed change into a silk dress and high heels. She had learned an important clue about Schwinger: he liked elegance.[1]

Because he had missed his breakfast, he was starving for his first meal of the day at 10 o'clock that night. On the other hand Clarice had had a big Sunday dinner, so she did not eat anything. After that first encounter, they saw each other sporadically. The relationship grew slowly and quietly.[2]

Clarice was born on 23 September 1917, six months before Julian. Their parents shared similar backgrounds. Both of Clarice's parents were Russian Jewish emigrés who arrived in the United States as teenagers. Her mother's family settled in New York City before relocating in Boston. Clarice's grandparents left Russia because her great grandmother had been shot during a pogrom, and her grandmother said she had to go to the United States even if she had to wash floors. But faced with the reality of a cold-water flat in New York, she wanted to go back. She would not allow her brilliant daughter Sadie to continue her education in New York because she was afraid that if Sadie went to school she would not want to return to Russia. All for naught, for Sadie fell in love with the United States when she first set foot on American soil. Throughout her life she possessed beauty, warmth, and gracious gentility. Sadie finally earned her high school diploma in her middle forties.

Clarice's father Abraham was a handsome blue-eyed adventurous strawberry blond who might have become a dancer if he hadn't become a successful construction contractor. He arrived alone in the United States when he was barely 17, found work, saved his money, and in time brought six of his eight brothers and sisters to America. He met Sadie in Boston and married her there (around 1910), had two sons and a daughter, and lived happily until the Depression devastated the construction industry. There was no period of recovery for the Carrols. Abraham died in 1935. Clarice had a happy, sheltered childhood, and enjoyed being the youngest child with two older brothers.* Four of her friends and she began high school at Girls' Latin School; by year's end, all five had left. She graduated, instead, from Dorchester High School for Girls. After filing

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* Jack went to Cincinnati and Charles, after the war, went to Parkersburg, West Virginia, because Sadie's only brother had a shoe factory there which he intended to leave to Charles. So he gave up dreams of going to dental school. There was no money; he became a school teacher.[2]

an application and having an interview, she thought she would be attending Katherine Gibbs School, a well-known business school in Boston. But some weeks later she received a letter from the school informing her that they were sorry, but their Jewish quota was filled. She went to Chandler Secretarial School instead, which she thought of as not so prestigious, but good enough. Her first job was with a criminal lawyer who, when he felt the dictation was too unsavory, would give it to the secretary next door. She left at the beginning of World War II to work as the lowliest of file clerks at the Army Base in Boston at twice her civilian salary. She suffered no physical discomfort, but experienced emotional traumas. She kept busy with her job, and volunteered as a nurse's aide at Boston City Hospital three evenings a week and every other Sunday morning. Her work at the War Production Board with a staff of 'one dollar a year men' (executives who volunteered their knowledge and skills to the government for the token salary of one dollar a year) was interesting; her spare time was filled with her hospital volunteer work and some social life.[1]

It was a very busy time, but Clarice's mother once again kept things going. She stood in lines with the ration coupons and got the dinner ready, so that when Clarice came home she was fresh and clean, and cheery and smiling, with no apron, as though she had done nothing all day except read a book and make dinner. She made a wonderful home for Clarice. They were still living in the same large house they had when her father was alive, and had to maintain it without the gardener, laundress, or housekeeper. But Sadie managed it all with grace and charm.[2]

Throughout the first year of their acquaintance, Clarice and Julian were too busy to go out often; however, towards the end of the war they were able to see each other more frequently, although almost never during the day. Typically, they would meet late in the afternoon, after Schwinger got up for breakfast. They would spend some time together and then he would head towards the Radiation Lab, because he went religiously to work every night. They did not take automobile trips or outings because of gas rationing. For fun they went to the theater. Clarice was surprised that Julian liked to go to nightclubs, especially jazz ones, and he liked dancing. They used to go to Lockover's for dinner. Schwinger was still in his steak and chocolate ice cream phase; he would urge Clarice to take something exotic so he could taste it. He enjoyed music and they went to the Symphony.[2] (Later, Schwinger took piano lessons and treated them very seriously. He learned to play fairly well, but he never achieved a level close to artistic proficiency.) He liked to fantasize that at 40, he would quit physics and become a composer.[3] But most of the time he was working, and he worked all night. Clarice and Julian did not get together very much during the day; it wouldn't fit with his schedule. A rare exception was when they once went canoeing in Norumbega Park with Mark and his date.[2]

Clarice did not feel swept off her feet. Their love grew slowly. Schwinger was very different from anybody else Clarice had ever met. He was very sweet to her. She liked his 'marvelous eyes, with the sweetness behind them' and his caring about people. Clarice recognized that he really cared about a lot of things. He was interested in the world and in people. Clarice found him nice to be with. He was a very good conversationalist, in spite of his being a very quiet young man. She found that if she initiated the conversation, he would talk about anything, but he was never one to start the conversation.

Clarice's mother worried because she thought that he was going to be too strong for Clarice. Although he was very quiet there was never any doubt that he knew exactly what he wanted and what he was going to do; because of his intense passion for his work anything else would come second. Sadie liked him almost immediately. He was certainly one of the most unusual characters Clarice brought home and they enjoyed each other's company.[2]

On one occasion, Clarice went to one of Schwinger's lectures at the Radiation Lab. The subject of the seminar may have been on synchrotron radiation. Watching Julian in action was a revelation to Clarice, who never discussed and, as a matter of fact, did not know much about science at that time. Her mother, whom Clarice regarded as much more intellectual than she, thought it was dreadful that Clarice should be seeing a man and not have any understanding of what he was doing. Clarice did not know what a physicist was and had no appreciation of his abilities or what he was like as a scientist until she heard him talk.

How this came about was as follows. One day Marcuvitz said that Schwinger was going to give a talk that afternoon and that Clarice might find it interesting. So she took time off from work and went to MIT and sat in the back and was amazed to see Schwinger, with whom Clarice felt she had possibly exchanged three words, put one foot over the threshold and began to talk. He didn't stop talking for an hour and a half; Clarice couldn't believe it. He was not the same person Clarice knew in private. Schwinger was aware of the fact that Clarice was in attendance and accosted her afterwards, saying that she didn't give him her full attention. Clarice thought that was very funny; all she had done was to look up and down the row, since she was in the back, to see how many people needed haircuts. That was the only time Clarice did not look at him, but although he hadn't known she was coming, he was perfectly aware of her presence and knew that she didn't look at him the whole time, which Clarice found extraordinary![2]

## Synchrotron radiation

Schwinger's last project at the Radiation Laboratory was on the stability of orbits and the electromagnetic radiation of accelerated charges in the new higher energy accelerators then being designed. It originally arose from a practical aspect in the design of cyclotrons and their successors related to the energy loss of a circulating charged particle, but in Schwinger's hands the subject quickly transformed itself into an important piece of theoretical classical physics. Until the 1940s the maximum energies achievable in cyclotrons were limited by the constancy of the frequency of changes of polarity of the accelerating plates. As the energies of accelerated particles increased to relativistic values, the period of a full cycle of a particle within the accelerator changed, and the frequency of the accelerating electrostatic field had to be synchronously adjusted. With the new breed of high-power microwave-frequency devices such adjustments could be accomplished with much greater accuracy than one could think of achieving before. It made the construction of synchrocyclotrons and then synchrotrons (as the new breed of machines was later named), feasible. (A related design was called the betatron, because it was used to accelerate electrons, what were called beta-rays in the early days. In this case, the acceleration was provided by a changing magnetic field, which gave rise to an accelerating electric field on the electron.) Towards the end of the war work began on the design of the first 70 MeV electron synchrotron, which was about to be built by General Electric. It was a small machine by today's standards, operating on an orbit of 29.2 cm radius. The first question had to do with the stability of the electron orbits. For Schwinger, it was an interesting problem. 'I have always been concerned with contact with experiment. In the paper on "Electron orbits in the synchrotron" with David Saxon [35], we were simply swept up with the problem of constructing the synchrotron and what problems there might be that the simple-minded original inventors had overlooked in the instabilities and vibrations and that kind of stuff. But we did just a little work of our own. This problem was not necessary for the Radiation Lab, but it was important physics. . . . Synchrotrons were obviously important because everybody who had been a nuclear physicist wanted to get high-energy machines; that was the direction in which to go and all these people were thinking about it, but we were theorists and we would pick up just little bits here and there. It was the experimenters by and large who came up with the general ideas, and we concerned ourselves with whether there were any loopholes in their reasoning that would prevent things from working. And, of course, there were. I don't think we did anything very important.'[3]

As Saxon recalled, '[Schwinger] and I worked together on a paper on the stability of synchrotron orbits. We both knew exactly the same about the problem, namely zero. That's one of the times I really had the rare privilege of seeing him start from absolute scratch. We struggled the first night. The question was how to do it—we worked all through the night trying to figure it out. The next day I came in and said, "I think I know how to do it!" and Julian said, "So do I." He had the complete theory. I knew how to do it, but he had done it. The amusing thing about the story is that people thought the orbit was unstable, because they had run it on a computer, actually a differential analyzer, and there were instabilities in the formulation. But Julian and I demonstrated that it was stable.'[4]

The above-mentioned paper with Saxon [35] was actually an abstract of a contributed talk at an APS meeting in Cambridge in the spring of 1946, another example of a characteristic Schwinger 'patent application' with some results, but devoid of any technical detail. A full paper on the subject by Schwinger, in fact, never appeared. The classical calculations of the energy loss by an accelerating charge become considerably simplified for charges at rest. Therefore Schwinger found a better method to carry out the calculations. He made a transformation to the rest frame of the charge, replacing the accelerating electromagnetic field by an equivalent rotating field. Already trained in simplifying complex electrodynamical problems, Schwinger focused not on the trajectories of circulating electrons, but on the equations that described the deviations of trajectories from the equilibrium orbits. Then he was able to rewrite the equations of motion as differential equations for the phase shift with respect to the phase of the rotating electric field. These differential equations were still of the fourth order, as demanded by the classical theory of retarded potentials which described the radiation reaction, but the calculations became simplified, and included the effects of the changing magnetic fields and of radiation losses.

In electrodynamics, a transformation from the rest frame to a moving frame of reference mixes the components of the vector magnetic and scalar electric potential and generally leads to complications which Schwinger wished to avoid. He found an easier way out by noticing that a relativistic calculation was unnecessary if it were possible to replace nonrelativistic formulas by equivalent expressions that were relativistic invariants. The trick worked and, with Saxon's help, Schwinger solved the phase equations and discussed several aspects of electron orbits, especially those related to the critical stage of the operation of an accelerator when its action was switched from a conventional betatron to a synchrotron mode.

Schwinger immediately realized that the key issue was the radiation emitted by the accelerated electron. In this he was apparently inspired by correspondence with Marcel Schein, a well-known cosmic-ray physicist at Chicago. Schwinger had promised him to investigate the question of whether betatron radiation occurs at all—Schein apparently was of the opinion that no radiation would occur, because there was a steady current flowing in the accelerator, that is, because of destructive interference between the different, but presumably coherent, electrons. In a letter to Schein dated 14 July 1945, Schwinger reported that he found no interference, that the electrons radiate separately, and in the infrared, not the microwave region as expected.[5]

Schwinger wrote a complete paper on the subject at the time, 'On radiation of electrons in a betatron' [32a], but never submitted it to a journal, showing it only to a few selected colleagues. He also gave various seminars on the subject, including those at Los Alamos in the summer of 1945. He did not submit a full paper to *Physical Review* until 1949 [56]. That paper was substantially different from the 1945 paper, although all the key results were already present in the earlier paper. The opening paragraph of the unpublished paper reads: 'It has recently been pointed out[6] that the radiative loss of energy produced by the accelerated motion of an electron in an induction accelerator, or betatron, sets a theoretical upper limit to the energy obtainable by such a device. However, the idea appears to be prevalent that this calculation for a single electron does not apply to an actual betatron when many electrons are present simultaneously, for, it is argued, the latter situation corresponds to a steady current which, of course, does not radiate. Otherwise expressed, the fields emitted by the electrons at various points of the circular path interfere destructively and thus suppress the radiation. The same objection to the individual action of the electron is raised, with opposite effect, concerning the radiative loss of energy by a "pulse" of electrons, which travel together distributed over a small part of the orbit. Here, it is argued, the radiation fields of the various electrons will interfere constructively and thus produce a loss of energy proportional to the square of the number of electrons. which would be a much more serious barrier to the attainment of high energies.' [32a]

The results given in the unpublished manuscript attracted considerable interest, and in the fall of 1946 Schwinger was invited to speak on the subject at the APS meeting in New York [37]. Schwinger's talk opened the series of invited papers in Session A, devoted to progress reports on the design of accelerators. The second invited speaker was John C. Slater, followed by Luis W. Alvarez.* Schwinger concentrated on the aspects that were of interest from the experimental point of view. He calculated the spectrum of radiation and found that the incoherent radiation extended into the X-ray region for an electron energy of 1 GeV; on the other hand, the coherent radiation due to electron bunching had a predominant frequency low enough to be strongly absorbed by metals

* Alvarez spoke on 'The design of a proton accelerator.' This recalls the claim that the microtron (which refers to the acceleration of charged particles (electrons or protons) by microwave cavities, the principle used in synchrotrons) was invented by Alvarez but named by Schwinger. In fact, Schwinger stated in a letter to N. Peter Trower, 'I [was] the first person to suggest the use of a high intensity microwave field. ... Toward the end of the war, I was in on the development of the synchrotron, as witnessed by my work on synchrotron radiation. That naturally raised questions of minimizing such losses by having fewer orbits and larger energy transfers. *Voilà*—the microtron.'[5] (We discussed Schwinger's ideas for the microtron briefly in the previous chapter.)

and posed no serious problem to be concerned about. He pointed out that to overcome radiation losses, the accelerating radio frequency voltage became 'formidable' for energies above 3 GeV. (Thus electron synchrotrons become impractical above an energy of 10 GeV.) He also communicated a comforting piece of information that, with proper radial dependence of the magnetic field, the radiation damping had a stabilizing effect on fluctuations of the electron trajectories by reducing the amplitude of oscillations of the electrons around the equilibrium orbit.

Schwinger's work on synchrotron radiation was published 'by proxy' in a summary paper by Leonard I. Schiff.[7] Others, like Edwin M. McMillan and John P. Blewett,[8] also used and acknowledged Schwinger's results several years before the reluctant author found the time to expound them himself in a highly pedagogical paper in the *Physical Review* in 1949 [56]. 'This was published much too late. I don't know why I waited so long. This work had been done much earlier and was communicated to various people, experimenters in particular. So it was generally known [before] I finally decided to get all the details down.'[3]

The version which appeared in 1949 differed substantially from Schwinger's unpublished paper that was circulated in 1945. In 1949, everyone knew that synchrotron radiation existed, so no discussion was made of coherent radiation or destructive interference. Moreover, by that time the extensive Russian work on the spectral and angular properties of synchrotron radiation by Ivanenko and Sokolov had appeared.[9] From Schwinger's point of view, however, the most important development was the solution of the problems of relativistic quantum electrodynamics, and by publishing the classical calculation Schwinger was reminding the public that he had been performing consistent relativistic calculations of the electromagnetic self-interaction already during the war. 'It was a useful thing for me for what was to come later in electrodynamics, because the technique I used for calculating the electron's classical radiation was one of self-reaction, and I did it relativistically, and it was a situation in which I had to take seriously the part of the self-reaction which was radiation, so why not take seriously the part of the self-reaction that is mass change? In other words, the ideas of mass renormalization and relativistically handling them were already present at this classical level.

'Well, everybody said that the mass of the electron must be an electromagnetic mass. I'm thinking really of something a little beyond the mere fact that what we have called the mass of the electron should have an electromagnetic part and perhaps a mechanical part. It was more the technique whereby you wed the two together. Lorentz had certainly never done it other than nonrelativistically. In fact everybody ran into relativistic troubles, because they had rigid things that didn't behave in the right way. What I did classically was a covariant relativistic calculation in which I could see how the electromagnetic effect and the mechanical mass just simply went together. It was a matter of technique, if you like. The technique is what this problem was all about. I did this in 1945, but when I wrote the paper in 1949 I was just interested in getting the practical details of the radiation down. After all, that message had already been absorbed and utilized [in quantum field theory]. There was no point in putting it classically. I'm just saying that it was important for me, but there was no point in publishing it. Maybe I should have. And actually the way I published it in 1949 was not the way I did it in 1945, but I was trying to simplify it. I used the relativistic covariant method, proper time, and everything else [in 1945], and it would have been unnecessarily mysterious when you can do it by much more elementary ways. When I did it in 1945 I was trying to learn something at the same time.

'What I had done was this: I was interested in finding out the radiation from an accelerated electron. And I was well aware that a very useful technique for doing this was not to study the radiation directly, but to find the reaction back on the electron, or the charge, because thereby in a very nice way you learn by the decrease in the energy of the charge the total power radiated; the emphasis was not so much on the spectrum, but on the energy loss because this was considered at the time as a practical difficulty in constructing a synchrotron. That it became an interesting tool in its own way came later. So I was interested in learning from the reaction back on the charge of its proper field what the energy loss should be, but as a relativistic problem. So I said, fine, let's do it by relativistic covariant methods and, after all, having been intoxicated with relativity since birth, it was a natural thing to do.

'So you find the radiation field covariantly and you put it in, it interacts with the current and then you get a self-reaction; putting it in the ordinary engineering language, there is a resistive part, which is energy loss, and there is a reactive part, which is the change in the properties of the particle itself. In physical language, there is radiation damping and there is the inertial effect. And they both occur together. It made no sense to say that I'll keep this and throw that away. I never felt that that was an answer to anything. But the point is that having done it covariantly, the electromagnetic mass just appeared in exactly the same form as the mass you put into the action of the mechanical particle in the beginning. So this is just added to that and obviously they are united.

'All these ideas were coming together in the context of the classical problem of electrodynamics. It was a natural thing. It's a learning situation. I believed from the very beginning in the reality of electromagnetic mass, that it behaved in the same way as mechanical mass, and there was no way physically to separate them. That's all. Then, three years later, there came a challenge.'[3] These ideas that came to Schwinger in 1945 in relation to the physical problem of the synchrotron radiation were really fundamental for solving the problem of mass renormalization in 1947.

To the uninitiated, the calculation might appear as a result of two ingenious tricks applied to classical electrodynamics, and a good use of the calculus of special functions. The purpose of these two 'tricks' was to make a non-covariant calculation produce covariant results. The classical problem of radiation emitted by an accelerated electron with nonrelativistic velocity had been solved long before, and the Larmor formula

$$P = \frac{2}{3}\frac{e^2}{m^2c^3}\left(\frac{\mathrm{d}\mathrm{p}}{\mathrm{d}t}\right)^2, \tag{5.1}$$

expressed the total power $P$ radiated by a particle of mass $m$ and charge $e$ with a rate of change of momentum $\mathrm{dp}/\mathrm{d}t$. However, Eqn (5.1) assumed that the velocity $v$ was much less than the speed of light $c$ ($v \ll c$), so it was useless for ultra-relativistic synchrotron electrons. The trick was to extend the non-covariant expressions like Eqn (5.1) to produce relativistically covariant analogs for which the original expression was a special case valid in one reference frame for low velocities. The right-hand side of Eqn (5.1) is not covariant, but the left-hand side is the ratio of the energy loss over time, both of which transform identically, and therefore it is Lorentz invariant. This said, rather than deriving the relativistic formula for the radiated energy from the ground up, Schwinger replaced $\mathrm{d}t$ in Eqn (5.1) by the invariant

$$\mathrm{d}s = \sqrt{\left(1 - \frac{v^2}{c^2}\right)}\mathrm{d}t, \tag{5.2}$$

where v is the velocity of the particle, and completed the square of a three-vector quantity, $(\mathrm{dp}/\mathrm{d}s)^2$, in order to make it the square of a four-vector, obtaining

$$P = \frac{2}{3}\frac{e^2}{m^2c^3}\left[\left(\frac{\mathrm{d}\mathrm{p}}{\mathrm{d}s}\right)^2 - \frac{1}{c^2}\left(\frac{\mathrm{d}E}{\mathrm{d}s}\right)^2\right], \tag{5.3}$$

or equivalently,

$$P = \frac{2}{3}\frac{e^2}{m^2c^3}\left(\frac{E}{mc^2}\right)^2\left[\left(\frac{\mathrm{d}\mathrm{p}}{\mathrm{d}t}\right)^2 - \frac{1}{c^2}\left(\frac{\mathrm{d}E}{\mathrm{d}t}\right)^2\right]. \tag{5.4}$$

The applicability of this simple formula was of limited value, but good enough to ascertain the total energy loss.* It became even further simplified in the

* This formula is exact in the sense that if integrated over all time it gives the total energy radiated by an arbitrarily accelerated particle. See [231], p. 388, for example.

practical cases of linear or circular trajectories. For example, in synchrotrons, the change of the linear momentum of relativistic ($v/c \approx 1$) electrons is much greater than the energy change (the former changes both in magnitude and direction); hence the second term in Eqn (5.4) could be ignored, while the first could be trivially expressed in terms of the radius $R$ of the orbit and the angular velocity $c/R$, so that the total energy loss during one synchrotron cycle was

$$\delta E = \frac{4\pi}{3}\frac{e^2}{R}\left(\frac{E}{mc^2}\right)^4 . \tag{5.5}$$

This textbook formula for the energy loss was reported by Schwinger in his invited talk at the 1946 APS meeting in New York [37], and appears early in his unpublished paper [32a].

For electrons moving along an arbitrary trajectory, or for detailed spatial distributions and the frequency spectrum of radiation, the problem had to be reworked. In the 1949 paper [56], Schwinger referred to a (non-covariant) volume integral expression for the rate at which an electric charge performs work on the electromagnetic field,

$$-\int \mathrm{j}\cdot \mathrm{E}_{\mathrm{ret}}\, dv, \tag{5.6}$$

where $\mathrm{E}_{\mathrm{ret}}$ is the retarded electric Maxwell field produced by the electric current, j, and charge, $\rho$, densities.

The second trick was to decompose $\mathrm{E}_{\mathrm{ret}}$ into two parts, symmetric and antisymmetric with respect to time reversal,

$$\mathrm{E}_{\mathrm{ret}} = \frac{1}{2}\left(\mathrm{E}_{\mathrm{ret}} + \mathrm{E}_{\mathrm{adv}}\right) + \frac{1}{2}\left(\mathrm{E}_{\mathrm{ret}} - \mathrm{E}_{\mathrm{adv}}\right), \tag{5.7}$$

by adding and subtracting the advanced electric field $\mathrm{E}_{\mathrm{adv}}$. The rationale behind this step was that the first term in Eqn (5.7), 'derived from the symmetrical combination of $\mathrm{E}_{\mathrm{ret}}$ and $\mathrm{E}_{\mathrm{adv}}$, changes sign on reversing the positive sense of time and therefore represents reactive power. It describes the rate at which the electron stores electromagnetic energy in the electromagnetic field, an inertial effect with which we are not concerned. However, the second part of Eqn (5.7), derived from the antisymmetrical combination of $\mathrm{E}_{\mathrm{ret}}$ and $\mathrm{E}_{\mathrm{adv}}$, remains unchanged on reversing the positive sense of time, and therefore represents resistive power. Subject to one qualification, it describes the rate of irreversible energy transfer to the electromagnetic field, which is the desired rate of radiation.' [56]

In the paper [56], Schwinger simply disregarded the first term as an inertial effect. However, in the original calculation of 1945, both terms had been included and—as Schwinger described above—the contributions produced by the even term combined with the inertial mass of the accelerated charge. (This discussion, however, does not appear in the manuscript [32a].) The same combination of advanced and retarded Liénard–Wiechert potentials was used by Wheeler and Feynman to describe the radiation damping of an accelerated point charge in an absorber.[10, 11] The important difference was that they considered that the self-interaction was only mediated by other charges within the absorber, but not explicitly through the field. Feynman and Wheeler postulated: '1. that an accelerated point charge in otherwise charge-free space does not radiate energy; 2. that, in general, the fields which act on a given particle arise only from other particles; 3. that these fields are represented by one-half the retarded and one-half the advanced Liénard–Wiechert solutions of Maxwell's equations. In a universe in which all light is eventually absorbed, the absorbing material scatters back to an accelerated charge a field, part of which is found to be independent of the properties of the material. This part is equal to one-half the retarded minus one-half of the advanced field generated by the charge. It produces negative damping (Dirac's expression) to give retarded effects alone.'[10]

The emitted power of synchrotron radiation was thus

$$P = -\frac{1}{2}\int \mathrm{j}\cdot(\mathrm{E}_{\mathrm{ret}} - \mathrm{E}_{\mathrm{adv}})\, dv. \tag{5.8}$$

Neglecting all the technical details of the calculations, let us just say that Schwinger expressed the fields in terms of the Maxwell potentials,

$$\mathrm{E}_{\substack{\mathrm{adv}\\ \mathrm{ret}}} = -\frac{1}{c}\frac{\partial}{\partial t}\mathrm{A}_{\substack{\mathrm{adv}\\ \mathrm{ret}}} - \nabla\phi_{\substack{\mathrm{adv}\\ \mathrm{ret}}}, \tag{5.9}$$

which, in turn, were integrals of charge or current densities, as for example the scalar potential, in the Lorentz gauge

$$\phi_{\substack{\mathrm{ret}\\ \mathrm{adv}}}(\mathrm{r}, t) = \int \frac{\delta(t' - t \pm |\mathrm{r} - \mathrm{r}'|/c)}{|\mathrm{r} - \mathrm{r}'|}\rho(\mathrm{r}', t')\, \mathrm{d}v'\, \mathrm{d}t', \tag{5.10}$$

where $\rho(\mathrm{r}, t)$ is the electric charge density. An identical expression, with the components of the current density replacing the charge density, describes the vector potential in the Lorentz gauge.

The calculation itself was complex and purely technical, done with the purpose of expressing the energy in the form

$$P(t) = \int_0^{\infty} \mathrm{d}\omega \int \mathrm{d}\Omega\, P(\mathbf{n}, \omega, t). \tag{5.11}$$

where $P(\mathbf{n}, \omega, t)$ represents the power radiated at the time $t$ in a unit angular frequency interval around $\omega$ and into the solid angle $\mathrm{d}\Omega$ about the direction $\mathbf{n}$.

Integrating over $\omega$ or $\Omega$ Schwinger could calculate the spatial and frequency distribution of the radiation. Actually, Schwinger recognized already in 1945 that different ends may be best pursued by different means: 'It is the purpose of this note to investigate in detail the properties of radiation emitted by a single electron moving in a circular orbit and, with the aid of these results, to study the radiation of these electrons in the two situations mentioned above [destructive and constructive interference]. The quantities of interest are the total rate of radiation, the rate of radiation into each of the frequencies generated by the electron, and the angular distribution of the radiation emitted at each of these frequencies. Three different methods will be employed, each yielding most advantageously one of these quantities.' [32a] Thus, not only was the total power radiated obtained,

$$P = \frac{2}{3}\omega_0 \frac{e^2}{R}\left(\frac{E}{mc^2}\right)^4, \tag{5.12}$$

equivalent to Eqn (5.5), but also the power radiated into the $n$th harmonic $P_n$, that is, with frequency $\omega = n\omega_0$, where $\omega_0$ is the Larmor frequency of revolution of the electron. $P_n$ was expressed as integrals and derivatives of Bessel functions. The angular distribution of the power in the $n$th harmonic was expressed in terms of Bessel functions and their derivatives.

The interesting feature was that for both linear and circular motion, the radiation from high-energy electrons was sharply focused in a very small cone along the direction of motion with the mean angle between the electron's velocity and the direction of emission inversely proportional to the electron's energy. As a kinematic consequence of that narrow spatial spread mostly high-frequency radiation had to be expected. The typical harmonic number was $n \sim (E/mc^2)^3$. This was good news, because it provided Schwinger with a convenient means for making approximations for integrations involved in calculating the energy spectrum which he was not able to do exactly.

Since the investigation on synchrotron radiation had been undertaken with a practical purpose in mind, Schwinger did what he had always done in his papers written for the experimentalists: he once again came up with a set of 'numbers' that could be easily used or verified. The experimental verification of these figures by Elder, Langmuir, and Pollock[12] had appeared in the *Physical Review* more than two years after Schwinger had completed the calculations, but about a year before Schwinger's complete paper. Schwinger learned about the first experimental confirmation of his predictions 'when I first talked about the work on quantum electrodynamics in New York in January 1948, [and] at the same meeting there was the first announcement by the General Electric people that with their synchrotron the theory of the spectrum of radiation had been checked experimentally. So, building of synchrotrons was one thing, and getting the experimental evidence on the validity of radiation theory took a couple of years.'[3] Schwinger returned to the subject of synchrotron radiation four years later, in 1953 [78]. The question now was whether quantum corrections could be significant to this classical radiation. The answer, in practical terms, is no: the classical power radiation, Eqn (5.12), is modified by the factor

$$1-\sqrt{3}\frac{55}{16}\frac{\hbar/mc}{mc^2}eH\frac{E}{mc^2}. \tag{5.13}$$

Even for a very strong field, $H = 10$ Tesla, this can be significantly smaller than unity only if $E \sim 10$ TeV, well beyond the reach of an electron synchrotron. (Schwinger confirmed this result, with a completely different technique, nearly 25 years later in a paper written with Wu-yang Tsai [186].)

With Robert Karplus, a student of the chemist the E. B. Wilson at Harvard, Schwinger wrote another paper with a certain electrical engineering flavor, 'A note on saturation in microwave spectroscopy' [44], submitted to *Physical Review* in January 1948. It built on the earlier work by Van Vleck and Victor Weisskopf[13] on collisionally broadened absorption lines, who, however, had not included saturation and frequency modulation. The work was the result of encouragement by Van Vleck. The Karplus–Schwinger paper was a fine example of density matrix techniques. Of most interest was the treatment, in Appendix I, of ordered expansion of exponentials: 'Here is the problem, find the expression for the exponential of the sum of two non-commuting operators. At which point every learned person will quote you some mathematician, but of course we didn't know about these mathematicians so we did it by a differential equation and by successive transformations which, was the same technique I would be using elsewhere [in quantum electrodynamics]. So it was not unimportant. ... I think everybody must have known that you could always write a formal solution in terms of ordering. But it's just a notation. It doesn't, by itself, do anything. And being very pragmatic I doubt I would have been satisfied with it.'[3]

## Choosing Harvard

With the end of the war in sight, Julian and Clarice knew that soon the world around them would change. The Radiation Laboratory would be disbanded and Clarice's job would also disappear. For a while they behaved as if nothing could interrupt their romance. Clarice was afraid that one day Julian would leave the Boston area. She knew that he was considering several attractive offers of university positions all around the United States. She did not want to part with him and did not know what the future would bring because Julian was not yet ready, or maybe just too shy, to say 'the big word.'[2]

Indeed, several most prestigious institutions had already tried to attract Schwinger with offers of professorships. He was not sure which one to accept. In 1942, shortly after his departure for the Radiation Lab, he was promoted *in absentia* to the rank of assistant professor at Purdue. It was mostly a symbolic gesture on the part of Lark-Horowitz, who saw that Julian had outgrown the place and no position there would be suitable for him any more. As his stature was fully recognized throughout the physics community, Schwinger was now interested only in truly first-class institutions where his influence would be most stimulating. He did not have to ask for anything; 'I certainly did not apply anywhere, but the guardian angels were appearing all over the place.'[3]

First, John Slater approached on behalf of MIT, at approximately the time of Schwinger's first visit to Los Alamos with Jerrold Zacharias. Schwinger suspected that the trip must have somehow been connected with this offer, which he considered seriously.

J. H. Van Vleck, the Chairman of the Department of Physics at Harvard, had already in 1939 attempted to bring Schwinger to Harvard's Society of Fellows, which Schwinger had declined. 'Harvard meant nothing to me at that time. In fact, it was almost a joke as far as I was concerned.'[3] Not discouraged by the previous refusal, Van Vleck made a second approach. During the five-year period that had elapsed, the recognized value of the prospect had increased, and so did the offer: from a three-year fellowship to tenured associate professorship.

This offer was extended to Schwinger after a very serious and lengthy worldwide search which began in the summer of 1944, soon after the Department of Physics at Harvard was authorized by the Harvard Corporation to make 'a new major appointment' for 'a man of the highest distinction' in the field of experimental or theoretical nuclear physics. Based on purely financial considerations, the decision was made to look for a theoretician rather than an experimenter, who would require a larger staff and considerable start-up funds which the Department was then unable to provide. Besides, Harvard already had a Cyclotron Laboratory under Kenneth Bainbridge's leadership and a very strong experimental physics group that also included P. W. Bridgman, J. C. Street, and O. Oldenberg. The role of the new appointee would be in part to advise and stimulate the existing experimental team and provide them with theoretical support.[14]

By Spring 1945, the search was narrowed to two candidates, Hans Bethe and Julian Schwinger. The Physics Department Committee responsible for making a choice between the two candidates felt unable to make a decision. Van Vleck wrote letters to Felix Bloch, Gregory Breit, Lee DuBridge, Enrico Fermi, Robert Oppenheimer, and Eugene Wigner, asking them for their advice on Schwinger's and Bethe's respective potentials. Aware of Schwinger's unusual habits, Van Vleck also made a few discreet inquiries among Julian's friends about his ability to teach regular (morning, that is) classes responsibly.*

The result of these tense deliberations within the Department was a draw, and the letters from external reviewers were of no help. All of them praised both candidates and expressed veiled opinions in conditional terms, which only mildly leaned toward one or the other candidate. Wigner flatly refused to declare his preference, while Oppenheimer cleverly pointed out that it was impossible to make an intelligent choice based on the assumption of superiority of one candidate over the other (although he did suspect Schwinger of being capable of the 'most fundamental' discoveries). He suggested that instead Harvard should deliberate whether to choose the younger or the more mature of the two candidates, since either one had no peer in his age category.[14]

The Departmental Committee was uncertain as to the course of action it should take; therefore, Van Vleck decided to ask for one more opinion: from Wolfgang Pauli. Pauli dodged the question of whether it was better to appoint an older or a younger man, but in the first of two letters praised Schwinger's talents and achievements as a theorist, a brilliant interpreter of experiments, and conscientious and talented teacher, making it clear that he, not Bethe, would be his preferred choice. The Committee was still divided, and, in an unusual course of action, it decided that the departmental recommendation should go to both candidates and the decision as to whom the position should be first offered be left to a special *ad hoc* committee.[14]

Finally, in the early summer of 1945, the decision was made—the candidates' ages indeed being apparently a factor—to make the offer first to Schwinger.† This time, Schwinger was interested, and in the fall he accepted Harvard's offer. In 1945 this was an enormous distinction by any standards; there were only a handful of tenured physicists at Harvard, and among them only three theorists: J. H. Van Vleck, Edwin C. Kemble, and Wendell Furry. 'I believe that what made my mind up was that Van Vleck or somebody else also asked Ed Purcell, and I had gotten very fond of Purcell at the Radiation Lab. Purcell had been the head of the group that did sort of far out experimental and theoretical stuff around 1939 [at Purdue]. So if Purcell was going to Harvard, I said "Okay, I'll go to Harvard too." I thought that was rather reasonable. Maybe I'd seen

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* For example, Morton Hamermesh assured Van Vleck that 'Julian could get to classes by 11 a.m. or noon.'[14]

† Clarice Schwinger recalled that the Secretary of the Harvard Corporation wanted to know what was involved, what equipment they would have to supply. Van Vleck told him, a notebook and a pencil. They decided it was the best bargain they could get, so they hired Schwinger.[2]

enough of MIT, and Harvard [looked attractive]. Not that I knew Harvard very well. When the call first came in 1938, I didn't think of Harvard as being an interesting physics department, but obviously things were going to change.'[3]

It was anticipated that Schwinger would be able to leave the Radiation Lab in time to assume his duties at Harvard in February 1946, in order to be available for lecturing in the spring term. But the competition was not over yet. Rabi also wanted his old protégé to return to Columbia and, after he learned from somebody about the details of Harvard's offer, he tried to use all his authority to bring Schwinger back to New York. He pleaded with F. D. Fackenthal, the Acting President of Columbia University, to come up with an even sweeter offer than Harvard's. He was as emphatic as he could be, and wrote Fackenthal a letter in which he stated that 'Dr. Schwinger is far and away the leading young theoretical physicist in the country, if not the world.'[14] He managed to convince the administration to outbid Harvard's offer. Then he contacted his former protégé and proposed that he should return to Columbia as a full professor at a salary of $7500 per annum. Schwinger politely declined, either fearing that he would have to live under Rabi's shadow or feeling that under the changing post-war conditions Columbia would no longer be able to maintain its former glorious status of 'the best university in the Universe.' He explained to Rabi that he would rather settle in Boston for its atmosphere and that he was no longer willing to return to the hectic style of life that New York would undoubtedly impose upon him.*

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* Rabi did not give up easily. He was still trying to recruit Schwinger in the fall of 1946, and secured a glowing letter from George Uhlenbeck, dated 11 September of that year. It reads, 'You know, and I don't mind to repeat it officially, that I consider Julian Schwinger as one of the best theoretical physicists, not only among those of his own generation, but actually of the country. In mathematical power *and* physical feeling there are few who are his equal. He is in these respects like Oppenheimer or Kramers, and better in my opinion than Bethe for instance. Of course, he has not published very much as yet, but what has come out is of very high caliber indeed. I think Schwinger is the foremost authority on the few particle nuclear physics. His papers on the neutron–proton interaction and on the scattering of thermal neutrons on ortho and para Hydrogen and Deuterium are quite basic, and I for instance have studied them like a textbook. Also, Schwinger's contributions to the meson theories (strong coupling, the Schwinger mixture), although not much is published, have made themselves felt.

'As a lecturer Schwinger is as good as Fermi for instance. It simply cannot be done better. As a director of graduate students he has still to prove himself. He likes to collaborate though, and because of his knowledge of experimental physics, he is really very "useful," as you know of course better than anyone else. I do not consider this usefulness quality an important requirement for a theoretician, but it is certainly fine if one has someone like that around.

Rabi's genuine insistence in trying to attract Schwinger was in itself remarkable, because at that time he was opposed to any strengthening of the presence of theorists at Columbia University. He thought that only a small number of truly the best theoreticians could be useful, and he believed that in this respect Schwinger had no equals. Four years later, no longer the Chairman of the Physics Department, Rabi received two fresh PhD graduates who were looking for jobs and interviewing for instructorships in the placement fair during the winter meeting of the American Physical Society at Columbia. Both were Schwinger students from Harvard, Eugen Merzbacher and Abraham Klein. He gave them a very cold if not rude reception. In 1991, during a symposium at Drexel University in celebration of his 65th birthday, Abraham Klein recalled Rabi's discouraging words that 'they had no real need for a theorist. For after all what could a theorist do other than give a lecture? I didn't dare to retort that this was more than he could do, since his reputation for coming to class unprepared was well known. He then proceeded to advise us that perhaps we could find jobs by emigrating, that there was nothing that we could calculate in a year that his protégé, Julian, couldn't do in an afternoon.'[15]

Schwinger also had another reason, a personal and powerful one, which was a desire to remain close to Clarice. Clarice was a born Bostonian, and felt committed to living in her beautiful and beloved city. Indeed, Clarice recalled Oppenheimer's unsuccessful attempt to win Schwinger for Berkeley: Oppenheimer wanted him to come to Berkeley after the war. Julian went to visit Oppenheimer and was nearly persuaded to go to Berkeley. Julian sent Clarice

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'With regard to the work done by Schwinger at the Radiation Laboratory, I consider it as first rate contributions to classical or mathematical physics. Again, it is a pity that so little has been published. I am sure that they would be recognized as of the same caliber as the work of say Lamb or Love or von Kármán. As a result of this work, the whole field of wave guide circuitry has been really put on a rational basis, and one can now regret that it has not been done sooner. Schwinger did the work, of course, in collaboration with a group of pretty good men, but there is no doubt that almost all the ideas and the inspiration came from Schwinger.

'From the practical point of view a fault of Schwinger is certainly his perfectionism. His list of published papers could easily have been twice as long if he had not put his standards so high. In my opinion this is a virtue though!

'What one can expect from Schwinger in the future is hard to say. It depends so much on the whole status of physics, which at present is certainly not very inspiring. Obviously Schwinger is in the midst of things, and if a breakthrough is possible, I would not be surprised if it came from him.'

A most perspicuous evaluation! We are grateful to Karl von Meyenn for bringing to our attention a photocopy of Uhlenbeck's letter to Rabi.

a telegram saying that 'the bastion has fallen.' Clarice almost died. Clarice had never lived in any place but Boston, which was her world of family and friends, and where she expected to live and die until she 'was 95.'[2] For a few days Clarice was a very unhappy young woman. But then Schwinger came back and changed his mind.[2]

Schwinger recalled: 'At first I said yes. [Oppenheimer] of course was a very seductive character. I would simply say I was teetering. But not that I had accepted. "The bastion has fallen?" Well, the bastion is an outer bastion, huh? And there are inner bastions.'[3]

Schwinger could not forget his happy stay in Berkeley, and ultimately California was the place where he really wanted to live, but under no circumstances under Oppenheimer's domination. '[In 1946] Oppenheimer said, "Would you like to come to Berkeley?" And my reaction was "No," because if [we] were at the same place, I would have the same darned problem of trying to avoid being overwhelmed, and I said no. And then he said—and this still bothers me—"Would it change your opinion any if you learned that I wasn't staying here?" He did not tell me that he was going off to Princeton, and when I read [later on] about the flaws in his character or his somewhat ambiguous approach to things, it's that example I think of. Nobody else knows about that.

'Anyway, I said no, and when he said would you come here if I were not here, I mean would that (I think he was beginning to understand my point) make it more attractive, I still said no, and now I'm not sure why. But I have the feeling that I was shocked by his duplicity.'[3]

Oppenheimer's offer, made to Schwinger after Oppenheimer had already accepted the directorship of the Institute for Advanced Study at Princeton, included a full professorship at Berkeley. When the news spread, Harvard University immediately matched the offer and promoted Schwinger to the rank of full professor. Financially, the offers were all the same, and money 'did not pose an overriding consideration.' Nor, for that matter, did any family considerations. 'I certainly did not consult my parents as to where I was going to work. My parents were still living in the same place. My father was getting older, obviously, but he was still well appreciated in his field and was in constant demand. And when my brother returned from the war, he became a lawyer.'*[3]

At Harvard Schwinger immediately became very busy. Initially he was assigned two courses to teach, one on theoretical nuclear physics, another on waveguides and applied electromagnetic theory,[3,4] just for his own satisfaction and in order to integrate this subject into theoretical physics

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* He eventually became a partner in the prestigious law firm of Zalkin, Rodin, and Goodman.[16]

conceptually. He soon began teaching quantum mechanics* and ordinary classical electromagnetism and electromagnetic theory of light. He enjoyed rediscovering for himself the details he had overlooked or had not thought about since he was a student at Columbia, such as how to explain that correlations of density fluctuations make the sky blue.[3]

As was expected from a dynamic researcher, upon joining Harvard Schwinger immediately took his first group of graduate students. They could rightly be called Schwinger's first class of doctoral students because of their unparalleled number: he accepted 11 of them at once! They were eager and excellent students, and they left their mark on physics. Bernard Lippmann and Kenneth Case tied for the distinction of being Schwinger's first students, and this group included such names as Walter Kohn, Roy Glauber, Bryce S. Dewitt, and Fritz Rohrlich. During his teaching and research career, Schwinger had 73 doctoral students (see Appendix B) and three of them, Sheldon L. Glashow, Walter Kohn, and Ben R. Mottelson, were awarded the Nobel Prize (Glashow and Mottleson in Physics, Kohn in Chemistry). At Harvard, only the experimentalist Norman Ramsey graduated more PhDs than did Schwinger (his number was 84[18]). The number and influence of Schwinger's doctoral students is unsurpassed in modern theoretical physics.

In the postwar era, talented physics graduates found themselves in great demand. Times were much different from the 1930s, when even the best graduate students had to take part-time high school teaching jobs while they did research work without compensation. Academic institutions began to heal from the losses of the young generation to the war and of the faculty to great migrations to giant national laboratories which had been created for wartime research projects. The industrial environment was profoundly changed by new discoveries. Especially in the first few years after the war, competition for talented people trained in new fields was quite fierce, and some graduates received very attractive, even lucrative, offers. One such graduate even merited a story in the *New York Times* immediately after receiving his doctorate, but not for the value or importance of his dissertation. His name was Frederic de Hoffmann, who received his PhD from Schwinger in 1948. His first job brought him a whopping sum of \$25 000 per annum, which by itself was a newsworthy fact.† By comparison, Schwinger's salary as a Harvard professor was then \$9000. The article upset Bella Schwinger; after reading the article she reprimanded her 'unworthy' son, 'If your student can do it, why can't you?'

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* Actually, the first term or so of the nuclear physics course was devoted to a beautiful course on quantum mechanics in the style of Dirac, but presented better than in any textbook.[17,18]

† De Hoffmann ultimately headed General Atomics, and became President of the Salk Institute for Biological Sciences in La Jolla, California.

## Professor of physics at Harvard University

As soon as he became a professor of physics at Harvard, Schwinger abandoned the style of working with his doctoral students regularly in the manner he had been used to in his collaborations at Berkeley and Columbia, but instead adopted the manner of his interactions at the Radiation Lab, where he had acted more like a distant mentor who stepped in as a last resource rather than as a close supervisor who would intervene in a student's daily progress. Also, as at the Radiation Lab, Schwinger's lectures played a very special role in this process, as a means of disseminating his ideas to his entourage, whose sheer size did not permit close personal interaction with everybody at all times. Schwinger's beautifully prepared lectures were an inspiration to his students, as well as a constant source of surprise, because he would always take up topics which had emerged in his own research and use them as part of his lectures. This handsomely compensated for the scarcity of close one-to-one interactions. Walter Kohn recalled the majesty and importance of these lectures: 'Attending one of his formal lectures was comparable to hearing a new major concert by a very great composer flawlessly performed by the composer himself. For example, his historic graduate courses on nuclear physics and waveguides given in the late 1940s consisted largely of exciting original material. Furthermore both old and new material were treated from fresh points of view and organized in magnificent overall structures. The delivery was magisterial, even, carefully worded, irresistible like a mighty river. He commanded the attention of his audience entirely by the content and form of his material, and by his personal mastery of it, without a touch of dramatization. Crowds of students and more senior people from both Harvard and MIT attended and, knowing his nocturnal working habits, I found the price of having to wait 10, 20, 30 minutes for his arrival quite trivial in comparison with what he gave us. I felt privileged—and not a little daunted—to witness physics being made by one of its greatest masters. Each of those two courses had a tremendous influence on the shape of their respective fields for decades to come, as did other later Schwinger courses such as quantum mechanics and field theory.'[19]

For many years, those attending the lectures were offered very precious and the latest unpublished information that almost nobody else anywhere had access to. This included very powerful computational techniques with plenty of examples, as well as stimulating ideas which Schwinger often did not wish to pursue himself. This was particularly true in nuclear physics, where Schwinger had enormous intuition and expertise but was not interested in becoming too deeply involved because, from his personal point of view, the discipline had reached the point of diminishing returns. 'At Harvard I gave courses. I was a nuclear physicist, who came back to nuclear physics, and there were people who took notes of those lectures, and I know that those notes were widely circulated during the years. When I was at the Radiation Lab, there was Harold Levine, [and] when I went to Harvard I took him with me as sort of my assistant. I guess I was granted that privilege. So he took notes of my lectures; he had an absolutely beautiful hand, and the notes were widely circulated because—I think at the time it was the only up-to-date text on the situation in nuclear physics.* Later on though, there was some competition from Robert Serber, a famous set of notes called "Serber Says." That was a little later and referred more to the accelerator age, whereas I was talking about general things, sort of going back over all the things I had worked on and putting them in context. Well, as somebody said, I'm probably more famous for what I have not published than what I *have* published.'[3]

Since Schwinger lectured extemporaneously, the notes from these lectures were the only available record of his teaching and really treasured possessions of those lucky enough to be in the audience. The handwritten copies of notes borrowed from Harold Levine were not enough, and could not benefit the physics community outside Harvard. Like David Saxon at the Radiation Lab, now John Blatt took and collected the notes of lectures on theoretical nuclear physics. As Roy Glauber recalled, the process was non-local: 'John Blatt took very good notes. These notes were shipped off to Princeton, where the graduate students there copied these notes onto ditto masters, and ran off copies. These notes were the most precious thing I owned. However, the students at Princeton often did not know what they were copying, so for example instead of the "unitary matrix" the notes had the "military matrix." These notes became an underground skeleton key [to nuclear physics], the "Cliff Notes of quantum mechanics." '[17] Hectographic reproductions of these notes were distributed to students as 'Lectures on Nuclear Physics' in 1947 and later years. The demand for these authorized but unofficial notes remained steady for a long time and in 1952 they were reproduced as a text at Boston University. They were not destined ever to become a book, because they did not meet Schwinger's impossibly perfectionist standards. Anyway, they continued to be circulated and were quoted by many researchers who learned from them, especially in the context of the applications of variational techniques.

Herman Feshbach described the importance of these lectures eloquently: 'Most of the physics graduate students and a fair fraction of the faculty in the Cambridge area made it a point to attend Schwinger's graduate course whatever the subject may have been. Nuclear physics was fortunate in that the lectures were written up by John Blatt and made available to a wide audience. These notes form an excellent introduction to the application of quantum mechanics,

* These beautiful notes may still be found in the Schwinger archive.[5]

developing a number of elegant methods of wide applicability. They contain many results specifically important for nuclear physics, many of which were never published or were later rediscovered. The notion of effective range for both the $p$–$p$ (proton–proton) and $n$–$p$ (neutron–proton) systems, the various consequences of non-central nuclear forces required by the existence of the quadrupole moment of the deuteron, the novel use of variational methods for both bound state and scattering problems, the interaction of nuclear systems with electromagnetic fields are examples. It is difficult to exaggerate the impact of these lecture notes on the generation of physics graduate students in the late forties and fifties by which time a substantial fraction of the notes had been incorporated into the general background material all practicing theorists were expected to know.'[20]

Another group of Schwinger's doctoral students, which included Richard Arnowitt, Stanley Deser, Paul Martin, Roger Newton, and Charles Zemach, collectively organized and wrote up the notes from his lectures on quantum mechanics in the early 1950s.[14] In this case, Schwinger did not agree to any form of dissemination; he was in the process of writing his own interpretation of the principles of quantum mechanics and the logical organization of the lectures was still rather fluid. He planned to write a book on quantum mechanics by himself, but he never finished the task. The primary version of the book, actually a reprint of typed lecture notes and a handful of journal articles was eventually published in 1970 [152], but Schwinger always wanted to return to writing this book. 'I wanted to catch them young and give them my version of quantum mechanics.... My approach to teaching quantum mechanics was quite special. I would begin with a very definite approach in which quantum mechanics was a symbolism of atomic measurements. Then I would introduce a symbolism of simple Stern–Gerlach experiments, composite Stern–Gerlach experiments, symbolize it by what I called then a measurement symbol, and the measurement symbolic algebra then evolved into quantum mechanics. The spirit was just to evolve in a natural way. Not deduce, but evolve the whole machinery from the beginning. Each time I did it, it became a little more sophisticated. I was rapidly transforming quantum mechanics into my own image.

'This way of presenting quantum mechanics is unique. And that's the way I'm going to write that book. I've been writing it since 1951. Now I have to get busy and write it.'[3] But although he worked on the project until the end of his life, it was never completed to his satisfaction. We will describe Schwinger's unique approach to quantum mechanics in Chapter 10.

The link between Schwinger's lectures and research was direct. A bit later, in 1954, Marshall Baker, upon receiving his AB degree from Harvard, foolhardily went to the California Institute of Technology for graduate work. There, he was very unhappy, although he did enjoy Feynman's lectures. His lifeline was the notes he received, on onion-skin carbons, from a student by the name of Paul Fennimore Cooper on Schwinger's lectures on field theory. 'These lectures I would devour. I thought these were the most exciting things I've ever had. I still have these beautiful notes.' The following year Baker returned to Harvard, and took Schwinger's classes on theoretical nuclear physics, meson theory, and what's usually called the Chew–Low theory.[21]

Taking notes during Schwinger's lectures presented a challenge in itself; Schwinger always insisted on staying home on the nights preceding his lectures to think carefully about the subject, and although he wrote out the lectures in advance he taught without referring to the notes. He mesmerized the audience and ignored the usual time constraints, yet the students waited patiently for his arrival which was often delayed by half an hour, especially if the class was scheduled to start at 11 o'clock. The first rows were usually packed by notetakers. Finally Schwinger would arrive and proceed straight to the blackboard. He would pick up the trail literally from the last word of the previous lecture and continue without interruptions until the subject was exhausted, which could well be after the scheduled lecture time had elapsed. One could not have any arrangements for taking lunch on the days Schwinger lectured! He spoke and wrote fast, leaving the audience just enough time to copy the derivations from the blackboard.

Schwinger did not encourage interruptions or questions and, after the lecture, the students had to sit down as soon as possible to fill in the blanks and collectively reproduce the interpretation and the commentary. There was nary a resource to consult, since from beginning to end Schwinger would do everything in his own way. He seldom referred to other physicists, it being noteworthy when he once mentioned the equation 'due to Dirac.'[22] The topics, method and organization of these lectures evolved from year to year as the subject was rethought, so it was worthwhile to attend these lectures year after year. Bryce DeWitt, one of Schwinger's doctoral students in the early years, recalled his memories of this period: 'It was virtually impossible to follow the lectures in class. I would simply write down everything that he wrote, which was all I could do. I barely had a moment to interject a comment or something that might guide me in the philosophy of what he was doing, or explain a step he either just finished making or was about to make. I would take these precious notes back to Kirkland House and in the evening I would rewrite them, reconstructing everything that Schwinger was trying to do, filling in gaps, and trying to make it all logical. Of course, it was all very elegant and formal and on a number of occasions I remember throwing down my pencil in disgust and saying: "The s.o.b. has done it again." What I meant was that he made something sound very plausible, but behind it were many deep unanswered questions or at least questions that if you looked at it carefully ought to be addressed. Schwinger was not addressing them and was merely being guided by some kind of intuition, led by the formalism itself. That is, the formalism would take a life of its own and just lead you even though it might not be completely legitimate to do so. ... It was always a challenge to try to fill in the gaps in Schwinger's lectures.'[14]

Abraham Klein had a similar but more appreciative perspective: 'Concerning Schwinger's brilliance as a lecturer, it is widely acknowledged that for many years he was almost in a class by himself. Though he seemed to move rapidly, generally it was possible to take notes and follow the thread of the argument, because he repeated ideas two or even three times (but never in the same words). On the other hand he did tend to smooth over difficulties and it was clear that he didn't encourage questions, so that none was ever asked, at least during his classroom lectures. I attended every course, every set of special lectures, and every seminar that Julian gave on field theory as long as I remained in Cambridge.'[23]

Kenneth Johnson, another of Schwinger's students, gave a still more positive testimony to the quality of experience of taking a course from Schwinger: 'Although at Harvard it was recommended by my faculty and graduate student advisors that I not begin my study of quantum mechanics with Schwinger's course, since I was nearly completely ignorant in that area, I not unreasonably disregarded the advice since I had arrived with the goal of learning it from him.

'I soon discovered Schwinger's style of lecturing was unique. Without holding anything written in his hands, each step followed logically from the previous analogously to the way the notes in a sonata by Mozart follow uniquely one after another. Similarly, just as one would not interrupt a great pianist in the middle and ask him to repeat something, one did not interrupt Julian with questions.... I still have my notes of those quantum courses and they provide me with teaching materials which 40 years later still provide "novel" approaches to some of the classic examples of quantum systems. It was at that time that I first learned that Julian Schwinger was a true master of "variational principles" and at the same time I found out that these methods provided elegant mathematical resources for solving physics problems. In the third semester of this course we learned about "Green's functions" and how powerful these tools became in the hands of Julian Schwinger. I still believe that these methods are the most transparent ways of solving problems in many areas of theoretical physics. They appear in many places and Schwinger's legacy in this area is truly profound.'[24]

After the lecture was over, Schwinger would usually walk out as briskly as he came in; after lunch, on Wednesdays, he would return to his office where a small crowd of his doctoral students waiting for consultations had already gathered. Doctoral students were a privileged group; others had difficulty in casually approaching the professor, who rarely ventured outside his besieged office. There was considerable competition among doctoral students for Schwinger's time. Their numbers grew well above the original 11. In the early post-war years graduate students at Harvard could choose only between only three theorists: Van Vleck, Furry, and Schwinger. Edwin Kemble was still a member of the faculty, but he was no longer active in research and had stopped accepting doctoral students. Van Vleck tried to bring down the number of dissertations in theoretical physics by transferring to experimental physics all those who did not pass the difficult examinations in mechanics and mathematics with flying colors, but because of the higher caliber of Harvard students the numbers that chose theoretical fields still remained large. Usually a few graduate students chose Van Vleck to work on condensed matter physics or magnetism, some went to Furry to study field theory and statistical physics, but the rest flocked to Schwinger, who was by far the most popular choice. He was not willing, or maybe unable, to turn down anybody, and in a short time—and for the next 25 years—he had to cope with truly overwhelming numbers of students.

Some at Harvard later criticized Schwinger for effectively abandoning his students to their own resources. This criticism leaves totally unexplained the indisputable fact of the very high professional achievement of all of Schwinger's students. Abraham Klein explained the stimulating effect of working under Schwinger: 'I somehow had the impression that it was my responsibility to exhaust all the leads he had given me in my previous interview with him, including what help I could get from the literature before seeking his help again. Once I had determined that I couldn't possibly go any further without him, I could usually get to see him before the end of the week in which that decision had been taken. The lineup outside his office was proverbial... but once through the door, there was never any sense of hurry. He took you and your intellectual problems seriously and bent all his gigantic intellect to the task of helping. I found every interaction of great value. I got as much help as I could benefit from at the time. I know that not everyone agrees with this assessment.'[15]

Not long after Marshall Baker returned to Harvard in 1955, with 10 other students he approached Schwinger to be taken on as a research student. Schwinger gave the group (which included Sheldon Glashow and Charles Sommerfield) some practice problems, which they all solved satisfactorily, although Baker's solution may have been the cleverest. 'In the following two years I may have seen Schwinger perhaps seven times, but never had the desire to see him any more, because I had so much material to work out. Schwinger may never have given me a concrete suggestion, but that was because I never had a concrete question. I was fairly lucky and found lots of things to do. Schwinger did not take credit for his students' work—in fact, just the opposite. I wrote a paper which was widely quoted. I made a very small contribution to that paper, but Schwinger had no desire to take any credit. He gave more credit to the student than the student deserved.'[21]

Kenneth Johnson, who became Schwinger's graduate student in 1952, recalled that 'At that time there was a mysterious dip in the number of students who wished to "work for Schwinger" so I did not experience the legendary waits on Wednesday afternoon when he would patiently listen to and advise his students. I always found him to be very tolerant and helpful in giving his counsel which most often helped me to move onto the next stage of research. Thus I remember him both as a superb classroom teacher and as a very kind and helpful research advisor.'[24]

In his recollections, Johnson brought up another characteristic point, that in many instances Schwinger's original ideas were presented to the unsuspecting audience as if they were a part of 'standard knowledge,' without any indication of his own authorship. Often it would take a student or co-worker a while to realize their full value. Usually the lecture notes taken by students or professors (many of whom, from Harvard or MIT faculty, attended Schwinger's lectures, often without regard to their topic) were the only written records of Schwinger's interesting explorations, which after the lecture, were erased from the blackboard. 'Julian gave a series of lectures on further developments of his functional formulation of equations for Green's functions of quantum fields which was first presented in his famous National Academy papers of 1951. There the use of "sources" as functional variables was introduced, ordinary classical sources for bosonic fields and Grassmann sources for fermionic fields. It was the functional differential equation version which, in its integral form, is presently called functional integration. Using this, many of the symmetry properties of the Green's functions can most transparently be gotten. This work alone would have been sufficient to make one famous as a mathematical physicist. I was later impressed to see how much of this material was rediscovered by others. Part of the thesis problem he gave me [Kenneth Johnson] was to work out this formulation for scalar charged fields.'[24]

Abraham Klein and Robert Karplus had a similar experience. While working under Schwinger on the fine structure of positronium, which was a fully relativistic two-body problem, they decided to learn and apply for this purpose the then newly developed method of the Bethe–Salpeter equation.[25] Klein remembered that Schwinger lectured about the two-body problem and presented a theory for it during the spring semester of 1950. When he consulted his notes of Schwinger's lectures he realized, to his great surprise, that Schwinger had taught the Bethe–Salpeter equation even before it was invented!*

This illustrates a major error in James Gleick's biography of Feynman.[28] Gleick claims that by the early 1950s Schwinger's students were at a serious

* Years later [142], Schwinger gave a time-ordered list of references to that famous equation: Nambu,[26] Schwinger [66], Gell-Mann and Low,[27] and Salpeter and Bethe.[25]

disadvantage because they were not exposed to Feynman's techniques. In fact, the evidence is that exactly the opposite is true: Schwinger's students were experts in both techniques, and because of the power of Schwinger's methods, were leading the development of quantum field theory, while Feynman had essentially no students. As Marshall Baker noted, 'When I came to Stanford [in 1957] I had a tremendous advantage,' having had a great deal of Schwinger and a bit of Feynman.[21]

Schwinger did not like to be surrounded by a crowd of disciples and felt much more comfortable in more formal lecturer–audience situations or direct person-to-person contacts with individuals. As his status as a celebrity in the academic world grew, he found it difficult to continue his established custom of meeting with friends in restaurants to discuss physics over a good meal. In the early years at Harvard, Schwinger often had such frequent conversations with Victor Weisskopf, who was then working on similar problems to Schwinger, though employing his own techniques. They did not really collaborate (though they did submit one joint abstract [45]), but to share and discuss the process of their respective efforts they met frequently, at least once a month, at a French restaurant called *Chez Dreyfus* in Cambridge. Weisskopf recalled that they had wonderful times together in 1946: 'I think it was roughly on the average once a month we had lunch together at some strange places. Some of them still exist. The food was not always very good, but the conversation was, and somehow, for me, to get regularly in touch with him was a great thing.'[29] These meetings soon became an institution of sorts and began to involve ever-growing crowds of young people who congregated to witness the conversations between Schwinger and Weisskopf. Schwinger did not enjoy this transformation of his encounters with Weisskopf, and withdrew. 'It began with the two of us, and then some other people came along. These meetings, without any invitation on our part grew into a mass orgy in which students from MIT and Harvard would also come and the whole point of an intimate conversation between the two of us was totally destroyed. Instead, we were on display, and I hated it very much.'[3]

Herman Feshbach also recalled his perspective of this early period: 'The students were superb and Schwinger had many. We ran a most delightful seminar, on Thursday evenings if I remember correctly. We often had dinner first in a French restaurant in Harvard Square. I remember one occasion when we were talking physics at such a dinner when he announced that he had just worked out the polarization that would be induced in neutrons upon scattering by $He^4$ [34]. But then he went on to add that he hadn't slept for the last 36 hours working the problem out.'[30] Elsewhere, Feshbach commented that 'This reaction continues to be often used as a polarization analyzer in many nuclear experiments.'[20]

Schwinger's coming to Harvard marked a distinct change in the style of his research. From his formative college years until the very end of the war, he had been building his reputation by engaging himself on topics of high complexity where he could use his unmatched gifts as a calculator. At Harvard the situation changed. With a legion of talented and aspiring students, Schwinger refocused on fundamentals. His list of publications reflects this rather abrupt transition. In the decade immediately following the war he published almost 50 papers (Nos. [33] to [80]). Half of these articles were co-authored, often with a junior colleague or student, and they addressed topics mostly ranging from nuclear physics through applications of quantum electrodynamics, with articles with Harold Levine on diffraction standing alone in a separate category. The other half were mostly Schwinger's fundamental papers on quantum field theory. Schwinger's relations with his co-workers during this period had one common aspect: they had to write the papers themselves, otherwise they would go unpublished, and often only a portion of the work done was actually published. Schwinger's own considerable contributions, which he had worked on alone or with minimal collaboration with others, were usually published under his name alone.

## Return to nuclear physics

Lecturing and supervising dissertations on nuclear physics turned Schwinger again into a part-time nuclear physicist. The emphasis was on using the variational methods, now improved and tested on applications on waveguides, for a number of nuclear physics problems for which he had originally developed the technique before the war. Also, the arrival of new electronic computational technology, which replaced the old-style mechanical calculating machines, suddenly made it possible to return to old, numerical analysis-intensive phenomenological calculations that had previously been abandoned as impractical because of their complexity. When Harvard acquired its first computer, named 'Mark I Calculator,'* Schwinger let it be known that he could keep the new machine constantly busy. 'I can think of only one committee I was on, which had to do with the question: Should we get involved with developing computers? This must have been in 1948. Van Vleck was on that committee. Aiken was the first developer of a fast computer at Harvard. And I think I said, "Well, if I really wanted to, I'm sure I could keep the machine busy totally, 24 hours a day, with

* The official name of this electro-mechanical machine was the IBM Automatic Sequence Controlled Calculator/Mark 1. It was presented to Harvard in August 1944 by IBM, and used by Howard M. Aiken there (who had suggested the idea for the machine to IBM) for calculations for the Navy during the war. By modern standards it was extremely slow—it had a 300 ms cycle time determined by the time required to advance the paper tape used for the program—but it was used at Harvard for calculating Bessel functions until 1959. The machine is on display at the Science Center at Harvard.

the problems I could dream up." I was thinking of all the quantum scattering problems that one could do with computers. Van Vleck's jaw sort of dropped when I said that, and he later remarked that my statement had made a very deep impression on him. "Did you mean it?" he asked. I said, "Oh, yes!" '[3]

Just at that time Schwinger found a new collaborator in the person of Herman Feshbach. Initially, with Schwinger's student Julian Eisenstein, they applied the variation-iteration technique, developed by Schwinger at the Radiation Lab, for a mixture of a central and a tensor Yukawa nuclear potentials with the radial dependence $e^{-r/r_a}/r$ with different ranges $r_a$. During 1948–49 they solved the problem of the deuteron ground state and numerically calculated the quadrupole moment, effective range and relative probabilities for the system remaining in a $D$-angular momentum state, as well as the cross sections for photoelectric disintegration of deuterons. Feshbach and Schwinger published the tabulated values of these quantities computed for a range of parameters describing the rate of the exponential decrease of the force and respective strengths of the central and tensor potentials. Preliminary results were reported at the Washington APS meeting in April 1948 [49]. Most of the work was completed in 1949, but the paper appeared only in 1951, when the theoretical results could be compared with the data from a number of experiments carried out between 1949 and 1951 [67]. Herman Feshbach recalled: 'These calculations, now of historical interest only, were one of the first to use a computing machine located at Harvard. You will notice that the results were announced in 1948 but not published until 1950 [sic]. The circumstances were as follows. I wrote the paper—which was unusual because Schwinger usually did the writing when he collaborated. We then went over my draft literally word by word. At the rate of one meeting a week it took a year to obtain the final version. It was an education for me.'[30]

A very busy scientific life and Schwinger's increasingly absolute perfectionist attitude towards the written (and also spoken) word began to result in inordinate delays in publishing. No article went out into the world unless it had been molded into a form that dazzled by its balance and elegance. Perhaps this is why, with the exception of a cursory abstract of a talk given at the APS meeting at Stanford in 1947 [40], the first written exposition of the variational method had to wait until 1950 when Schwinger wrote the well-known article with his student Bernard Lippmann on 'Variational principles for scattering processes'[60], which, characteristically, referred to Saxon's waveguide notes [148]. By that time, Schwinger's results, originally generated as approximations by variational methods from general postulates, had been rederived directly and confirmed by other researchers[31] and a number of Schwinger's own students. Before publication, the only way to learn the new theories was to go straight to the source and attend Schwinger's lectures.

Many articles written by these formal or informal auditors contained considerable fragments of Schwinger's unpublished work, which was otherwise not commonly available. For example, the article by John Blatt and J. D. Jackson from MIT in the *Physical Review* on neutron–proton scattering[32] included as many as five pages of detailed derivations described by these authors in a footnote as 'The following derivation, except for a few trivial changes, is reproduced from lecture notes of a course on nuclear physics given by Professor Schwinger at Harvard, Spring 1947.' Indeed, with this kind of unsolicited assistance there was no need to rush and publish the still imperfect work if the task could be done by others!

It is impossible to give a full account of all the contributions of people from Schwinger's circle that were inspired by his lectures, but the earliest included doctoral dissertations by members of his 'first class of graduate students': Thus Fritz Rohrlich investigated the variational method for improving the Born approximation for high-energy (by 1948 standards) scattering, and Walter Kohn, in his dissertation, generalized the technique of treating many-body problems like the scattering of light nuclei.

These were already more sophisticated applications of the variational method, but were similar in spirit to the early applications by Schwinger in which he used scattering data to verify theoretical postulates about models of the shape and range of nuclear potentials. This was done by comparing theoretical and experimental values of the phase shifts of the partial wave functions of scattered particles (partial waves being the individual terms in the expansion of the wavefunction into spherical harmonics, each corresponding to a different angular momentum eigenstate). In the early years of nuclear theory such comparisons were useful, for example in interpreting the data obtained by the scattering of slow neutrons by hydrogenous targets. For slow neutrons it was generally enough to consider only the $S$-waves. In the asymptotic region the phase of the scattered wave was shifted with respect to the incoming wave, and the sign and magnitude of that shift depended on the form of the interaction potential.

More energetic collisions demanded that higher spherical harmonics also be included and the number of necessary phase shift parameters quickly multiplied. The phase shifts are functions of the energy. For the purpose of analyzing the experimental data, one usually expanded them into power series in energy or, equivalently (which was then preferred), in powers of the square of the wave number $k$, which is proportional to the energy measured in the center-of-mass frame. The first few terms of such a series expansion for the $S$-wave were traditionally written as,

$$k \cot \delta = -\frac{1}{a} + \frac{1}{2} r_0 k^2 - T k^4 + \dots, \qquad (5.14)$$

where $\delta$ is the phase shift. Here $a$ is called the scattering length and $r_0$ the effective range. With nuclear physics well past its age of infancy, the modeling of nuclear potentials with square wells, so useful in the times of Fermi and Amaldi, was no longer satisfactory. With more accurate experimental evidence it became possible to hypothesize about the shapes, ranges, or possible spatial distributions of nuclear potentials. This posed a very serious computational challenge. In order to determine the coefficients of expansion in Eqn (5.14), one had first to solve the Schrödinger equation corresponding to the scattering problem and only then perform the expansions. The first step, even for relatively simple potentials, required extremely laborious numerical computations. The scattering data necessary for the interpretation and assessment of theoretical hypotheses were not accurate enough to justify comparisons with the theory going beyond, perhaps even as far as finding $r_0$, the effective range. It was very frustrating to see that, despite all the progress in experimental and theoretical nuclear physics, Nature stubbornly refused to divulge any new information about the exact form of the nuclear force. Even more frustrating was the fact that the weakest link in the process of matching theory with experiment was a seemingly trivial procedure of fitting the simple free parameters in the models for nuclear interaction potentials, like slopes and ranges, to match the coefficients of expansion of the power series, Eqn (5.14).

The beauty of Schwinger's method was that in one fell swoop it circumvented two of the most frustrating elements of analysis: first and foremost, it completely eliminated the need to calculate the phase shifts from the general solutions. Instead, the phase shifts were used as variable parameters for the variational method. Secondly, the technique even eliminated the need to solve exactly for the wavefunctions. All that was required was to have a general idea about the main characteristic properties of the solutions. Only after the general initial hypothesis was confirmed by the data, would it be worthwhile to invest additional time and effort in trying to find the analytical solutions, which was much easier when one knew their general features beforehand. Therefore, people—like Hans Bethe—were often able to benefit from the foresight provided by Schwinger's variational technique to find rigorous answers.[31]

The following simplified presentation is based on that given by Blatt and Jackson,[32] who began by saying that 'the Schwinger analysis, upon which this whole work is based, can be summarized for our purposes as follows.' In the center-of-mass frame of the colliding particles, the radial part of the wavefunction of the scattered particle is given by

$$\left(-\frac{d^2}{dr^2} - k^2\right)\Psi(r) = U(r)\Psi(r), \tag{5.15}$$

which is a simplified equation; certain constants are absorbed in the potential $U(r)$ and the actual Schrödinger wavefunction is $\Psi(r)/r$.

One of the lessons Schwinger had learned from dealing with other seemingly unsolvable problems at the Radiation Lab was that it was generally helpful first to separate the interaction-independent aspects of the solution. He developed a perfect technique for doing this with the help of Green's functions $G(r, r')$ corresponding to the interaction-free analog of Eqn (5.15),

$$\left(-\frac{\mathrm{d}^2}{\mathrm{d}r^2} - k^2\right) G(r, r') = \delta(r - r'), \tag{5.16}$$

with certain additional boundary conditions. Then it was possible to replace the differential Eqn (5.15), together with its boundary conditions, by an equivalent integral equation,

$$\Psi(r) = \sin kr + \int_0^\infty \mathrm{d}r'\, G(r, r')U(r')\Psi(r'). \tag{5.17}$$

The way to arrive at Eqn (5.17) is now a standard textbook problem, and Schwinger's role in discovering it for physical applications is generally understated. The form in which it is written also contains a clue to understanding Schwinger's manner of conducting perturbative calculations. His emphasis was more of iteration than of expansion.

Eqn (5.17) is a good starting point for applications in a theory in which the potential $U(r)$ is already known. The interaction potential is present only in the integrand, the sin $kr$ term is the solution corresponding to the asymptotically correct interaction-free propagation case and, barring irregular potentials, one could apply to it a series of successive iterations. From the asymptotic form of Eqn (5.17) one can find the value of the tangent of the phase shift,

$$\tan\delta = k^{-1} \int_0^\infty \mathrm{d}r' \sin kr'\, U(r')\Psi(r'). \tag{5.18}$$

Structurally, Eqns. (5.17) and (5.18) have a form identical to the integral equations which Schwinger had encountered while working on the theory of waveguides. In his book on waveguides [148], he gave the prescription for constructing the variational equations for $\Psi(r)$. He casually called these steps 'the usual recipe.' Schwinger's 'usual recipe,' applied to the phase shifts produced the following variational equation for $k \cot\delta$,

$$k\cot\delta = \frac{\int_0^\infty \mathrm{d}r\, U(r)\Psi^2(r) - \int_0^\infty \mathrm{d}r \int_0^\infty \mathrm{d}r'\, U(r)\Psi(r)G(r, r')U(r')\Psi(r')}{\left(k^{-1}\int_0^\infty \mathrm{d}r\, U(r)\Psi(r)\sin kr\right)^2}. \tag{5.19}$$

The integral expression (5.19) is stationary with respect to the variation of $\Psi(r)$ that solves the original Eqn (5.17), and simultaneously gives the value of the phase shift calculated from Eqn (5.18). This property of stationarity establishes a surprisingly quickly converging sequence of iterations. All one has to do is to substitute a trial wavefunction into Eqn (5.19) and compute the first approximation to the phase shift; then substitute it into the integral Eqn (5.17) and iterate to get an improved value for the wavefunction $\Psi(r)$. The new $\Psi(r)$ then serves as a better trial function for the next round of iterations.

The power of the variational method comes from the fact that the error of the result is proportional to the square of the error made in estimating the input. It worked like magic when solutions of the form (5.14) were needed. The solution of the equation

$$-\frac{\mathrm{d}^2}{\mathrm{d}r^2}\Psi_0(r) = U(r)\Psi_0(r), \tag{5.20}$$

which is easily solvable, provides a good trial function for the first round of iterations. Since it is obtained from Eqn (5.15) by dropping the term proportional to $k^2$, one can say that its solution is 'accurate up to order $k^2$'; thus the error in the phase shift calculated with its use would be of the order of $k^4$. Therefore, the first two terms in the expansion (5.14) could be accurately predicted already in the first round of the approximation! The cavalier simplicity of this reasoning does not work in the general case, but is justified by the short range of nuclear forces. Blatt and Jackson concluded this section of their paper with the words: 'The preceding remarks are intended merely to sketch in some of the background of this particular application of the Schwinger formalism. It is hoped that Professor Schwinger will soon find the time to publish the general formalism in detail.'[32]

Schwinger published an interesting example of the power of the variational technique early in 1950, 'On the charge independence of nuclear forces' [58]. The application was to 'the small difference between the neutron–proton interaction in the $^1S$ state.' What was of interest here was his use of a variational principle for the $^1S$ phase shift, and the 'first published effective range derivation.'[33] (Of course, the latter were contained in his unpublished, but well-circulated, lecture notes.)

For applications to more complex scattering processes more sophisticated techniques, capable of unraveling subtler details of interactions, were needed. For these, Schwinger turned to the scattering operator method. He sought a way of reformulating it so that it would be derivable from a quantum extremum principle of a more general nature.

In Schwinger's eyes, the variational principle was assuming an ever more prominent role. It evolved from an expedient tool of making approximations to a fundamental principle defining quantum theory in the same spirit as Hamilton's principle of least action defines classical mechanics. The paper with Lippmann on the 'Variational principles for scattering processes' [60] included an elegant derivation—written from the perspective of the improvements and applications over several years—of the variational principle for the scattering or collision operator. It was followed by an application to the calculation of the phase shifts in nuclear scattering, essentially similar in spirit to the just outlined *ad hoc* method taken over from applications to waveguides.

Already, in the introduction of the paper, after enumerating the successful uses of the variational principle in scattering processes, waveguides, neutron diffusion and acoustical and optical diffraction, Schwinger gave his readers a clue by emphasizing that the usefulness of the variational method in such a diverse group of phenomena might not be a mere accident, but a consequence of a hidden underlying principle of more fundamental nature: 'Indeed, such methods are applicable in any branch of physics where the fundamental equations can be derived from an extremum principle.' [60]

The derivation started from Schwinger's trademark technique of unitary transformations to the quantum interaction picture, which was already present in his first, unpublished, paper [0]. In the interaction picture the Schrödinger equation for the state vector takes the form

$$i\hbar\frac{\partial}{\partial t}\Psi(t) = H_I(t)\Psi(t), \tag{5.21}$$

where $H_I$ (Schwinger's original notation) is the Hamiltonian of interaction in the interaction picture and all explicit reference to the Hamiltonian of free particles is avoided. For applications to scattering, Schwinger described the change in time of the state vector $\Psi(t)$ as an evolution from the initial state $\Psi(-\infty)$ through the action of the unitary, forward-in-time, evolution operator $U_+(t)$,

$$\Psi(t) = U_+(t)\Psi(-\infty), \tag{5.22}$$

which evidently satisfies the same equation as the state vector,

$$i\hbar\frac{\partial}{\partial t}U_+(t) = H_I(t)U_+(t), \tag{5.23}$$

with the initial condition $U_+(-\infty) = 1$. The $S$ operator, what Schwinger called the collision operator, is the operator that generates the final state from the initial state, thus

$$U_+(+\infty) = S. \tag{5.24}$$

The integral equation, equivalent to the differential equation (5.23)

$$\begin{aligned} U_+(t) &= 1 - \frac{i}{\hbar}\int_{-\infty}^{t} H_I(t')U_+(t')\,dt' \\ &= 1 - \frac{i}{\hbar}\int_{-\infty}^{+\infty} \eta(t-t')H_I(t')U_+(t')\,dt' \end{aligned} \tag{5.25}$$

and where* $\eta(x) = 1$ for $x > 0$ and vanishes if $x < 0$, resembles Eqn (5.17) and is its temporal equivalent with $\eta(t-t')$ acting as the Green's function describing the evolution forward in time, and with the interaction Hamiltonian $H_I(t')$ appearing instead of the potential. Symmetrically, Schwinger also introduced $U_-(t)$, interpreted as an operator of evolution backward in time,

$$U_-(t) = 1 + \frac{\mathrm{i}}{\hbar} \int_{-\infty}^{+\infty} \eta(t' - t) H_I(t') U_-(t') \, \mathrm{d}t'. \tag{5.26}$$

Schwinger tried to construct an expression for the scattering operator which involved $U_\pm(t)$ and reproduced the original equation for the evolution operator in the interaction picture, Eqn (5.23), as the condition of stationarity of $S$ under arbitrary variations of $U_\pm(t)$ that would justify an iterative procedure in which one could start from imperfect empirical predictions for $U_\pm(t)$ and improve on them, gradually approaching the exact form of the scattering operator. The prescription itself was not unique. The simplest integral expression for $S$ that Schwinger found suitable was

$$S = U_+(\infty) - \int_{-\infty}^{\infty} U_-^\dagger(t) \left( \frac{\partial}{\partial t} + \frac{\mathrm{i}}{\hbar} H_I \right) U_+(t) \, \mathrm{d}t, \tag{5.27}$$

with the boundary condition $U_+(-\infty) = 1$. The crudest possible trial function for the evolution operator is the identity transformation $U_\pm \equiv 1$. When substituted in Eqn (5.27), it reproduces the first term of the Born approximation,

$$S = 1 - \frac{i}{\hbar} \int_{-\infty}^{\infty} H_I(t) \, \mathrm{d}t. \tag{5.28}$$

However, one can also use a form that, upon variation, yields the integral Eqn (5.25) or (5.26). That is,

$$\begin{aligned} S = 1 &- \frac{\mathrm{i}}{\hbar} \left[ \int_{-\infty}^{\infty} U_-^\dagger(t) H_I(t) + H_I(t) U_+(t) \right] \mathrm{d}t \\ &+ \frac{i}{\hbar} \int_{-\infty}^{\infty} U_-^\dagger(t) H_I(t) U_+(t) \, \mathrm{d}t \\ &+ \left( \frac{i}{\hbar} \right)^2 \int_{-\infty}^{\infty} \int_{-\infty}^{\infty} U_-^\dagger(t) H_I(t) \eta(t - t') H_I(t') U_+(t') \, \mathrm{d}t \, \mathrm{d}t', \end{aligned} \tag{5.29}$$

* Schwinger always used $\eta$ for the unit step function rather than the usual $\theta$. This was, he said, because the capital letter eta, $\mathcal{H}$, reminds one that this function was introduced by Heaviside.

with no restriction imposed on $U_+$ or $U_-$. Now if the crude approximation $U_\pm(t) = 1$ is employed we obtain, already in the first step, a more accurate result identical to the second Born approximation

$$S = 1 - \frac{i}{\hbar}\int_{-\infty}^{\infty} H_I(t)\,\mathrm{d}t + \left(\frac{\mathrm{i}}{\hbar}\right)^2 \int_{-\infty}^{\infty}\int_{-\infty}^{\infty} H_I(t)\eta(t-t')H_I(t')\,\mathrm{d}t\,\mathrm{d}t'. \tag{5.30}$$

However, there remained two subtle points regarding consistency. The fact that Schwinger discussed them at all demonstrated that he treated the procedure more as a step toward obtaining a fundamental principle than as a calculational device. The variational method did not guarantee automatic unitarity of the scattering matrix as exemplified by the fact that neither Eqn (5.28) nor Eqn (5.30) satisfied the unitarity condition $SS^\dagger = 1$. This turned out to be a curable defect; Schwinger circumvented it by not formulating the variational principle for the scattering operator itself, but for a related Hermitian reaction operator $K$ which defined the scattering matrix through the identity

$$S = \frac{1 - \frac{1}{2}\mathrm{i}K}{1 + \frac{1}{2}\mathrm{i}K}. \tag{5.31}$$

The other point of consistency was of a different nature. Schwinger argued that the concept of the non-interacting state of two particles rests on the spatial separation of the two parts of the physical system. Therefore, if, after the quantum interaction, the two parts of the system involved move away from each other and become free again, they can no longer be described by plane waves. The plane waves are exact momentum eigenstates, and by Heisenberg's uncertainty principle no assumption about their localization should be made. He argued that the energy eigenstates should be used instead and the interaction turned off adiabatically, infinitely slowly, so that when the particles move away from each other there should be no interaction, no matter whether the particles' wavefunctions overlap or not. Schwinger elaborated the technical details of such reformulation of the scattering problem by introducing a multiplicative factor $\exp(-\epsilon|t|/\hbar)$, $\epsilon \to 0$, into the Hamiltonian.

Of course, the Lippmann–Schwinger paper is best known for its formulation of scattering theory rather than for the variational scheme proposed. Schwinger summarized his view of the importance of the paper as follows: 'The idea was to write down the general quantum-mechanical form, operator form of stationary expressions for scattering amplitudes. The emphasis is on operator form. And I think I essentially wrote the paper. [Lippmann] contributed some calculations, which were published separately. I proceeded then to sketch in operator form what I considered to be the standard formulation of scattering theory in terms of incoming waves and the scattered waves and so forth. I thought the importance of the paper was the variational principle. Everybody disregarded the variational principle and somehow thought that my compact restatement of conventional scattering theory was a major step forward. To my utter astonishment. I thought I was just repeating what everybody knew and suddenly it becomes enthroned as a new way of stating scattering theory. I still don't understand it. But anyway the so-called Lippmann–Schwinger scattering equation is to me conventional scattering theory written in operator notation. Nothing new. But that is what everybody paid attention to.'[3]

Schwinger's final foray into nuclear physics was an abstract written with his student Robert B. Raphael in 1953, 'On high energy nuclear scattering and isobars' [72]. It was an attempt to explain the difference between the $n$–$p$ and $p$–$p$ scattering data discussed three years earlier in [58]. 'It was fundamentally a thesis problem. [Raphael] was a rather unique graduate student who became a Trappist monk after I gave him his PhD. It was a phenomenological theory. I remember that Oppenheimer saw that paper and said he thought that the development had gone beyond that stage, which I thought was quite wrong because nobody had really worked out a quantitative theory of the isobars and all the rest of it. I really don't know what he meant, but I think he presumed that we now knew enough about the nucleon and the pi-meson that a phenomenological treatment was unnecessary. But I don't think that's ever true. A phenomenological treatment side by side with a more fundamental theory is always useful.'[3] Although Schwinger started to write a note expanding on this abstract, once again the fragment was never completed. [5]

## Marriage

Julian and Clarice were seeing each other regularly, but were not engaged. Friends of Clarice at work were a bit jealous; they considered Julian an attractive and perhaps also a wealthy man. Clarice did not have a telephone on her desk and on the rare occasions that Julian called her at the office, whoever answered the telephone was impressed with Schwinger's marvelous voice, and they were even more impressed when he came to call for her in his Cadillac.[2]

Finally, Clarice's mother decided it had gone on long enough, and they should get married. Clarice stopped seeing him for a while, which had the desired effect: he decided he would like to marry Clarice. He invited her out and proposed, and Clarice accepted on the spot. They became engaged in April 1947, and got married on 8 June 1947. Julian's mother had a significant influence on the wedding. His mother was a very elegant woman and she had very definite ideas as to how things should be done. Clarice had wanted to be married in her house, and have only a small family wedding. Because of Belle's affectation, they had a big wedding, with 110 people in attendance. It was a beautiful wedding held in the Plaza Hotel in Boston.[2]

The food was good and Clarice recalled that Julian ate his dinner, and she drank his wine. All the family members from both sides were present, coming from New York, Philadelphia, and Boston. Clarice's father had two sisters with large families in Boston. Julian's mother had a sister who died when she was very young, but her three daughters came with their grandparents. Of course people from Harvard attended. Harold, Julian's older brother, was the best man, and Rhoda Abrams, Clarice's friend from the age of eight, was the matron of honor.[2] Julian recalled that only his immediate family, father, mother, and brother came, 'I doubt there was anybody else. There were other friends. There's a very funny photograph of Hamermesh and Feld, who are both very tall, very narrow people, framing me. It was amusing.'[3]

The wedding took place just a few days after the famous Shelter Island Conference, which was held from 1 to 4 June. (We will discuss this conference in the next Chapter.) Maybe because of the approaching wedding day, Schwinger returned from the conference by seaplane to New York, and then, also by plane, to Boston. There was bad weather and Clarice recalled that she was visiting one of her aunts when he came from the airport to see her, and he looked 'mean and gray.' It appeared that he had literally smoked himself sick; he came back so ill they thought they would have to postpone the wedding. As a result Schwinger just decided he wasn't going to smoke anymore. He stopped cold. Everybody assumed that it was the new wife who had influenced him in quitting smoking, but in fact, Clarice learned to smoke from him.* Schwinger attributed his heavy smoking to 'the baleful influence of Oppenheimer, who set the model for everybody.' But after Shelter Island he stopped because 'it was my good sense saying enough of this agony.'[3]

After the wedding, the Schwingers went on a honeymoon, but not without a delay of a few days caused by a siege by graduate students and the need to discuss some physics problems with Lippmann. 'Lippmann, working on something, cornered me.'[3] (Although Clarice recalled that they were just slow in getting started.[2]) The trip was to be by car because Julian wanted to show Clarice all the places he had visited before, and use the occasion to see as many

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* Clarice recalled going to a nightclub with Julian. He smoked heavily, and Clarice thought he was not aware of how many cigarettes he was smoking. So she decided to play a trick on him. Every time he would light up, Clarice asked him for a cigarette, in order to make him realize how many he was smoking. Clarice was green by the end of the evening. She had never smoked so much in her life. All he did was call the cigarette girl over and get another package of cigarettes. He was a chain smoker until he got sick, and then he abruptly stopped.[2]

natural wonders as possible. They started so late that they could not cover much territory; they reached as far as Concord by 11 p.m., and not many hotels were open. The next day they left there at about 2 in the afternoon and stopped for dinner around 11. Eventually they arrived in Chicago, where they met Bob Sachs and his first wife who took them to a seedy nightclub. Julian, who was always interested in clothes, bought Clarice a very beautiful pair of high-heeled ankle-strap shoes. As a result when they got to Wisconsin Clarice found that everybody else was running around in shorts while she was wearing a black wool suit and black suede ankle-strap pumps.[2]

From Madison, Wisconsin, the newlyweds proceeded west, their destination being Yellowstone, and then to California, where they made two long stops, first at Berkeley and then at Los Angeles, everywhere meeting Julian's old friends and acquaintances. At Berkeley they of course visited the Oppenheimers at their beautiful house called 'Eagle's Nest,' where Clarice was impressed with Oppenheimer's marvelous Van Gogh. She was sitting there admiring it when Oppenheimer came in and said, 'Nice, isn't it?' 'Nice' wasn't at all the way Clarice was feeling about it. But she remembered that Oppenheimer was very charming and his wife Kitty was warm and pleasant. However, after they gave her their famous Martinis Clarice went into the garden and instead of just one flower she saw a field of flowers. They had a very pleasant visit in Berkeley, where they also saw Bob and Charlotte Serber. Clarice, as a newlywed, was impressed because they had been married 14 years.[2]

The next stop was Los Angeles. David Saxon, Schwinger's good friend and co-worker from the MIT Radiation Laboratory, had been on the faculty of UCLA for a year or so. He had family in Los Angeles and very hospitable parents who invited Julian and Clarice to stay with them. As a general rule, the Schwingers never stayed with anybody, because they considered themselves such terrible houseguests, staying up late and then, when the hosts were ready for lunch, wanting breakfast! But they did it in Los Angeles. They went to Saxon's parents' house, and as the 'immature children we were, despite our advanced age,' they stayed 10 days.[2] Once again it was a very warm, outgoing house, full of hospitality and ease.

Clarice thought Los Angeles to be a very pleasant place to live but a very difficult place to visit. It seemed phony to her. There was nothing that appealed to her, but Julian always liked it.[2] The Schwingers turned around in Los Angeles. On the way from Berkeley to Los Angeles they had visited Yosemite, but from Los Angeles they continued towards Los Alamos. The first thing they did there was to meet the Tellers and the Goldbergers, with whom they went on a very nice picnic. Schwinger's visit to Los Alamos could not pass without him being invited to give a colloquium, so Mary Hall, who was responsible for seminar arrangements, contacted Schwinger and asked him to give a talk at eight in the morning, and he accepted. The Schwingers therefore had to go down to Santa Fe to buy an alarm clock. Because of the ticking of the clock, they never slept that night. They waited all night for 7:00 to come. Then Schwinger gave his talk at 8:00. They kept that alarm clock for a long time as a souvenir of that event.[2]

Los Alamos is quite close to the Bandelier National Monument, famous for its Indian ruins. The Schwingers went there and when Clarice got out of the car she burst into tears. What she had not realized was how much she had missed seeing green. She thought that she had been perfectly content in the desert and was having a perfectly good time; both could not have been more startled at Clarice's reaction.[2]

From Los Alamos the Schwingers took the southern route home. Upon their return they did not immediately start an independent life. Initially they settled down with Clarice's mother, who lived in a comfortable house in the Dorchester area of Boston. Julian got along very well with his mother-in-law. Eventually, with the help of Sadie Carrol, who perused the apartment listings in the newspaper, they found an apartment of their own on 58 Fayerweather Street in Cambridge, in a convenient location almost across the street from the marvelous Georgian house of J. H. Van Vleck. Starting from the Schwinger's house the tone of the neighborhood changed; it consisted of two- and three-family houses for firemen, policemen and other people of modest means. Clarice thought it was a hideous stucco house, and Julian also hated it with a passion. He said that he had to close his eyes every time he went into it. But inside the Schwingers found it enormously comfortable. They had the second and third floors with large rooms and a porch. Behind them lived Martin Deutsch from MIT, who had a beautiful old house on a half acre of woods. All in all, the Schwingers found it comfortable and convenient; they ended up staying there for 12 years. The rent was only $100 a month, and the landlords never increased it, so there was never any incentive to move.

Although Clarice became Julian's wife, her mother still managed the house. When they left her house her mother came with them. The relationship between Julian and Sadie was extraordinary. She and Julian lived together until her death in 1986. They understood each other very well and they loved each other dearly.[2]

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# The development of quantum electrodynamics until 1947: the historical background of Julian Schwinger's work on QED

## Introduction

Prior to 1947, Schwinger had not worked in quantum electrodynamics, apart from his first unpublished paper 'On the interaction of several electrons' [0], which he wrote in 1934 for his private pleasure and did not show it to or discuss it with anybody—not even with Lloyd Motz, his friend and mentor at CCNY, with whom he was collaborating at that time. Schwinger had amazing powers of absorption and read everything he could lay his hands on in the field of theoretical physics. Before joining CCNY, he had already studied P. A. M. Dirac's *The principles of quantum mechanics*, first published in 1930, which he had purchased in a secondhand bookstore in New York City. From then on, Dirac became his principal—though invisible—teacher, and he turned to Dirac always for inspiration and instruction. As a freshman at CCNY, Schwinger studied the recently published papers on quantum field theory of Dirac, Heisenberg, Pauli, Fermi, Oppenheimer, and others; he absorbed all that was being done in this field. However, apart from that first unpublished paper on quantum electrodynamics, he did research and published in other fields that were brought to his notice by Otto Halpern, Lloyd Motz, Edward Teller, Isidor Rabi, and others; he worked especially on nuclear problems.* However, he maintained his interest in quantum field theory, and had more exposure to the subject when he went to Berkeley from Columbia to work with J. Robert Oppenheimer for two

* In fact, Robert Finkelstein quoted Robert Sachs as saying that Schwinger would never become a field theorist because of his pre-eminence as a nuclear physicist.[1]

years. There he became convinced of the reality of vacuum polarization—the virtual appearance, for short periods of time, of electron–positron pairs in the vacuum. On his way to his first regular job as a physics instructor at Purdue University, he came into contact with Wolfgang Pauli as a fellow lecturer at the Michigan Summer School in 1941. At the MIT Radiation Laboratory, where he spent the last few years of the war working on problems of waveguides and classical electrodynamics, he again paid attention to Pauli's work on meson theory and Heisenberg's introduction of the scattering matrix, which he put immediately to use as we have discussed in Chapter 4.

In his first year as a professor at Harvard University, Schwinger led his students and collaborators again to work on problems of nuclear physics. Nathan Marcuvitz recalled that they were supposed to write jointly a volume for the Radiation Lab series which Marcuvitz was editing, 'but we got involved in other things.'[2] Schwinger did continue working on classical diffraction problems with his assistant Harold Levine. Most significantly, however, already during his work at the Radiation Laboratory Schwinger had become interested in the problem of what would be called synchrotron radiation; he was interested in finding out the properties of the radiation from a relativistically accelerated electron. In this context, he began to think more deeply about the nature of the mass of the electron; it had long been recognized that the mass of the electron should have an electromagnetic part and perhaps a mechanical part, but at MIT he saw how to tackle this problem relativistically. After World War II, the problems of quantum electrodynamics and quantum field theory were again coming to the fore, with new measurements being made with the experimental techniques developed during the war, many of them in connection with the development of radar. Already in the late 1930s Schwinger had been aware of the discrepancies between the predictions of Dirac's theory of the electron and the experimental measurements on the fine-structure of hydrogen. Thus, for example, he had read all the available experimental literature showing minor differences between Dirac's theoretical predictions for the hyperfine structure of hydrogen and the experimental data. Schwinger was certainly familiar with the old developments in quantum electrodynamics, and we shall briefly outline this general development in order to place in context the great achievements of the physicists of Schwinger's generation in this field during the period 1947–50. For further details, see Refs. 3–9.

## P. A. M. Dirac's theory of radiation

The beginning of quantum electrodynamics as a modern theory of interaction of light with matter was made by Paul Dirac in 1927 in his fundamental paper on 'Quantum theory of emission and absorption of radiation,' communicated to the *Proceedings of the Royal Society (London)* by Niels Bohr.[10] At the very beginning of this paper, Dirac stated that 'hardly anything has been done up to the present on quantum electrodynamics.'[10] Up to that time there existed a classical theory of radiation and a sketch of certain phenomenological aspects of the quantum theory of light (Max Planck's quantum theory of blackbody radiation and Albert Einstein's revolutionary idea of the existence of light quanta or photons) and the non-relativistic quantum theory of particles of matter (i.e. Werner Heisenberg's matrix mechanics and Erwin Schrödinger's wave mechanics, with deep insights from Paul Dirac, Max Born, Pascual Jordan, and some others). In the papers of Born and Jordan[11] and of Born, Heisenberg, and Jordan,[12] an attempt had been made (by Jordan alone) to apply matrix mechanics to the eigenvibrations of a string mainly in order to calculate the mean square fluctuations in the field of cavity radiation. They confirmed Einstein's famous formula[13] for the mean square fluctuation for the blackbody radiation,

$$\mathcal{E}^2 = h\nu E + \frac{c^3}{8\pi\nu^3\,\mathrm{d}\nu}E^2, \tag{6.1}$$

where $E$ is the average energy (per volume) of the radiation between frequencies $\nu$ and $\nu + \mathrm{d}\nu$, and $\mathcal{E}$ is the mean squared fluctuation in the energy.

Dirac's idea was to apply quantum mechanics not only to the particles in atoms but also, by making use of the ideas of Paul Ehrenfest and Peter Debye, to consider the radiation field in empty space as a system of quantized oscillators which interact with atoms. The difficulties involved were so great that Dirac found it worthwhile to look into an approximation which was not relativistic. As a total system, he considered an atom in interaction with a radiation field. In order to have a discrete number of degrees of freedom for the latter, he enclosed the system in a finite box, and decomposed the radiation into its Fourier components. The Hamiltonian of the entire system of atoms and radiation takes the form

$$H = H_0 + H_{\text{int}}, \tag{6.2}$$

where $H_0 = H_{\text{atoms}} + H_{\text{field}}$ is the Hamiltonian of the non-interacting atoms and electromagnetic field, $H_{\text{atoms}}$ being the Hamiltonian of the atoms only, and $H_{\text{field}}$ being the Hamiltonian of the field alone. The $H_{\text{int}}$ term describes the electromagnetic interaction, which, for charged particles with electric charge $e$ in a radiation field described by the three-vector electromagnetic potential A, has the form

$$H_{\text{int}} = e\int \mathrm{j}\cdot\mathrm{A}\,\mathrm{d}^3x, \tag{6.3}$$

where j is the three-vector (probability) current density of the particles. As long as one cannot solve the entire problem exactly, Dirac proposed to treat the effects of the interaction term $H_{int}$ as a perturbation. Then one can quantize the two parts of the free Hamiltonian $H_0$ individually, i.e. one may consider separately the quantum problem of the atoms with the Hamiltonian $H_{atoms}$ (at that time only the non-relativistic Schrödinger equation was available for this purpose) and the radiation field Hamiltonian $H_{field}$. When the perturbation is taken into account, the states of the atoms and the electromagnetic field are no longer stationary and transitions between different states become possible. Hence one can describe the emission or absorption of light with the help of the standard methods of quantum perturbation theory.

Now, expanding the wavefunction of the interacting system (i.e. of the radiation and the atom, the interaction between the two being approximated by that between the electric field and the electric dipole moment of the atom) into free radiation modes, Dirac chose the following dynamical variables ($r$ labels the modes):

$$b_r = N_r^{1/2} e^{-i\theta_r/h} \quad \text{and} \quad b_r^\dagger = e^{i\theta_r/h} N_r^{1/2}, \tag{6.4}$$

where the dagger denotes the Hermitean conjugate, $N_r$ the absolute square of the Fourier coefficient $b_r$, and $\theta_r$ is a phase variable conjugate to $N_r$. For the $b$s he assumed the commutation relations

$$b_r b_r^\dagger - b_r^\dagger b_r = 1, \tag{6.5}$$

all others being zero. The $N_r$ take on only integral values, larger than or equal to zero. Dirac recognized the nature of $b$ and $b^\dagger$ as annihilation and creation operators, showing that the interaction of the atom and the radiation causes transitions of photons with energy $E_r$ into those with energy $E_s$. By calculating the matrix elements for these transitions, Dirac obtained Einstein's $A$ and $B$ coefficients as functions of the interaction potential.

As an indirect consequence of his theory, Dirac arrived at a completely new picture for the vacuum. After Einstein had abolished the concept of the ether, the matter-free and field-free vacuum was considered as an entirely empty space. But in quantum mechanics, because of Heisenberg's uncertainty principle, the electromagnetic field oscillators cannot be strictly at rest. As a consequence, even in the ground state with the lowest possible energy, there still exist the so-called zero-point oscillations of quantum oscillators of frequency $\omega$, having the energy $\frac{1}{2}\hbar\omega$. Hence the oscillatory nature of the electromagnetic field of radiation leads to the zero-point oscillations of this field in the vacuum state (the state of lowest possible energy). The physical vacuum is not an empty space, but is 'populated' with zero-point oscillations, which are the cause of the spontaneous emission of radiation from atoms. Thus Dirac's theory provided the explanation for all results regarding the emission and absorption of radiation by atoms.

The numerical results, derived from the Hamiltonian (6.2), when the interaction term (6.3) is treated as a first-order perturbation, were quite satisfactory from the point of view of the experimental situation at that time. Some other radiative processes, such as the non-relativistic Compton scattering of photons by electrons, had been calculated with the help of Dirac's radiation theory, in good agreement with experimental data in the second order of perturbation theory, where they first appeared.[14] This theory was also able to explain the natural width of spectral lines, which had been calculated by Eugene Wigner and Victor F. Weisskopf.[15] However, there arose certain difficulties when one tried to calculate higher-order approximations in perturbation theory with the Hamiltonian (6.2). A year after Dirac's paper on 'second quantization,' Pascual Jordan, Oskar Klein, and Wigner developed a similar scheme for Fermi fields.[16] One might wonder why Dirac himself did not proceed in this direction: on the one hand, he wanted to deal with the radiation problem, and had therefore to apply Bose statistics; on the other, he could not as yet deal with the electron field in the same relativistic manner as with the photon field.

The essence of Jordan, Klein, and Wigner's method consisted in considering simultaneously the matter-particle field, given by the corresponding wavefunction, as an operator field, as Dirac had already done with the field of photons. Thus it became possible to deal with the corresponding creation and annihilation operators for all kinds of quanta, and then one could express all physical quantities by these operators. But there appears a very important difference between the description of photons and electrons or, speaking more generally, between particles with integer or half-integer spin. It was well known by this time that photons have spin 1 and obey Bose–Einstein statistics, i.e. there can be an arbitrary number of such particles in the same state; they were called 'bosons.' In contrast, particles like electrons, with half-integer spin, obey Fermi–Dirac statistics, i.e. only one such particle occupies a given state in accordance with the Pauli exclusion principle; such particles were called 'fermions.' The nomenclature 'bosons' and 'fermions' was given by Dirac. Suppose the creation and annihilation operators of bosons are $b$ and $b^\dagger$, respectively, and the corresponding creation and annihilation operators for fermions are $a$ and $a^\dagger$. Then, in order to satisfy the corresponding statistics for a given mode, these operators must satisfy the following algebraic properties: the boson operators obey the commutation relation

$$[b^\dagger, b] \equiv b^\dagger b - bb^\dagger = 1, \tag{6.6}$$

whereas the fermion operators obey the anticommutation relation

$$[a^\dagger, a]_+ \equiv a^\dagger a + aa^\dagger = 1, \tag{6.7}$$

where 1 denotes the identity operator. These relations were first established for photons and electrons only.

## Relativistic quantum mechanics

Dirac recalled an occasion 'when I was in Copenhagen, that Bohr asked me what I was working on and I told him I was trying to get a satisfactory relativistic theory of the electron. And Bohr said, "But Klein and Gordon have already done that!" That answer first rather disturbed me: Bohr seemed quite satisfied by Klein's solution, but I was not because of the negative probabilities that it led to. I just kept on with it, worrying about getting a theory which would have only positive probabilities.'[17]

In 1926 Oskar Klein[18] had obtained a relativistic field equation for a scalar field by inserting quantum operators for momentum and energy in the equation

$$E^2 = p^2c^2 + m^2c^4, \tag{6.8}$$

in which one makes the operator replacements, $\mathrm{p} \rightarrow \frac{\hbar}{\mathrm{i}}\nabla$ and $E \rightarrow \mathrm{i}\hbar\frac{\partial}{\partial t}$ when this acts on a field $\psi$. The resulting equation was also independently discovered by Walter Gordon[19] in Hamburg, and is now referred to as the Klein–Gordon equation. The difficulties which perturbed Dirac were connected with two questions. First, if one used the Klein–Gordon equation for a single particle and interpreted the expression

$$\psi^*(x)\frac{\partial\psi(x)}{\partial t} - \psi(x)\frac{\partial\psi^*(x)}{\partial t} = \rho(x) \tag{6.9}$$

as the probability of finding this particle at a certain place (since the total probability, $\int \rho(x)\,\mathrm{d}^3x$, is conserved), then one could have a negative probability.* Secondly, Dirac had already set up the transformation theory in its general form which was a very powerful tool, and he felt that it was not only correct, but had to be preserved and brought into harmony with relativity. To achieve the latter goal he needed an equation linear in time.

Dirac started 'playing with equations rather than trying to introduce the right physical idea. A great deal of my work is just playing with equations .... It is my habit that I like to play about with equations, just looking for mathematical relations which maybe do not have any physical meaning at all.'[20] By 'introducing the right physical ideas' Dirac meant the idea of spin. The spin of the electron had already been introduced by George E. Uhlenbeck and

* Since $\psi$ refers to a complex Klein–Gordon field, one nowadays would prefer to call $\rho(x)$ the charge density rather than the particle density; the former of course need not be positive.

Samuel Goudsmit in 1925, to explain the doublet structure of the single electron spectra without the 'hypothesis of non-mechanical stress'; Pauli had developed the theory of the spinning electron further and described the electron by a two-component wavefunction, which could be used for explaining the empirical spectral data using a non-relativistic Schrödinger equation.[21]

Dirac's intention was to go beyond such an approximation. A scalar product in three dimensions could be formed from Pauli's $\sigma$-matrices and the momentum, and Dirac wanted to extend it to four-dimensional space–time. After several weeks of concentrated effort he discovered the simple solution that he could do so by generalizing the $2 \times 2$ $\sigma$-matrices to $4 \times 4$ matrices, which he called $\gamma$-matrices. From the generalization of the $\sigma$-algebra, it naturally followed that the $\gamma$s should anticommute. In his derivation of his new wave equation, Dirac had set things up in the absence of an electromagnetic field.[22] The homogeneity of space and time required that the coefficients of the momenta were independent of space and time, and he thus obtained,

$$\left(\mathrm{i}\sum_{\mu=1}^{4}\gamma_\mu p_\mu + mc\right)\psi = 0, \tag{6.10}$$

where $p_\mu = -\mathrm{i}\nabla_\mu$ and

$$\gamma_\mu^2 = 1, \quad \gamma_\mu\gamma_\nu + \gamma_\nu\gamma_\mu = 2\delta_{\mu\nu}. \tag{6.11}$$

In the same paper, received by the editor on 2 January 1928, Dirac then introduced an arbitrary electromagnetic field and replaced the components of the four-momentum by its gauge-covariant extension, $p_\mu \to p_\mu - \frac{e}{c}A_\mu$. Finally he used his equation to describe the motion of electrons in a centrally symmetric field, giving a treatment of the hydrogen spectrum.

In his second communication on 'The quantum theory of the electron,' submitted a month after the first one, Dirac proceeded to calculate the states of the hydrogen atom in his new theory.[23] He started by giving the proof that 'the change of probability of an electron being in a given volume during a given time is equal to the probability of its having crossed the boundary. This proof ... is necessary before one can infer the theory will give consistent results that are invariant under a Lorentz transformation.'[23]

The results of the new theory were later summarized in a lecture which Dirac presented at the *Leipziger Universitätswoche* in June 1928.[24] It followed from his theory for alkali-atom spectra that the electron had to have a spin of magnitude $\frac{1}{2}\hbar$. The new classification of spectra was determined by the total angular momentum which is the sum of the intrinsic spin and the orbital angular momentum. The selection rules were not changed. The new theory also yielded Sommerfeld's fine-structure formula.[25] Dirac himself had thought that

'If I got anywhere right with the approximation method, I would be very happy about that. I would have been too scared myself to consider it exactly because it might have given unfortunate results that would compel the whole theory to be abandoned.'[17] However, Walter Gordon and C. G. Darwin solved the problem exactly.[26] In his Leipzig lecture, Dirac also mentioned the problem which had bothered him the most. If one writes the wave equation with $-e$ instead of $e$ (electron charge), one would expect something completely new, a negative energy state, and he speculated that it might refer to the proton. The equation, however, did not yield such a heavy partner to the electron. He concluded at the time that, if there were no transitions between the $+e$ and the $-e$ solutions of the equation, it was not too bad. In his theory, the transition probability turned out to be finite, albeit very small, being of the fourth order in $v/c$, where $v$ is the velocity of the electron. The theory could therefore only be an approximation to Nature; one probably had to change the concepts entirely, even bringing in an asymmetry of the laws between past and future.

Dirac knew that his relativistic theory was still imperfect, and in a paper on 'A theory of electrons and protons,' he explicitly gave the explanation.[27] The wave equation had, in addition to 'solutions for which the kinetic energy of the electron is positive, an equal number of unwarranted solutions with negative kinetic energy for the electron, which appear to have no physical meaning.'[28] By examining the wavefunction of a negative energy solution in an electromagnetic field, Dirac found that it behaved like a particle with positive charge. But this connection would not solve the problem if one did not also have the fact that the electrons obey the exclusion principle. He could therefore assume that 'there are so many electrons in the world that the most stable states are occupied, or more accurately that all states of negative energy are occupied except perhaps a few of small velocity.'[29] Dirac argued that the transition of electrons from states with positive energy to those with negative energy was highly suppressed, and only the unoccupied negative states, the 'holes,' could be observed. He assumed that 'the holes in the distribution of negative energy electrons are the protons. When an electron of positive energy drops into a hole and fills it up, we have an electron and proton disappearing together with emission of radiation.'[30] In a following note in the *Proceedings of the Cambridge Philosophical Society* he calculated the annihilation rate,[31] and in a paper read before the British Association Meeting at Bristol on 8 September 1930 Dirac summarized his results.[32] Matter consists, he said, of 'electrons and protons,' and the existence of protons 'follows from the relativistic wave equation.' There remained a difficulty with this interpretation: in his theory Dirac could calculate the transition probability for the annihilation process only under the 'approximation' that the masses of the electron and proton were equal, and the resulting amplitude was several orders of magnitude higher than that suggested by empirical evidence on electron–proton annihilations.[33] In spite of this problem Dirac had faith in the essential correctness of his interpretation of the wave equation.

After Dirac's publication of the electron wave equation in 1928, many people took up its study. Erwin Schrödinger gave an interpretation of the spin properties of a particle in terms of rapid oscillations or '*Zitterbewegung*.'[34] Dirac knew that he had to go still further in order to make the physical interpretation consistent. There was the problem with negative energy states, for which he did not propose a solution until 1930: 'It was an imperfection of the theory and I didn't see what could be done about it. It was only later that I got the idea of filling up all the negative energy states.'[17] Then there were the unequal masses of the positively and negatively charged particles which existed in Nature. 'I felt right at the start that the negative energy electrons would have the same rest-mass as the ordinary electrons.... I hoped that there was some lack of symmetry somewhere which would bring in the extra mass for the positively charged ones. I was hoping that in some way the Coulomb interaction might lead to such an extra mass, but I couldn't see how it could be brought about.'[35] After Hermann Weyl's careful investigations proved that the new particles formed by these holes must have the same mass as the electrons, Dirac gave up the idea that the positively charged hole was a proton: 'It thus appears that we must abandon the identification of the holes with protons and must find some other interpretation for them. A hole, if there were one [in the world], would be a new kind of particle, unknown to experimental physics, having the same mass and opposite charge to an electron. We may call such a particle an anti-electron. We should not expect to find any of them in Nature, on account of the rapid rate of recombination with electrons, but if they could be produced experimentally in high vacuum they would be quite stable and amenable to observation. An encounter between two hard $\gamma$-rays (of energy of at least half a million volts) could lead to the creation simultaneously of an electron and anti-electron. This probability [of the creation of a pair] is negligible, however, with the intensities of $\gamma$-rays at present available.'[36]

Then on 2 August 1932 there appeared the announcement of the discovery of the positron by Carl D. Anderson,[37] and shortly thereafter by P. M. S. Blackett and G. P. S. Occhialini.[38] The antiproton was discovered 23 years later.[39] Thus the resolution of the question about the negative energy states turned out to be one of the greatest discoveries of the twentieth century: the discovery of the existence of antimatter. In quantum electrodynamics, however, completely new processes are possible involving antiparticles. A negative energy electron may be lifted up into a positive energy state, if it is given enough energy, for example by the absorption of light quanta. This process would look like the creation by a photon of an electron with positive energy and a hole in the negative energy sea, i.e. the positron. In the 'annihilation,' these particles disappear and their combined energy is radiated away as photons. It was not hard to calculate the probability of annihilation of an electron and a positron into two photons;[40–42] moreover, the cross-section for pair creation (electron and positron) by photons in the Coulomb field of atomic nuclei could also be readily calculated.[43] The theoretical results for radiative scattering and the creation of pairs were confirmed by experiments with cosmic-ray cascade showers of matter, once the incoming energy is transformed into electrons, positrons, and photons.

Dirac's interpretation of the negative energy states in the solution of the Dirac equation for the electron forced physicists to arrive at the following important conclusions. No one-particle systems exist in Nature, nor even few-particle systems; these are concepts that belong only to non-relativistic theory. In relativistic quantum electrodynamics one must take into consideration the infinite number of electrons and positrons in the vacuum. The pair production and annihilation of electron–positron pairs leads to a theory in which the particles of matter must be considered as the quanta of the corresponding field, just as the photons are the quanta of the electromagnetic field.

A new picture of forces between particles appears in quantum field theory. We can understand the interaction between two charged particles at a distance as an exchange of virtual photons, which continuously pass from one charged particle to another. These exchanged virtual particles are not directly observed as particles because of the conservation of energy, but, according to Niels Bohr's extension of Heisenberg's uncertainty principle, $\Delta E \Delta t \geq \hbar$, such an exchange is possible for short enough time intervals. Hence the virtual particles can be created for a very short time in the intermediate states of the physical processes, but they must be absorbed quickly enough. As a result, the charged particle is surrounded by a cloud of virtual photons. The latter can produce other virtual particles, such as electrons and positrons, by means of pair creation in the vacuum, and then the electrons and positrons thus created must annihilate each other very quickly to preserve energy conservation within the limits of the uncertainty principle. Thus the cloud around the charged particle consists of photons, electrons, and positrons.

As we see, in the new relativistic quantum theory one has quite a complicated picture of the physical vacuum, of physical particles, and of physical interactions. A corresponding mathematical formalism to describe this complicated physical picture, involving infinitely many particles, was needed.

## Heisenberg, Pauli, Fermi, and Dirac's relativistic theory

Three years before the discovery of the positron Heisenberg and Pauli—in two papers '*Zur Quantenmechanik der Wellenfelder*'[44] and '*Zur Quantenmechanik* *der Wellenfelder II*'[45] of 29 March and 7 September 1929, respectively—took a decisive step forward to develop a consistent theory of quantum electrodynamics. Heisenberg and Pauli started with an unspecified relativistically invariant classical field theory with a scalar Lagrange density, which was supposed to be a local function of the fields and their first derivatives. Following a well-known procedure from classical analytical dynamics, they introduced momenta conjugate to the fields, and eliminated the temporal derivatives of the fields by a Legendre transformation. By subjecting the fields and their conjugate momenta at equal times to the Heisenberg commutation relations, there arose the quantum theory corresponding to the classical theory. This procedure, with its unsymmetrical treatment of time and space, was very far from being manifestly relativistically invariant; it was a great achievement therefore to prove that, despite its non-covariant nature, Lorentz invariance was not destroyed by this canonical quantization. The proof given by Heisenberg and Pauli in their first paper was so complicated that, following the ideas of John von Neumann, they replaced it by a simpler one in their second paper.

Heisenberg and Pauli then applied the result to quantum electrodynamics, more specifically to a system of a finite number of Dirac electrons interacting with the electromagnetic field. To do this certain modifications were required. First, the canonical quantization of the Dirac field leads to Bose–Einstein statistics; it has to be replaced by Jordan–Wigner quantization.[16] Second, and more disturbingly, the electromagnetic field does not quite fit canonical quantization, since one of the canonical momenta vanishes identically. This is due to the vanishing rest-mass of the photon or, equivalently, due to the presence of the electromagnetic gauge group. Therefore quantum electrodynamics was considered in Heisenberg and Pauli's papers as the limiting case of $\epsilon = 0$ of a family of theories which depend on a parameter $\epsilon > 0$ and which was not subject to the problem of a vanishing canonical momentum. This was done by adding a term $-\frac{1}{2}\epsilon(\partial_\mu A^\mu)^2$ to the Maxwell Lagrangian, which, of course, spoils gauge invariance.

Heisenberg and Pauli were well aware of the shortcomings of their theory: the divergence difficulties (which we will discuss in the next section) and the problem of negative energies for the electron. However, the importance of the Heisenberg–Pauli theory cannot be exaggerated; it opened the road to a general theory of quantized fields and thereby prepared the tools, albeit not perfect ones, for the Pauli–Fermi theory of $\beta$-decay and for the meson theories. However, the Heisenberg–Pauli theory did not meet Dirac's approval, as we shall now see.

Enrico Fermi, who formulated a quantum theory of radiation in Rome during the winter of 1928,[46, 47] gave a simple solution of the problem of how to divide the total electromagnetic field into a Coulomb field (which binds the electron to the nucleus) and the radiation field. As a result of a fundamental theorem, it is possible to represent the three-vector potential A as a sum of two terms: $A = A_{\parallel} + A_{\perp}$. The so-called transverse field $A_{\perp}$ has a zero divergence: $\operatorname{div} A_{\perp} = 0$. Its Fourier-transformed three-vector is orthogonal to the direction of propagation of the electromagnetic field. Fermi proposed to connect the radiation field 'with the transverse field $A_{\perp}$, and the Coulomb field with the longitudinal field $A_{\parallel}$.' This procedure leads to the simple and physically clear picture of the quantum theory of particles and the electromagnetic field,[48] and many young physicists, including Richard Feynman, learned quantum mechanics from Fermi's famous paper entitled 'The quantum theory of radiation,'[48] based upon his lectures at the University of Michigan Summer School at Ann Arbor, Michigan, in 1930. Although Fermi used Dirac's relativistic equation for the electron, his approach was not completely relativistically invariant, thereby spoiling the so-called gauge invariance of electrodynamics. Indeed, the condition $\operatorname{div} A_{\perp} = 0$ is that for a special type of gauge, called a 'Coulomb gauge,' which spoils gauge invariance, besides not being relativistically invariant. This leads to difficulties when one tries to develop the relativistically invariant perturbation theory, because in Fermi's approach the transverse and the longitudinal parts of the electromagnetic field were treated differently from each other, and the separation of these parts is relativistically not invariant. It was clear, however, that, in principle, this difficulty was only a formal one; the physical results must, anyhow, be independent of the choice of gauge, but in the Dirac–Heisenberg–Pauli–Fermi theory this was not obvious.

In a paper entitled 'Relativistic quantum mechanics,' submitted to the *Proceedings of the Royal Society of London* in March 1932,[49] Dirac criticized the foundation of the Heisenberg–Pauli relativistic quantum theory of 1929, especially the assumption that the field could be regarded 'as a dynamical system amenable to Hamiltonian treatment and its interaction with the particles as describable by an interaction energy, so that the usual methods of Hamiltonian mechanics may be applied.' In particular, Dirac noted: 'There are serious objections to these views, apart from the purely mathematical difficulties to which they lead. If we wish to make an observation on a system of interacting particles, the only effective method of procedure is to subject them to a field of electromagnetic radiation and see how they react. Thus the role of the field is to provide a means for making observations. *The very nature of an observation requires an interplay between the field and the particles.* We cannot therefore suppose the field to be a dynamical system on the same footing as the particles and thus something to be observed in the same way as particles. The field should appear in the theory as something more elementary and fundamental.'[50]

In contrast to Heisenberg and Pauli, Dirac assumed 'the field equations as [being] always linear,' hence 'deep-lying connections and possibilities for simplification and unification' may be reached.[50] In any case, he concluded that

that 'quantities referring to two initial fields, or to two final fields, are not allowed,' because they 'are unconnected with results of observations and must be removed from consideration if one is to obtain a clear insight into the underlying physical relations.'[51]

Dirac's new proposal deviated from the procedure which followed from the classical theory—such as 'assuming a definite structure of the electron and calculating the effect of one part of it on the field produced by the rest'[51]—by taking into account the influence of both the incoming and the outgoing fields, such 'that we may associate, say, the right-hand sides of the probability amplitudes [for the quantities of the relativistic theory] with ingoing fields and the left-hand sides with the outgoing fields. In this way we automatically exclude quantities referring to two ingoing fields, or two outgoing fields and make a great simplification in the foundations of the theory.'[52]

The interaction of an electron with a given electromagnetic field is given by

$$F\psi = 0, \tag{6.12}$$

where $\psi$ is the electron field, assumed spinless for simplicity, in which case $F$ is just the Klein–Gordon operator

$$F = \left(i\hbar\frac{\partial}{\partial t} + eA_0\right)^2 - \left(i\hbar c\frac{\partial}{\partial x} - eA_x\right)^2 - \cdots - m^2c^4 \tag{6.13}$$

(with $e$ and $m$ denoting the charge and mass of the electron). In the special case of interaction between two electrons the $\psi$ must then satisfy two equations with the respective operators $F_1$ and $F_2$ depending only on the coordinates of the first and second electron, respectively. The interaction manifested itself only in the functions $\psi_1$ and $\psi_2$, each satisfying Eqn (6.12), but 'neither of the products $\psi_1\psi_2$ and $\psi_2\psi_1$ will satisfy both equations [(6.12)].'[53] Dirac finally demonstrated in a simplified example—two electrons in one space dimension—that the usual result of (the Heisenberg–Pauli) quantum electrodynamics was also obtained in the new theory.

Dirac eagerly presented his new approach to relativistic quantum field theory—the first he had proposed since his pioneering work five years earlier on the relativistic theory in 1927[10]—both to Heisenberg and to the other members of Bohr's Institute in Copenhagen (where he visited in April 1932). The official published response to Dirac's work was given by Léon Rosenfeld in a paper submitted from Copenhagen to *Zeitschrift für Physik* in May 1932: 'The Heisenberg–Pauli quantum mechanics represents a possible formulation of the program of relativistic quantum mechanics proposed recently by Dirac,' and showed that the two theories were equivalent.[54,*] Yet Paul Dirac, though he admitted the mathematical equivalence of both theories—'The connection which you give between my new theory and the Heisenberg–Pauli theory is, of course, quite general'[56]—strongly insisted upon the physical difference and continued to think about and work upon it. When he attended the Leningrad conference on the theory of metals, organized by his friend Igor Tamm in September 1932, Paul Dirac not only mentioned it in his talk but also discussed the problem with two other participants, Vladimir Fock and Boris Podolsky. Together they submitted a joint paper, entitled 'On quantum electrodynamics,' to the *Physikalische Zeitschrift der Sowjetunion*.[57]

The Dirac–Fock–Podolsky investigation consisted of two parts, one devoted to a 'simplified proof' of the 'equivalence of Dirac's and Heisenberg–Pauli's theories,' while the other treated 'the Maxwellian case' in detail. The main aspect of the new theory of Dirac, Fock, and Podolsky lay in the fact that it allowed them to exhibit relativistic invariance more explicitly. Thus the Heisenberg–Pauli scheme described a system consisting of two subsystems, $A$ and $B$, by the Hamiltonian equation

$$\left(H - i\hbar\frac{\partial}{\partial t}\right)\psi(q_a, q_b, t) = 0, \tag{6.14}$$

with the Hamiltonian operator

$$H = H_a + H_b + V \tag{6.15}$$

(where $a$ and $b$ referred to the subsystems $A$ and $B$, respectively, with the position coordinates $q_a$ and $q_b$ and time $t$). In Dirac's new scheme, Eqn (6.14) had now to be replaced by

$$\left(H_a^* + V^* - i\hbar\frac{\partial}{\partial t}\right)\psi^* = 0, \tag{6.16}$$

* Pauli's response was scathing. In a letter to Dirac he said, 'Your recently published remarks in the Proceedings of the Royal Society concerning Quantum electrodynamics were . . . certainly no masterpiece. After a confused introduction, that consisted of only half understandable, because only half understood, sentences, you come finally to results in a simplified one dimensional example that are identical with those that the formalism of Heisenberg and I gives for that example. (This identity is immediately recognizable and has since been calculated in much too complicated a fashion by Rosenfeld.) This conclusion of your work stands in contrast to your more or less unambiguous assertion in the introduction that somehow you can construct a better quantum electrodynamics than Heisenberg and I.'[55]

where:

$$\psi^* = \exp\left(\frac{i}{\hbar}H_b t\right)\psi \tag{6.17}$$

$$F^* = \exp\left(\frac{i}{\hbar}H_b t\right)F\exp\left(-\frac{i}{\hbar}H_b t\right), \tag{6.18}$$

with $F = (H_a, V)$. Since $H_a$ commuted with $H_b$, $H_a^* = H_a$, and further

$$V^* = V(p_a, q_a, p_b^*, q_b^*). \tag{6.19}$$

Evidently, if the subsystem $A$ (having dynamical variables $q_a$ and $p_a$) represented the particle and $B$ ($q_b$, $p_b$) the Maxwellian field—as in Dirac's quantum electrodynamics of March 1932[49]—the $q_b^*$ and $p_b^*$ satisfied the free Maxwell equations, unperturbed by the presence of the subsystem $A$. Moreover, Dirac, Fock, and Podolsky found that Eqn (6.16) might assume the form

$$\left[\sum_s (H_s + V_s^*) - i\hbar\frac{\partial}{\partial t}\right]\psi^*(r_s; J, t) = 0, \tag{6.20}$$

where $\sum_s H_s$ denoted the sum of the particle contributions to the free Hamiltonian $H_s$. The particles then interacted with the electromagnetic field, such that $V^* = \sum_s V_s$ represented the sum of the interaction terms involving the field and the particles. In the wavefunction, $J$ stood for the variables of the field and $r_s$ for the space coordinates of the particles. Equation (6.20) now possessed a simpler solution if one introduced 'besides the common time $t$ and the field time an individual time $t_s = t_1, t_2, \ldots, t_n$ for each particle,'[58] namely

$$\left(R_s - i\hbar\frac{\partial}{\partial t_s}\right)\psi^* = 0. \tag{6.21}$$

where the Dirac Hamiltonian operator is

$$R_s = c\,\alpha_s \cdot \mathrm{p}_s + m_s c^2 \alpha_s^{(4)} + e_s[\Phi(r_s, t_s) - \alpha_s \cdot \mathrm{A}(r_s, t_s)], \tag{6.22}$$

and at the end, all the individual times $t_s$ are put equal to the common time. [Here the $\alpha$s are the Dirac matrices, and $\Phi$ and $\mathrm{A}$ are the scalar and vector potentials, respectively.]

Indeed, in spite of his dissatisfaction with the Heisenberg–Pauli quantum electrodynamics, Dirac and his collaborators were not able to change the situation effectively in the 1930s. However, the Dirac–Fock–Podolsky paper had an almost immediate impact on the then 14-year-old Julian Schwinger, who within a year or two read this paper and generalized it to the case where the charged particles were described by second-quantized Dirac field operators. Like Dirac, Schwinger had to discard the 'infinite self-energy of the charges' [0]. Equations (6.21) defined what was later called the 'many-time formalism' which was used especially by Sin-itiro Tomonaga many years later to formulate relativistic quantum electrodynamics.[59, 60] Shortly after his work with Fock and Podolsky, Dirac wrote a paper on 'The Lagrangian in quantum mechanics,[61] which had a profound influence on Richard Feynman's doctoral dissertation on 'The principle of least action in quantum mechanics' at Princeton,[62] and his later work on the formulation of the 'Space—time approach to quantum electrodynamics.'[63] This paper further formed the basis for Schwinger's development of the quantum action principle, Schwinger's final operator field formulation of quantum field theory, which he began developing in 1950, and which we shall describe in Chapter 9.

The commutation relations (6.6) and (6.7) were first established for photons and electrons only. In 1934, Wolfgang Pauli and Viktor F. Weisskopf investigated the electromagnetic interactions of spin-0 charged particles described by a scalar field governed by the Klein—Gordon equation.[64] They established that the scalar field must be quantized according to commutation relations (6.6) and that scalar particles do satisfy Bose—Einstein statistics. But in contrast to the photon field, the field now carries a charge, and the corresponding antiparticles exist as well. Pauli, who did not like Dirac's hole theory of positrons, was quite satisfied when Weisskopf proved that in the case of scalar particles one did not have to consider the sea of antiparticles. The reason was that this is simply impossible, since the scalar particles did not obey Fermi–Dirac statistics, so there is no exclusion principle. Since, in this paper, it was shown that Dirac's hole theory was not a universal approach to negative energy states in relativistic quantum mechanics, Pauli called this paper 'our anti-Dirac paper.'[65] This work led Pauli to the discovery of the general relation between spin and statistics in connection with the commutation rules expressed by equations (6.6) and (6.7).[66] The particles with spin 0, as well as particles with spin values other than $\frac{1}{2}$ or 1, were not known at that time, but Pauli's important discovery of the fundamental connection between spin and statistics became part of the basic general principles of quantum field theory. We will discuss the spin-statistics theorem in more detail, and Schwinger's contributions to the subject, in Chapter 11.

## The infinities in quantum electrodynamics

Despite the great achievements of the theory of quantum electrodynamics before World War II, there remained serious difficulties in it, besides the fact that the theory suffered from some inconsistencies, such as questions of gauge and relativistic invariance. These difficulties were connected with infinities of different kinds that appear in the theory.

Heisenberg[67] and Dirac[68] discussed the appearance of an infinite energy and infinite density of charge in a finite volume of three-dimensional space, arising from the physical properties of the vacuum in the new theory. The infinite energy was connected with zero-point oscillations of the electromagnetic field after its quantization, and the infinite charge appeared because of the Dirac sea, which was filled by the negative energy electrons with infinite density.

The resolution of these two difficulties turned out to be quite simple. Pauli first proposed the primitive solution of the infinite charge density problem by a redefinition of the charge and the current.[69] Considering the symmetry between electrons and positrons, we can take equal proportions of the electron sea and the positron sea, with the consequence that in the resulting theory the charge of the vacuum will be zero, since the vacuum charge of the electron sea will be exactly compensated by the vacuum charge of the positron sea.

J. Robert Oppenheimer and Wendell Furry gave this idea the right mathematical form.[70] They recognized that a proper ordering of the creation and annihilation operators in the quantum electron–positron Hamiltonian will lead to zero vacuum charge density and make Dirac's idea of a filled vacuum unnecessary. With the same ordering of the corresponding operators in the quantum Hamiltonian of photons, the zero-point vacuum energy also vanishes. This ordering of the creation and annihilation operators was called 'normal ordering,' and gives us a special kind of rule for the quantization of classical dynamical systems. It was important that this rule did not destroy the form of the quantum equations, nor the existence of vacuum fluctuations of the photon and the electron–positron field. The only effect of the normal ordering was the shift of the zero-point vacuum energy and the vacuum charge density to the correct zero values of these quantities.

In the resulting improved theory, not only was the picture of the vacuum simple again, but there were now only three fundamental interactions between the electrons, the positrons, and the photons: the scattering of the fermion with the emission or absorption of the photon, and the annihilation or creation of the electron–positron pair with the emission or absorption of the photon. For treating these first-order processes perturbation theory was perfect. But when one tried to calculate some more complicated processes in higher orders of perturbation theory one met difficulties, because other new infinities appeared. Already in 1930, Oppenheimer had first recognized that higher-order corrections in perturbation theory would lead to infinities.[71] He calculated the effect of the interaction between an atomic electron and the quantum electromagnetic field, and discovered that this interaction leads to the infinite shift of atomic energy levels. Later on, the investigation of higher-order terms was continued by Ivar Waller[72] and others.

Among the different divergences of this type, two were the most important. In 1934, Pauli had asked the young Viktor Weisskopf, who worked as his assistant at that time, to calculate the self-energy of the electron in the new relativistic quantum theory. In classical theory this self-energy is proportional to $e^2/a$, where $e$ is the charge and $a$ is the radius of the electron. Hence, for a point electron, the classical self-energy is infinite and diverges as the radius of the electron goes to zero. The quantum result is far less divergent, although in Weisskopf's original calculation, he obtained much too large an answer. This error was pointed out to Weisskopf by Wendell Furry, and after correcting his calculation Weisskopf obtained the correct expression for the self-energy of the electron in the relativistic quantum theory (in the second order of perturbation theory) in the form[73]

$$E_{\text{self}} = m_0 c^2 \left[ 1 + \frac{3}{2\pi} \frac{e^2}{\hbar c} \log \left( \frac{\lambda_c}{a} \right) \right], \tag{6.23}$$

where $m_0$ is the mechanical mass of the electron at rest and $\lambda_c (= h/mc)$ is its Compton wavelength. This formula showed that the self-energy of the point electron is still infinite in quantum electrodynamics as well, but as a result of the completely different nature of quantum processes this infinity is considerably weaker and the self-energy diverges only logarithmically as the 'cutoff' $a$, which plays the role played by radius of the electron in the classical theory, goes to zero. Another important conclusion derived from formula (6.23) is that, in quantum electrodynamics, the divergence is actually not in some sense a particularly physically relevant one. In fact, the logarithmic term in this formula will have the value of the order of the first term at distances of the order of $10^{-70}$ cm, which is extremely small compared with the Schwarzschild radius of the electron, which is only about $10^{-55}$ cm. At the latter distance the theory will surely be wrong, because one must take into account at least the gravitational force. Nevertheless, quantum electrodynamics itself is evidently not a consistent theory because of this divergence, and does not give unique predictions even at distances not so fantastically small. For example, changing the arbitrary cutoff $a$ by a factor of 2 changes $E_{\text{self}}$ by about 0.1%, a level then already detectable by experiments.

The second, physically important, divergence has another character. As a result of pair creation, the physical vacuum becomes a medium with dielectric properties. In the presence of a charged particle in the vacuum, virtual electron–positron pairs appear, and the induced cloud of such virtual pairs changes the value of the effective charge of the particle. The effective charge depends on the distance $r$ from the particle and has the form $e(r) = e/\epsilon(r)$, where $\epsilon(r)$ is the dielectric coefficient of the vacuum. This coefficient was first calculated by Serber[74] and Uehling[75] in second-order perturbation theory in the Coulomb field. Heisenberg and Euler[76] and Weisskopf[77] obtained an exact expression for slowly varying static fields. The result for the Coulomb field was that, at large distances $r$, the effective charge behaves like

$$e_r \approx e\left[1 + \frac{3}{4\pi^{1/2}}\frac{e^2}{\hbar c}\left(\frac{\lambda_c}{r}\right)^{3/2}\exp\left(-2\frac{r}{\lambda_c}\right)\right], \quad \text{for } r \gg \lambda_c, \tag{6.24}$$

while for short distances

$$e_r \approx e\left[1 + \frac{2}{3\pi}\frac{e^2}{\hbar c}\log\frac{\lambda_c}{r}\right], \quad \text{for} \quad r \ll \lambda_c. \tag{6.25}$$

Equation (6.24) shows that at large distances one should see the particle with the usual charge $e$, but from Eqn (6.25) we note that at short distances the effective charge is logarithmically divergent. The reason is that the entire charge of the cloud of virtual particles in any finite volume around any charged particle such as an electron has an infinite value. In other words, because of pair creation, the vacuum has an infinite polarization near the charged particles. The range at which this polarization gives a change in the charge comparable to the charge seen at large distances is the order of $\lambda_c \exp(-\hbar c/e^2) \sim 10^{-70}$ cm, i.e. this effect is large at the same distance scale which we have discussed in connection with the self-energy of the electron, and at such short distances this theory is surely physically inapplicable. But, in spite of this difficulty, at larger distances the theory gives the usual right predictions. For example, at accessible distances a measurable change in the charges occurs—e.g. for experiments carried out at energies corresponding to the mass of the carrier of the weak force, the $W$ boson, around 100 GeV, $\alpha = e^2/\hbar c = 1/128$ rather than $1/137$, exactly as Eqn (6.25) predicts.[78]

Since one can reach very short distances only because of a very large energy of the particles, and the large energy of light quanta or photons corresponds to spectral frequencies far beyond the violet range, the two divergences mentioned above were called 'ultraviolet divergences.' The real situation in quantum electrodynamics was much more complicated, because the ultraviolet divergences also appeared in higher orders of perturbation theory, and before the work of Freeman Dyson,[79] one might expect new types of divergence in each order of perturbation theory, i.e. in each successive term in the infinite Taylor series expansion in powers of the coupling constant $\alpha = e^2/\hbar c \approx 1/137$. (Of course, this was not the belief of either Feynman or Schwinger.) The true behavior of the self-energy and the effective charge at very short distances still remains an open question, which cannot be solved by formulas like Eqns (6.23) and (6.25), which are but first approximations.

Finally, there also occurred certain infinities in quantum electrodynamics, which were called the 'infrared divergences.' Their physical meaning is entirely different. They are connected with the radiation of low-energy, or 'soft,' photons, hence the name 'infrared divergences.' The quantum electrodynamic result was that the charged particle emits infinitely many photons with zero frequency when it has an accelerated motion as, for instance, when an electron is scattered by a static electric field. In that case, the emitted energy does not vanish in the limit of the zero frequency of the light quanta, in congruence with the classical results for the electromagnetic radiation of charged particles. Felix Bloch and Arnold Nordsieck[80] and Pauli and Marcus Fierz[81] showed that the difficulty with the infinite number of zero-frequency photons, although a physical phenomenon, may be removed by a proper contact transformation of the theory.

Thus, after 1937, the only remaining problem for the theory was how to deal with the ultraviolet divergences. One very important obstacle in the struggle with these infinities was the absence of a relativistically invariant perturbation theory. The nonrelativistic perturbation theory led to different series in different coordinate frames, and it was difficult to understand the physical meaning of the results. The only exception was several papers by Ernst C. G. Stückelberg;[82] he gave a manifestly covariant formulation of field theory, which could have been the basis for a true physical theory. But, unfortunately, these papers 'were rather obscure, and it was difficult to understand them or to make use of his methods.'[83] (For an interesting technical review of what Stückelberg accomplished, see Ref. 84.)

## The earlier attempts to overcome the infinities in quantum electrodynamics

At the end of the 1930s, it was clear to all active theoreticians in the field that quantum electrodynamics was not in a good state, and that something radically new was needed to overcome the divergences. We will briefly describe some of the proposed ideas for the solution of the problems.

In 1937 John Archibald Wheeler[85] and, independently, in 1943 Werner Heisenberg[86] proposed giving up quantum field theory entirely and replacing it with an entirely new theory, in which instead of fields one must operate only with directly measurable quantities. This approach was described as an '*S*-matrix' theory. (This approach in fact had a renaissance in the 1960s, as we shall discuss in later chapters.)

In 1938, Heisenberg had proposed to introduce a new fundamental constant in the theory, called the 'fundamental length'.[87] His idea was that at distances less than the fundamental length, physical processes—and even geometry—are not the same as we hold them usually, and that the theory has to be changed radically at distances smaller than this fundamental length.

In 1942, Dirac proposed the notion of an 'indefinite metric,' which would introduce into quantum mechanics intermediate states with negative probability. Such states were not to be observable, but they might help in obtaining convergent corrections rather than divergent ones.[88]

During 1940–42, Feynman and Wheeler considered the possibility of completely eliminating the electromagnetic field as the carrier of interaction between charged particles, and of replacing it by an action-at-a-distance theory.[89, 90] Other revolutionary approaches, such as the Born–Infeld[91] nonlinear version of classical electrodynamics and Dirac's reconstruction of the classical theory of the point electron[92] were proposed to overcome the divergence difficulties of quantum electrodynamics, but nothing helped; the solution lay in an entirely different direction.

Léon Rosenfeld[93] had already discovered the infinite self-energy of the photon in quantum electrodynamics; it was due to the current fluctuations of the electromagnetic field in the vacuum. Here, for the first time, the notion of 'renormalization' procedure was used. The basic idea was that if the polarization of the vacuum was finite then its constant part would have been physically inessential, since no measurable physical effects would have been connected with such a constant polarization. Only the sum of the 'true' and the induced charge of the particle may be measured. It seems natural to ignore the constant part of vacuum polarization, too, in the case when it is infinite, and to take into account only the finite deviations from this constant part. As we noted above, such deviations were investigated by Serber,[74] by Uehling,[75] by Weisskopf,[77] and by Serpe,[94] a student of Kramers', but the early attempts to measure the corresponding very small effects were unsuccessful. The first steps in the right direction in dealing with the infinities of the theory were taken by Hendrik Kramers in his attempt to deal with the other ultraviolet divergence in connection with the self-interaction of charged particles. Kramers's idea was that, first, one has to overcome the difficulties in classical electrodynamics, and then, to build the quantum theory of it in accordance with the correspondence principle; one could expect to be free of the difficulties in the quantum case after they had been removed from the classical theory.

Kramers's program for the solution of this problem in classical theory was to use subtractions of the infinite quantities connected with the self-interactions of the charged particles with their own fields. Following the ideas of his teacher, Hendrik Antoon Lorentz, Kramers stated that the mass $m$ of the charged particle in its equation of motion, $mc^2\ddot{a}_\nu = eF_{\nu\mu}(a)\dot{a}^\mu$, is not the experimental mass, but some auxiliary 'bare' mass. Then the experimental mass $m_{\text{exp}}$ of the charged particle is the sum of the bare mass $m$ and the electromagnetic mass $\delta m_{\text{selfint}}$

which originates from the self-interaction of the particle with its own field, $F^{\text{self}}_{\mu\nu}$, and has an infinite value for a point particle,

$$m_{\text{exp}} = m + \delta m_{\text{selfint}}. \tag{6.26}$$

Insofar as the self-interaction of the particle with its own field, $F^{\text{self}}_{\mu\nu}$, has been taken into account by its equation of motion, we have to subtract, according to Kramers, the field $F^{\text{self}}_{\mu\nu}$ from the entire electromagnetic field in order to obtain the 'mass-renormalized' equation of motion. Hence this equation acquires the form

$$m_{\text{exp}} c^2 \ddot{a}_\nu = e F^{\text{ext}}_{\nu\mu}(a) \dot{a}^\mu, \tag{6.27}$$

where $F^{\text{ext}}_{\nu\mu} = F_{\nu\mu} - F^{\text{self}}_{\nu\mu}$ is the external field, i.e., the electromagnetic field without the field of the particle itself. Equation (6.27) has a remarkable property; it is written completely in terms of observable quantities and, despite the fact that the mass $\delta m_{\text{selfint}}$ in Eqn (6.26) is infinite, the mass $m_{\text{exp}}$ is supposed to be the finite observed mass of the particle, and then there are no infinite quantities in Eqn (6.27) at all.*

Kramers had had reservations about Dirac's theory of radiation and its later development, because he had insisted on a proper separation of bare mass and electromagnetic mass throughout the theory. Thus, according to this idea, the self-energy of the electron could be written in the form

$$E_{\text{exp}} = m_{\text{exp}} c^2 = mc^2 + W_V, \tag{6.28}$$

where $W_V$ is the electromagnetic self-energy of the electron in some external field $V$. Then $W_0 = \delta m_{\text{selfint}} c^2$ is the electromagnetic self-energy of the free electron with no external field $V$. Kramers argued that the difference between the two infinite terms, $W_V - W_0$, should lead to observable effects and may be finite.[94]

Another important example was the fluctuations of the electromagnetic field. The quantum averages, $\langle 0|E^2|0\rangle$ or $\langle 1|E^2|1\rangle$, of the square of the electric field $E$, both in the vacuum state $|0\rangle$ with zero photons and in the state $|1\rangle$ with one photon, are infinite. But according to the subtraction procedure of Kramers,

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* In the 1960s Sidney Coleman pointed out that classical renormalization was introduced in 1833 by George Green, who considered a pendulum moving in an incompressible fluid. The period of the pendulum is changed as though its inertial mass were augmented by half the mass of the fluid displaced. See also H. Lamb's book, and references therein.[95]

the difference $\langle 1|E^2|1\rangle - \langle 0|E^2|0\rangle$ is a finite and measurable quantity.[96] Kramers was able to calculate certain quantities of this type as early as 1940, but he did not actually carry through his program until 1947. He had published some of his ideas in 1938[97] and in his monograph on quantum mechanics in the chapters dealing with quantum electrodynamics.[98] However, these ideas of Kramers' were practically unknown* until after the Shelter Island Conference (1947) and the eighth Solvay Conference in Brussels.[96] Moreover, as a follower of Lorentz, Kramers had developed his ideas on the basis of the old nonrelativistic Lorentz model of the electron. Kramers emphasized the use of the correspondence principle, which, in his opinion, would have to lead to the right nonrelativistic quantum electrodynamics, and only after that did Kramers intend to develop the relativistic one. Such an approach did not turn out to be useful, for in it one missed dealing with important physical phenomena, such as pair production, vacuum polarization, and the other relativistic effects. Kramers, therefore, was himself not very successful in developing his own ideas concerning the renormalization procedure, the general concept of which, nevertheless, is now universally accepted to be the right way to overcome the divergences in quantum electrodynamics. Nevertheless, it was important that as early as 1937–38 Kramers had mentioned the possibility of making corrections to the predictions of the Dirac–Heisenberg–Pauli–Fermi quantum electrodynamics for the states of the electrons in atoms and in other physical phenomena.[97]

Certain important steps toward the new theory of quantum electrodynamics were made during wartime in Japan. In 1942, S. Sakata[100] proposed to overcome the divergences by introducing a neutral scalar field; in 1946, the same idea was independently developed by Abraham Pais.[101] Sin-itiro Tomonaga, D. Ito, and Z. Koba calculated cross-sections in the new theory, and discovered some mistakes in earlier calculations of these quantities in the work of S. M. Dancoff,[102] the rectification of which was quite essential for the development of the renormalization program. (We described Dancoff's misleading results in Chapter 3.) Tomonaga also succeeded in developing Dirac's multitime relativistic formulation of field theory. But because of the war all normal contacts and communications within the international scientific community were broken, and these achievements were completely unknown in the USA and Europe until Tomonaga wrote a letter to Oppenheimer after the Shelter Island Conference in the summer of 1947. The story of Tomonaga's heroic wartime efforts will be recounted in Chapter 8.

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* Schwinger had read Kramers' paper, but was 'repelled by it. I didn't think redefining things was to be done at the classical level.'[99]

## The earlier experimental evidence for the deviations from Dirac's theory of the electron

Already in 1937–38, experimental evidence had appeared which cast a shadow of doubt on the predictions based on Dirac's relativistic theory of the electron. William Houston and Robert C. Williams had found deviations from Dirac's theory of the hydrogen spectrum. According to Dirac's theory, the $2^2S_{1/2}$ and $2^2P_{1/2}$ levels of hydrogen must have the same energy. But the experimental evidence was against such a degeneracy in these levels.[103, 104] Simon Pasternack[105] reached the conclusion that the difference between the $2^2S_{1/2}$ and $2^2P_{1/2}$ levels might be represented as an upward shift of the $2^2S_{1/2}$ level by approximately $0.03\text{cm}^{-1}$ ($\sim 1000$ megacycles) relative to the $2^2P_{1/2}$ level. The effect was thus referred to as the Pasternack effect. The first attempts to calculate this effect theoretically were made by Fröhlich, Heitler and Kahn.[106] Although their calculations were in good agreement with the experimental data, they were incorrect, as was shown by Willis E. Lamb.[107]

The situation thus remained unclear, and it did not change during the years of World War II, when the attention of most physicists was directed to war-related scientific research problems. However, as a result of war-related projects, a great improvement in experimental devices and techniques took place during the war years. These achievements had a great influence on the post-war progress, both experimental and theoretical, of quantum electrodynamics.

## The post-war development and the Shelter Island Conference

The spectacular event in the development of quantum field theory after World War II was the emergence in the USA during 1947–49 of quantum electrodynamics as a theory in which reliable calculations could be performed. In the year following the end of the war, many American physicists had returned to purely scientific research problems. Many European physicists, who left Europe before or during the war, also continued to do their work in America. Immediately after the war, physicists wished to get away from the applications of science to engineering and technology and return to work on fundamental physical problems; this especially included the young, energetic, American-trained generation of theoretical physicists who needed to be brought into the mainstream of the research community. They, with their scientific elders, had been pioneers in making new devices, forging the technology of new weapons that led the Allies to victory in World War II. The government and society in the United States deeply appreciated the achievements of the scientists, especially the physicists, during the war, and when the war had been won, increasing support was forthcoming for research in fundamental science. New types of national scientific organizations and institutions, such as the Manhattan Project at Los Alamos and dozens of other laboratories, had sprung up; suddenly there was great activity in the form of conferences, symposia, scientific workshops, and seminars on specialized fields under the guidance of leading experts. The era of the lone scientist, working by himself in isolation, which had always been something of a romantic fiction, was now certainly over, and one of the most important new ingredients of success was to 'get these guys organized,' as Feynman used to say later.[108]

As a result of some conversations and exchange of letters between Karl K. Darrow, the permanent secretary of the American Physical Society, Duncan McInnes, a distinguished physical chemist at the Rockefeller Institute, Frank Jewett, then president of the National Academy of Sciences, and John Wheeler, then a young professor of theoretical physics at Princeton University, the idea was generated of organizing several small conferences on fundamental issues in several fields of science. The aim was to make an evaluation of the current status of the fundamental problems, and to have serious and critical discussions among the best experts on current specific topics of interest. (For a detailed account of the origin of these conferences, see Ref. 6.)

The first of these conferences devoted to physics took place on 2–4 June 1947 at Ram's Head Inn on Shelter Island, at the tip of Long Island, and the general theme of the conference was 'Problems of quantum mechanics and the electron.' This conference turned out to be a turning point in the development of quantum electrodynamics. As Feynman recalled later on: 'There have been many conferences in the world since, but I've never felt any to be as important as this . . . . The Shelter Island Conference was my first conference with the big men . . . . I have never gone to one like this one in peacetime.'[109] The participants in this conference were: Abraham Pais, Arthur Nordsieck, Bruno Rossi, David Bohm, Duncan McInnes, Edward Teller, Hans Bethe, Hendrik Kramers, Herman Feshbach, George Uhlenbeck, Gregory Breit, Isidor Rabi, John von Neumann, John Van Vleck, John Wheeler, Julian Schwinger, Karl Darrow, Linus Pauling, Richard Feynman, Robert Marshak, J. Robert Oppenheimer, Robert Serber, Viktor Weisskopf, and Willis E. Lamb, Jr.

In the New York *Herald Tribune* of 2 June 1947, one could read the announcement: 'Twenty-three of the country's best known theoretical physicists—the men who made the atomic bomb—gathered today in a rural inn to begin three days of discussion and study, during which they hope to straighten out a few of the difficulties that beset modern physics.'

Unlike the usual reports by participants in regular conferences, at the Shelter Island Conference there took place only extensive discussions following comprehensive talks of the discussion leaders; the three rapporteurs were Kramers, Oppenheimer, and Weisskopf. These leaders distributed in advance topics for discussion to the participants.

Weisskopf's topics concerned the difficulties of the theory of elementary particles: (A) the difficulties of quantum electrodynamics—self-energies and other infinities, modifications of the classical theory and of the formalism after quantization, subtraction formalism for deriving finite results in quantum electrodynamics, the high-energy limit of quantum electrodynamics; (B) nuclear forces and mesons—cosmic-ray experiments, beta decay; (C) proposed experiments, electron and proton accelerators, new machines. Weisskopf was quite pessimistic about the immediate development of the theory and finished his outline with the words: 'In view of the failure of the present theories to represent the facts and the small probability that this conference may produce a new theoretical idea, part (C) of this agenda (namely, the experiments) could become the most useful part of this conference.[110]

Oppenheimer's outline did not refer to the problems of quantum electrodynamics at all, but tried to adapt some field-theoretical methods to meson theory and compared the difficulties of the multiple scattering of mesons with the infrared divergences in the theory of radiation.[110]

Kramers's proposal concentrated on the difficulties of quantum electrodynamics since 1927: the divergences in second-order perturbation theory, the infinite shift of the spectral lines, the impossibility of describing a steady state of an atom in the radiation field, and the reaction of the radiation on the atomic particles. Then he outlined his own work on the renormalization of the mass and showed 'how an electron with experimental mass behaves in its interaction with the electromagnetic field.' Kramers stated that 'the infinite shift of spectral lines, with the Dirac Lagrangian, is immediately connected with the divergences of the electromagnetic mass for a point electron.'[110] Kramers had proposed the subtraction procedure, i.e. the mass renormalization described earlier, to obtain finite results for physical quantities. One of Kramers' very important conclusions, which he had mentioned already in 1937, was that 'as a result, we expect that the correction must be applied to the energy values of stationary states of the hydrogen atom as given in the Dirac theory of 1928.'[97]

During May 1947, rumors of an important new experiment on the level shift in the fine structure of hydrogen, performed at the Columbia University Radiation Laboratory, were spreading. The experiment had been done by Willis Lamb, together with his graduate student Robert Retherford, and the result was the first precisely established value of the (fine structure) shift between $2^2S_{1/2}$ and $2^2P_{1/2}$ levels in the spectrum of atomic hydrogen (see the previous section). Lamb presented the results of this experiment at the beginning of the Shelter Island Conference, and it became one of the central concerns of the conference. 'The results indicate clearly that, contrary to (Dirac's) theory, but in essential agreement with Pasternack's hypothesis,[105] the $2^2S_{1/2}$ state is higher than the $2^2P_{1/2}$ one by about 1000 Mc/sec (0.033 $\text{cm}^{-1}$, or about 9 percent of the spin relativity doublet separation).'[111] The experiment became possible owing to 'the great wartime advances in microwave techniques in the vicinity of three centimeters wavelength.'[111]

Another important experimental result, also reported on the first day of the Shelter Island Conference by Rabi, was from his work of Nafe and Nelson, and that of Kusch and Foley, on the hyperfine structure of hydrogen, deuterium, and more complex atoms. These experiments gave indications that another discrepancy existed between Dirac's theory of the magnetic moment of the electron and its experimental value. In the Dirac theory, the gyromagnetic ratio for the electron is $g_s = 2$. But the experimental value obtained by Foley and Kusch, for example, was $g_s = 2.00244 \pm 0.00067$.[112] It was not clear where such a small discrepancy was coming from and how one could modify Dirac's relativistic theory of the electron, one of the great triumphs of which had been the explanation of the magnetic moment of the electron. The number $g_s$ is a fundamental characteristic of the electron. It gives the relation between the spin S of the electron, and its magnetic moment $\mu$:

$$\mu = \frac{e}{2mc} g_s \mathrm{S}. \tag{6.29}$$

The factor $e/2mc$ is the so-called Bohr magneton, the natural unit of the magnetic dipole moment for a particle of mass $m$ and charge $e$.

The talks on the problems of quantum electrodynamics by Weisskopf and Kramers took place on the second day of the conference, while the third day concentrated on phenomenological concerns. The highlight there was Robert Marshak's two-meson hypothesis,* about the existence of the pi-meson, which was discovered soon thereafter. Moreover, on that last day Feynman presented his space–time approach to quantum mechanics, which we shall discuss in Chapter 8. But the most important discussions took place in the domain of quantum electrodynamics, a field in which immediately after the conference great advances in the theoretical understanding of the physical processes were achieved. Robert Oppenheimer declared: 'The developments, which could have been carried out at any time during the last fifteen years, required the impetus of experiments to stimulate and verify.'[3]

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# Quantum electrodynamics and Julian Schwinger's path to fame

## Julian Schwinger and the Shelter Island Conference

In the beginning of June 1947 there took place the Shelter Island Conference on the fundamental problems of quantum mechanics, of which J. Robert Oppenheimer was the acknowledged leader. As Schwinger recalled later, 'In spring 1947 I got this invitation from Oppenheimer to attend the Shelter Island Conference, and Vicki Weisskopf and I traveled together. Since we had heard only rumors in Cambridge about the measurement of the Lamb shift, we must have talked a little about it and other topics of immediate interest, but our discussions on the train paled in the light of discussions at Shelter Island; we were overwhelmed by what was disclosed experimentally at the Shelter Island Conference. I don't remember about the order of speakers. Obviously, Willis Lamb talked. It was not a surprise except that we were now presented with a definite number: [nine] percent, or whatever it was, of the accuracy of the day. That did not change anything; I was only a listener and I may have made some brief comments because Weisskopf and I had discussed on the train what we knew about Willis Lamb's work. It had been suggested since 1939 that there was something in optical spectroscopy that indicated that the 2*S* level was shifted, but there was nothing very precise. So Lamb was providing a relatively precise measurement of this effect, but it was not new. I hadn't really thought about it until Weisskopf and I got together and approaching Shelter Island we talked about it.'[1]

'Simon Pasternack's name had been attached in connection with this problem,[2] but I had not thought about it at all. The question of infinities loomed large in everybody's mind. In fact, the prevailing opinion was that quantum electrodynamics was intrinsically wrong and needed to be modified. Pasternack must have invented some new interaction that was designed to explain this shift, but I'm not quite sure of that. It would be the automatic thing for anybody to do.'[1] During his stay at Berkeley, Schwinger was not particularly attracted to work on field theory, especially quantum electrodynamics, for he was affected by Oppenheimer's attitude that the latter was intrinsically wrong and would break down at some point, despite the beginnings of the development of renormalization theory and the possibility of being able to manage infinities in QED. 'I had read all the suggestions on changing things. Paul Dirac, in his Bakerian lecture of 1942, explored the possibility of introducing the indefinite metric.[3] There were all kinds of attempts to change quantum electrodynamics. I think I was entirely neutral on the subject.'[1]

After his intensive work on the theory of waveguides at the MIT Radiation Laboratory and return to the field of nuclear physics at Harvard, Schwinger was about to confront the problems of quantum electrodynamics at Shelter Island. 'To get back to this conversation with Weisskopf: then we began to think that perhaps electrodynamics by itself without any further modifications might be sufficient to account for this, and that's the spirit in which I listened and proceeded. (I do not recall actually saying anything at Shelter Island, but Bethe acknowledges such remarks.[1]) I thought that probably the relativistic quantum mechanics of Heisenberg, Pauli and Dirac[4] could do the trick. That was, I think one could say, a conservative approach .... The conservative approach to a new experiment is to ask how much of this can be understood on the basis of what you really know.'[1]

On the other hand, the other experimental news at Shelter Island was a complete surprise. '[Isidor I.] Rabi's talk (on the measurement of the anomalous magnetic moment of the electron) [by J. E. Nafe, E. B. Nelson, and Rabi[5] and by P. Kusch and H. M. Foley[6]] was a total shock and we had some general idea that it was an electrodynamic effect. While the announcement of Lamb's measurement did not change anything, the news about the measurements of Kusch and Foley was something that indicated that there was an extra effect beyond the Dirac theory, because it did not fit the experimental data. That it could be explained by an anomaly in the magnetic moment came only later—but I like to think that was my immediate interpretation. I'm not sure. I know that Gregory Breit did publish such a suggestion rather quickly; unfortunately, he got rather wrong details of it, for he did not correctly draw the consequences of his empirical hypothesis.[7] He arrived at a value of the additional magnetic moment about five times larger than what more direct experiments, not to mention the relativistic electrodynamic theory, would disclose. But by that time I was well along with my own calculations.'[1]

Thus the two big revelations of the Shelter Island Conference were the measurement of the Lamb shift and what would ultimately be interpreted as the anomalous magnetic moment of the electron. 'One was, shall we say, an electrical anomaly; the other was clearly or possibly a magnetic anomaly, and it certainly was stimulating to see, and natural to wonder, what any theory would do to account for both together. To me it seemed quite obvious that the magnetic anomaly would be much simpler. So my attention did not go immediately to the electrical anomaly, but they came hand in hand and the theory would have to account for them both.

'At Shelter Island I met [Richard] Feynman again; of course I had seen him at Los Alamos in summer 1945, and then we saw each other at the Conference early in June 1947. He came from Cornell, and had been isolated in that backwater all the time. Our trajectory had not intersected at all. At Shelter Island he did not say much at all. He may have; I didn't, I don't think. But that was the first time I actually met Feynman and that was my first discussion of the anomalous magnetic moment publicly with a preliminary estimate of what that number would be.'[1] (Actually, Feynman and Schwinger had met in Los Alamos in 1945, but they did not talk extensively. See Chapter 4.)

At the Shelter Island Conference, Julian Schwinger also had his first encounter with Stephen White, who was then a science reporter for the New York *Herald Tribune*. 'I recall our strolls outside the Ram's Head Inn at Shelter Island. He was curious about what was going on and I did my best to explain it to him at the time. I also remember the night before when we were all taken to dinner on the mainland and fed what was a Long Island shore dinner, I believe, it consisted of clams and this and that. As for my steak and potato orientation: Well, that was coming out of my shell! I don't recall having any discussions with Willis Lamb. I also don't recall being particularly struck by what [Robert] Marshak considers his finest hour in which he suggested the two-meson theory.

'After the Shelter Island Conference, at which Weisskopf and I agreed that these were electromagnetic effects, we went our separate ways to actually do the calculating. I think we compared notes from time to time. I was excited by the reports of new experimental results at the Shelter Island Conference, but I was distracted from any immediate attention to those problems by the act of getting married, which followed three days later.'[1]

At the end of the Shelter Island Conference, Oppenheimer and Schwinger took a seaplane from Port Jefferson to Bridgeport, Connecticut, where a connection to the railroad could again be found. (Schwinger may have taken a plane all the way to Boston to save time because of his approaching wedding.) Schwinger returned feeling very unwell. We recall that the return from the Shelter Island meeting marked a major change in Schwinger's habits. 'I had been a heavy smoker up to that time, probably due to the baleful influence of Oppenheimer, who set the model for everybody. I reproached myself for following that particular habit. At the Shelter Island Conference I had a severe stomach upset just before leaving, and had wondered whether the wedding ceremony would have to be postponed. Actually I thought that sickness was a secret I kept to myself, but of course I told Clarice later. I'm sure that all that smoking and change of food made the gastric contents of my stomach too acidic; well, I clearly associated that with smoking and instantly decided to stop, which I did. Of course, my good wife tells everybody that she gets the credit for it. It was my good sense to put a stop to all that agony.'[1]

Julian recovered quickly and the wedding went ahead as planned. A few days after their wedding, Julian and Clarice Schwinger left for a two-month-long honeymoon, visiting all the places where Julian had lived or visited earlier and where he had friends, and he had a complete rest from physics, as we recounted in Chapter 5. This delayed his extraordinary response to the news from Shelter Island, but, of course, the rest may have been just what was needed for a resurgence of his energy.

## Hans Bethe's calculation of the Lamb shift

Right after the Shelter Island Conference, on the train from New York City to Schenectady, New York, where he was a consultant at General Electric's Research Laboratory, Hans Bethe made his famous calculation of the Lamb shift, which he completed fully upon arrival in Schenectady. In his Nobel lecture, Feynman remarked: 'Professor Bethe, with whom I was then associated at Cornell, is a man who has this characteristic: If there's a good experimental number you've got to figure it out from theory. So, he forced the quantum electrodynamics of the day to give him an answer to the separation of these two levels ($2^2S_{1/2}$ and $2^2P_{1/2}$ levels of the hydrogen atom)... and thus made the most important discovery in the history of the theory of quantum electrodynamics.'[8]

At the Shelter Island Conference, 'Schwinger and Weisskopf, and Oppenheimer have suggested that a possible explanation might be the shift of energy levels by the interaction of the electron with the radiation field. This shift comes out infinite in all existing theories, and has therefore always been ignored. However, it is possible to identify the most strongly [linearly] divergent term in the level shift with an electromagnetic mass effect which must exist for a bound as well as for a free electron. The effect should probably be regarded as already included in the observed mass of the electron, and we must therefore subtract from the theoretical expression, the expression for a free electron of the same average kinetic energy.' This was how Bethe introduced his published nonrelativistic calculation of the Lamb shift.[9]*

* The names of Schwinger and Weisskopf were not mentioned in the preprint note that Hans Bethe sent out to several persons. Bethe included them after hearing from Weisskopf (see Weisskopf's letter to Bethe, cited below).

The main idea in Bethe's calculation was to use Kramers's renormalization procedure (although in a quantum, rather than a classical context) for the self-energy of the electron in a nonrelativistic, but quantum, consideration of this problem. Bethe recalled: 'I also heard of Kramers's renormalization procedure for the first time at that time, namely, the idea that self-energy of a free electron is simply part of its mass, and you have to subtract that self-energy from the self-energy that you get for a bound electron. So, after Shelter Island I took that famous train ride to Schenectady and tried to write down what this difference of self-energies might be, and it turned out that you could fairly easily subtract one from the other.'[10, 11]

For the self-energy $W$ of the bound electron in a quantum state $m$ in the hydrogen atom, Bethe used the standard formula of the ordinary radiation theory:

$$W = -\frac{2e^2}{3\pi\hbar c^3}\int_0^K k\,\mathrm{d}k \sum_n |v_{mn}|^2/(E_n - E_m + k). \qquad (7.1)$$

where $k = \hbar\omega$ is the energy of the light quanta of the radiation field, and $v_{mn}$ are the matrix elements of the velocity of the electron (in the nonrelativistic theory, $\mathrm{v} = \mathbf{p}/m = (\hbar/\mathrm{i}m)\nabla$). The sum in Eqn (7.1) goes over all atomic states $n$, which have energies $E_n$, and the integral is over all the photon energies from zero up to some maximum value $K$, which has to be chosen later.

For the free electron this self-energy is given by the formula

$$W_0 = -\frac{2e^2}{3\pi\hbar c^3}\int_0^K k\,\mathrm{d}k \sum_n |v_{mn}|^2/k. \qquad (7.2)$$

After integration over $k$ and making some manipulations, and using the properties of the hydrogen wavefunctions, which he knew by heart,* Bethe obtained the difference

$$W'_{ns} = \frac{8}{3\pi}\left(\frac{e^2}{\hbar c}\right)^3 \mathrm{Ry}\frac{Z^4}{n^3}\ln\frac{K}{\langle E_n - E_m\rangle_{\mathrm{av}}}, \qquad (7.3)$$

where Ry stands for the Rydberg constant, which is the ionization energy of the ground state of hydrogen, 13.6 eV, $Ze$ is the charge of the nucleus, and the average excitation energy $\langle E_n - E_m\rangle_{\mathrm{av}}$ was calculated numerically.

The nonrelativistic result in Eqn (7.3) is still divergent, but it diverges logarithmically (instead of linearly), when $K$ goes to infinity, because as a result

* However, Bethe had some help. He thanks 'Dr Stehn and Miss Steward for the numerical calculation' of the average excitation for the 2$S$ state, $\langle E_n - E_m\rangle_{\mathrm{av}} = 17.8$ Ry, 'an amazingly high value.'

of the subtraction procedure, what Bethe computed was $W - W_0$, in which the linearly divergent terms in the self-energy of the bound electron and of the free electron cancel each other. Bethe suggested that in the relativistic theory, where the self-energy of the electron is itself only logarithmically divergent, the difference $W'_{ns}$, which ought to give the Lamb shift, should be finite. 'Since we expect that relativity theory will provide a natural *cutoff* for the frequency $K$, we shall assume that in [(7.3)]

$$K = mc^2. \tag{7.4}$$

... This would set an effective upper limit of th order of $mc^2$ to the frequencies $k$ of light which effectively contribute to the shift of the level of a bound electron.'[12]

Using this value for $K$, Bethe obtained for the Lamb shift $W'_{ns} = 1040$ megacycles 'in excellent agreement with the observed value of 1000 megacycles. [Thus Bethe had shown that:] (1) the level shift due to interaction with radiation is a real effect and is of finite magnitude; (2) the effect of the infinite electromagnetic mass of a point electron can be eliminated by proper identification of terms in the Dirac radiation theory; (3) an accurate experimental and theoretical investigation of the level shift may establish relativistic effects (e.g., Dirac hole theory). These effects will be of the order of unity in comparison with the [large] logarithm in equation [(7.3)].'[13]

Bethe had completed his calculation of the Lamb shift by 9 June 1947 and sent a preliminary draft of a short paper to those participants at the Shelter Island Conference who were directly interested in the problem of the theoretical calculation of the Lamb shift. In the accompanying cover letter to Oppenheimer, Bethe wrote that the calculation 'does work out. Also, the second term already gives a finite result and is not zero as we thought during the conference. In fact, its logarithmic divergence makes the order of magnitude correct. It also seems that Vicki [Weisskopf] and Schwinger are correct that the hole theory is probably important in order to obtain convergence. Finally, I think it shows that Kramers cannot get the right result by his method.'[14]

Bethe's objection concerned Kramers' method of modifying the conventional Hamiltonian at the classical level in terms of the experimental mass of the electron. Only then, in Kramers' approach, can one use the perturbation theory without any subtraction procedure. In 1948 Kramers finally arrived at the complete fulfillment of his nonrelativistic program, in which one has no difficulties with the self-energy of the electron, but his numerical results turned out to be quite unsatisfactory because his method did not take into account the relativistic effects and the recoil effects in the interaction of the electron with radiation.

Kramers, in turn, did not much appreciate Bethe's calculation. His comment was that 'It is difficult to make (Bethe's) argument quite rigorous, but it has certain physical plausibility.'[15] Kramers did not believe that relativity would provide a natural cutoff at $mc^2$, as in Eqn (7.4), for the upper limit $K$ of the integral in Eqn (7.3), and he considered Bethe's treatment to be highly arbitrary.

Bethe's achievement in calculating the Lamb shift was highly appreciated by Weisskopf. (But see the following footnote.) He wrote to Bethe that he was 'quite enthusiastic about the result. It is a very nice way to estimate the effect and it is most encouraging that it comes out just right. I am very pleased to see that Schwinger's and my approach seems to be right after all. Your way of calculating is just an unrelativistic estimate of our effect, as far as I can see.

'I am all the more pleased about the result since I tried myself unsuccessfully to estimate the order of magnitude of our expression. I was unable to do this, but I got more and more convinced that the method was sound.

'That the $2^2S_{1/2} - 2^2P_{1/2}$ split has something to do with radiation theory and hole theory was proposed by Schwinger and myself for quite some time. We did not do too much about it until shortly before the conference. We then proposed to split an infinite mass term from other terms and get a finite term shift, just as I demonstrated at the conference. Isn't it exactly what you are doing? Your great and everlasting deed is your bright idea to treat this at first unrelativistically.'[16,*]

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* Years later, Weisskopf made the following remarks concerning Bethe's nonrelativistic calculation of the Lamb shift: 'When he [Hans Bethe] sent me this note [Bethe's draft of his calculation], I was actually really unhappy. First of all, he could have told me [that he was going to do this calculation]. I was interested in the Lamb shift problem even before the war; at that time it was called the Pasternack effect. At the Ann Arbor [University of Michigan] Summer School in 1940 I had a lot of conversations with Kramers, with whom I was very close since the old Copenhagen days. He believed, as did I, that the Pasternack effect was real and he asked me to calculate it. He first brought to me the idea that true enough the self-energy is infinite, but maybe the self-energy difference between a bound and a free electron can be calculated and will be finite, and that [later on, in 1947] should be the Lamb shift. From then on I was sort of living with this problem. During the war I became occupied with other problems [at the Manhattan Project], and the Pasternack problem was put on the back burner. But, after the war, I again wanted to take it up and I definitely knew about the problem when I came to MIT [from Los Alamos after the war]. Then came the Lamb shift, Lamb's observation that Pasternack was right and one even had quantitative results.

'Schwinger and I went together on the train to New York [to attend the Shelter Island Conference], and we discussed this problem; we arrived at the conclusion that the nonrelativistic part could be calculated with matrix elements. Then I talked a lot with Hans [Bethe] about where the difficulty lies and that the nonrelativistic part is not so difficult; the difficulty lies in the relativistic region, but I did not know how to do that.

'So when he sent me that note [Bethe's preliminary calculation], [I was unhappy] because first of all he could have told me about it, and in some ways my name should

Bethe's work stimulated Bruce French and Weisskopf at MIT, Lamb and Kroll at Columbia, and Julian Schwinger to start hole-theoretic calculations of the Lamb shift in Dirac's radiation theory. But they used non-covariant procedures for calculation, which had been described in the second edition of Heitler's book on the quantum theory of radiation.[18] The relativistically invariant approach to calculate the quantum electrodynamical effects was hardly needed. (But see Schwinger's remarks below.)

## 'I can do that for you!'

The unfolding of the story of the Shelter Island Conference, and what happened soon thereafter, is a fascinating chapter in the historical development of physics in the twentieth century, and we shall try to reconstruct it here.

At the Shelter Island Conference, Richard Feynman had already tried to make use of Kramers' suggestion about the electromagnetic origin of the Lamb shift. He tried to estimate how much his damped oscillator [his current model for interaction of a electron with the electromagnetic field] shifted in its frequency, but he didn't understand the real problem.[19]

Right after his calculations during the train ride from New York to Schenectady, and their completion there, Bethe telephoned excitedly from Schenectady to Feynman, who, at that moment was visiting Bethe's house in Ithaca. As Feynman recalled: 'He said to me that he understood the Lamb shift, that he had calculated it, and he explained the idea about mass renormalization to me. I don't remember the details, because I didn't follow it very well. And he said that he got about 1000 megacycles for the shift; he was very excited and wanted to talk about it. Although I didn't understand it too well, I realized it from his excitement that it was something very important.'[20]

Upon returning to Ithaca early in July, Bethe gave a lecture in which he explained in detail his calculation of the Lamb shift. Bethe stressed the point that the self-energy of the free electron diverges linearly. However, if you take the difference of the self-energies of the bound electron and the free electron, calculated nonrelativistically, a logarithmically divergent expression is obtained. The upper limit of the integration is to be taken to be $mc^2$ instead of infinity, so Bethe obtained a level shift of about 1000 megacycles per second. So he knew that he was on the right track. The only problem that remained was to deal with

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have been on that paper. Personally I think that he should have asked me to publish this note together with him.

'I could actually have made the calculation myself of what then was the Pasternack effect, already in the early forties. And when Lamb measured the shift accurately, I should have won the Nobel Prize.'[17]

the relativistic case precisely; exactly what do you do with the upper limit, not just cut it off arbitrarily to get the right estimate?*

'So it was a relativistic problem. In this lecture Bethe said that you have to make so many subtractions of such large terms, really infinite terms, that it's very confusing at times, exactly what to subtract from what. And he thought that if there were any way whatever to make the theory finite, even though it didn't agree with experiment, some artificial way of cutting off electrodynamics which was relativistically invariant, then we could cut off all these things which were infinite, you could subtract them exactly, and it would be very much simpler, and then you could do the relativistic end without ambiguity. Otherwise it was very confusing.'[20]

After Bethe's lecture, Feynman went up to him and told him, '"I can do that for you! I'll bring it in for you tomorrow." I knew every way to modify quantum electrodynamics known to man, at that time. I had done electrodynamics now with path integrals. [We will describe this approach in the next chapter.] I turned it upside-down, turned it in and out. It was easy for me to handle. I could change a delta-function to a sharp function instead of a delta-function and take a limit later. There was no problem. I had complete freedom to structure it. If you were to try to change a delta-function in a Hamiltonian formalism, you could be in a hell of a lot of trouble because you would have to define how to come out from the differential equations for the different functions and keep the relativistic invariance while to me, by this time, nothing was difficult. I could do electrodynamics in any way I wanted. So I told Bethe that I could do that.

'So I went home and, believe it or not, this shows you how stupid a man can be: because for the first time I applied the path integrals to electrodynamics in the *conventional representations* instead of the half-advanced and half-retarded scheme—just plain, ordinary, common usage of electrodynamics. I probably had written it a few times, but I had never tried to do anything with it. So I took the normal electrodynamics, modified it, and found a way to translate what I saw into the conventional description and that was effectively that you can subtract [from] the relation with the frequency $k$ [a corresponding term] with a higher mass and integrate it over that mass. That was the idea of the convergence scheme. [We will also describe Feynman's convergence scheme in the next chapter.] So I saw the convergence scheme, but now what was surprising was that I had never done any real problem—like calculating the self-energy, vacuum polarization, or the energy level shift, or anything.

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* Kroll and Lamb pointed out that if the effect of retardation were taken into account then the upper limit in Bethe's integration would have become $2mc^2$ instead of the arbitrary value of $mc^2$ which he had taken.[21]

'The next day I went to Bethe and told him: "Tell me how to compute the self-energy of the electron and I'll show you how to correct it, so you'll get a finite answer." I didn't know how people computed the self-energy of an electron, which was quite stupid of me; it's simply a second-order perturbation. I had gone too far on my own, but I had not looked at simple problems. So, Bethe showed me how to calculate the self-energy of the electron, and we tried to work it out. I told him the rule, and he found that the divergence went to the sixth power, instead of converging at all, which was much worse.

'So, having failed miserably, I went home and thought about it, and I couldn't figure out what was wrong, why it didn't converge. We did not know what we did wrong, but when we went over it again, following directly the rules which I was proposing, it converged! What we had done before, I don't know; in the meantime I had to learn how to do it myself. So I learned how to calculate the self-energies, energy-level differences, and the whole business, during that period.

'I learned how to do conventional quantum electrodynamics, still working from path integrals. Thus I was trying to connect my path integrals with the conventional language, and saw what the perturbation theory was from the point of view of path integrals. I noticed lots of things, including the fact that several things in perturbation theory, like the Coulomb correction and the transverse wave correction, were just one correction—the exchange of a photon. They could be represented by summing over the four directions of polarization. It was obvious from the path integrals that I would do that, and (other people) wouldn't understand me, but they would check and it would always be right. I thought that they must know that if they take the regular Dirac theory, instead of using transverse waves and summing over four directions, it takes care of the Coulomb correction, but apparently they didn't know. And I discovered great simplifications in the methods of calculation.

'As far as I was concerned, I was just taking path integrals and avoiding the perturbation theory, seeing what the terms were. They were all much simpler. The reason why they were simpler is quite clear: they were all relativistically invariant. Everything I was computing was covariant. The way others had formulated everything, they had separated the Coulomb potential and the transverse waves. That depends on the coordinate system. If you say that the divergence of A is equal to zero, it depends on the coordinate system. So they had done everything non-covariantly and, of course, the final answer for a physical problem like the scattering of two electrons, Bhabha scattering [electron-positron scattering] is simple, but it was the result of a rather complicated bunch of terms which all added together, and a whole lot of junk that was complicated was now simplified whereas when I started with my path integrals, I could see the relativistic invariance. I knew which terms went together, how they went together, and how to generalize to four dimensions from the two transverse dimensions. It was obvious; it would work; that was the fun of it. *It would always work.* I thought I was trying to learn how others did it, but I would try to do what they had done and I'd get their answers; but when I would talk to somebody, they would be so shocked that it was the right answer, and they would check and say, "Yes, it is the right answer." I began to realize that I already had a powerful instrument; that I was sort of flying over the ground in an airplane instead of having so many terms.'[20]

## A note on Richard Feynman[22]

Richard Phillips Feynman, the first child of Melville Arthur Feynman (who came as a young boy with his father as an immigrant to America from Minsk in Byelorussia in 1895) and his wife Lucille, was born on 11 May 1918 in Far Rockaway, Queens, New York City. Before Richard was born, Melville had told Lucille, 'If it's a boy, he'll be a scientist,' and he guided Richard in that direction during his childhood. Melville paid great attention to the intellectual upbringing of his son; he also had a set of the *Encyclopaedia Britannica* at home, that they often consulted together until Richard could read all the scientific articles alone, and introduced him to the Museum of Natural History in Manhattan as well as to making observations of natural phenomena. Richard grew up mostly alone, though he played with his cousins who lived in the same house; his sister Joan was born when he was nine years old. As a child, Richard learned about a wealth of natural occurrences from his father, especially how to notice unusual things and to reflect about them. Feynman's growing up in Far Rockaway, including his hobbies—he had an electrical laboratory at home and became an expert at repairing radios—his friends, family, and influences upon him, especially the influence of his father—who instilled in him the love of rational inquiries about natural phenomena and a hatred of all fuzziness in thinking—were decisive for his future development. He attended Far Rockaway High School, where the teachers were sympathetic and intelligent, especially the physics teacher, Abram Bader,* who, in a private conversation after class with the boy Feynman, told him about the beauty of the principle of least action—a principle which would become a continuously running thread in Feynman's later scientific work. Richard graduated with many honors from high school, and then went to MIT in 1935. He had also applied for entrance to Columbia University but it seems that because of the Jewish quota prevalent at that time he was not accepted, and he harbored a resentment against Columbia all his life. If Feynman had gone to

* Bader was the uncle of physicist Carl Bender, while his father Al Bender was one of Schwinger's physics teachers at Townsend Harris High School.[23]

Columbia at that time, he would have encountered Julian Schwinger there, and one can only wonder what kind of collaboration might have developed between them; the following unfolding of theoretical physics might well have been greatly enhanced by their association and joint endeavor at the same place!*

The great influence at MIT was that of Phillip Morse, who gave special lectures on quantum mechanics to Richard Feynman and his friend (another very bright youngster) Theodore Welton in their sophomore year. Morse gave them problems to solve: to determine the spectra of low-atomic-number elements, up to 10 or 12. They had to determine the energy levels by a variational principle, using hydrogen-like wavefunctions and parameters. Feynman recalled that he was 'so excited to get numbers reasonably close to the real world, which was to me very exciting, because to me the real world was always so complicated. To be able to figure out what's actually happening, that was impressive.'[24] For quantum mechanics, he studied Pauling and Wilson's *Introduction to quantum mechanics*. Feynman also studied Ruark and Urey's *Atoms, molecules and quanta* and P. A. M. Dirac's *The principles of quantum mechanics*. He learned statistical mechanics from Richard Chace Tolman's book on that subject, and relativity from A. S. Eddington's *The mathematical theory of relativity*. For nuclear physics, he studied Hans Bethe's articles in the *Reviews of Modern Physics*. During his years at MIT from 1935 to 1939, Feynman learned about the whole of theoretical physics on his own. He even published a couple of articles. He co-authored an article on 'The scattering of cosmic rays by the stars of the galaxy' with the cosmic-ray physicist Manuel Sandoval Vallarta,[25] and published his senior thesis on 'Forces in molecules';[26] both of these articles were published in the *Physical Review*. From his senior thesis, a theorem called the Hellman–Feynman theorem became well known.[27]

Feynman went to Princeton University for his PhD. At Princeton, Feynman was supposed to become a research assistant to Eugene Wigner, but was instead assigned to John Archibald Wheeler, who had just recently joined the Princeton physics faculty. Wheeler profoundly influenced Feynman, and the latter gave as much as he got. At Princeton, Feynman perfected his path-integral approach to nonrelativistic quantum mechanics and wrote a doctoral dissertation on 'The principle of least action in quantum mechanics;'[28] he took his PhD from Princeton in 1942. At Princeton he gave a seminar on his work with John Wheeler on the action-at-a-distance theory of electrodynamics that was attended by 'monster minds' such as Einstein, Pauli, Henry Norris Russell, John von Neumann, and Eugene Wigner. This period will be described in detail in the next chapter.

* However, perhaps not, given Schwinger's fear of dominance. Moveover, physics undoubtedly benefited from their independent paths to renormalized QED.

Already, during his last year at Princeton, even before he had completed his doctorate, Feynman was persuaded to join the atomic bomb project by R. R. Wilson. In the Manhattan Project at Los Alamos, Feynman became the leader of the Technical Computations Group in the Theory Division headed by Hans Bethe. Feynman developed a special rapport with Bethe and was deeply influenced by him. He came to have a great affection and respect for Bethe and, after Los Alamos, followed Bethe to Cornell, where Bethe had arranged a faculty appointment for him. Feynman met Schwinger at Los Alamos, when the latter went there in summer 1945 with Jerrold Zacharias to give some lectures on waveguides and synchrotron radiation, which we described in Chapter 4. Feynman arrived in Cornell for his teaching assignment only in the beginning of November 1945. For quite some time in the beginning he did not know in which direction to go scientifically, but later he spent a beautiful, satisfying, and productive period of several years; he was promoted to an associate professorship at Cornell in February 1947.

In a major experiment, performed by Willis E. Lamb, Jr, and his graduate student Robert C. Retherford at Columbia University in the last week of April 1947, the Lamb shift had been discovered and measured.[29] As we have already discussed, it became one of the principal themes of discussion at the Shelter Island Conference on the fundamental problems of quantum mechanics in the first week of June 1947, which Feynman was also invited to attend. On the return trip from Shelter Island, Bethe took his famous train ride from New York to Schenectady and during the three or four hours ride on the train he made the initial nonrelativistic quantum electrodynamics calculation of the Lamb shift, which we described on pp. 211–215. Bethe pointed out that if a means could be found to make electrodynamics finite with the help of a cutoff procedure, then a relativistic quantum field-theoretic calculation of the Lamb shift could be carried out quite simply. Bethe's lecture at Cornell when he returned really got Feynman started on his research again, for he knew how to introduce a relativistically invariant cutoff into the Lagrangian of classical electrodynamics by his path-integral method. Feynman immediately began to perfect his space–time approach to quantum electrodynamics,[30] in which his path-integral formulation of non-relativistic quantum mechanics played a fundamental role.[31] (See pp. 215–218 and Chapter 8.)

## Julian Schwinger and the aftermath of the Shelter Island Conference

As we have discussed, after the Shelter Island Conference 'Bethe then instantly proceeded to exploit his great familiarity with hydrogenic dipole matrix elements and sum rules to compute the nonrelativistic aspects of these ideas.

Owing to the comparative insensitivity of the calculation to the unknown high energy cutoff, a better than order-of-magnitude number emerged. The agreement of that number with the observed level shift ended any doubt, if doubt there was, concerning the electromagnetic nature of the phenomenon. Yet the relativistic problem, of extracting from the theory a finite and unique prediction, remained.' [197]

After their honeymoon and nostalgic trip around the country during the summer, Julian and Clarice Schwinger returned home only in September, and Julian 'set out on the trail of relativistic quantum electrodynamics.' Upon his return to Harvard, Norman Ramsey, who had just joined the Harvard faculty, reassured Schwinger that perhaps the electron had an intrinsic magnetic moment that was different from the value predicted by the Dirac equation. 'Julian really cross-examined me as to whether the hyperfine anomaly was true. He thought he knew how to explain the anomaly in the hyperfine interaction. Breit[7] had previously pointed out that you could explain the anomaly in the hyperfine interaction by assuming an anomalous magnetic moment of the electron.' Breit had made two mistakes in the first draft of his paper, one of which, that the magnetic moment of the proton was unchanged, was corrected by Ramsey before publication. The other error consisted in effect of treating the anomalous magnetic moment not as that due to a circulating current, but as separated north and south poles.[32]

This is how Schwinger would shortly put his analysis of the situation, in the first published report of the new quantum electrodynamics: 'Attempts to evaluate radiative corrections to electron phenomena have heretofore been beset by divergence difficulties attributable to self-energy and vacuum polarization effects. Electrodynamics unquestionably requires revision at ultra-relativistic energies, but is presumably accurate at moderate relativistic energies. It would be desirable, therefore, to isolate those aspects of the current theory that essentially involve high energies, and are subject to modification by a more satisfactory theory, from aspects that involve only moderate energies and are thus relatively trustworthy. This goal has been achieved by transforming the Hamiltonian of the current hole theory electrodynamics to exhibit explicitly the logarithmically divergent self-energy of a free electron, which arises from the absorption and emission of light quanta. The electromagnetic self-energy of a free electron can be ascribed to an electromagnetic mass, which must be added to the mechanical mass of the electron. Indeed the only meaningful statements of the theory involve this combination of masses, which is the experimental mass of a free electron.'[43]

Then Schwinger continued: 'It is important to note that the inclusion of the electromagnetic mass with the mechanical mass does not avoid all divergences: The polarization of the vacuum produces a logarithmically divergent term proportional to the interaction energy of the electron in an external field. However, it has long been recognized that such a term is equivalent to altering the value of the electron charge by a constant factor, only the final value being properly identified with the experimental charge. Thus the interaction between matter and radiation produces a renormalization of the electron charge and mass, all divergences being contained in the renormalization factors.' [43]

Before this paper was written, which we will describe in the following chapter, Julian Schwinger went to attend the Tenth Washington Conference on Theoretical Physics (13–15 November 1947), devoted to 'Gravitation and electromagnetism'; it was organized by Edward Teller and George Gamow who were at the George Washington University. Richard Feynman also attended that conference; on his way to Washington, Feynman gave a seminar on 12 November at the Institute for Advanced Study in Princeton on 'Dirac's electron from several points of view.' Dirac himself was visiting the Institute during that academic year, and also attended Feynman's seminar. In his talk, Feynman briefly presented the content of his paper on the path integral in quantum mechanics,[31] and some attempts to give a sum-over-histories formulation of quantum mechanics for the spin-$\frac{1}{2}$ particle, which obeys the Dirac equation. For this purpose, Feynman used not only paths which go forwards in time, but also paths which go backwards in time, in accordance with the idea which he had learned from John Wheeler as a graduate student in Princeton in 1940. Feynman also described his attempt to calculate the Lamb shift.[33]

At the Washington meeting, Schwinger gave a status report on the calculation of the additional magnetic moment of the electron, results which he had obtained since the Shelter Island Conference and his return from the honeymoon. His calculation on the magnetic moment was not yet complete. 'But the magnetic moment of the electron was not my sole concern at that time,' Schwinger recalled. 'My one distinct memory of the Washington meeting is of sitting at a big table and apparently taking notes during a lecture: Was it George Gamow explaining his ideas on the blackbody residual radiation of the big bang? I do not recall. What I do recall is that I was actually doing some simple computations, using my knowledge of the hydrogenic wave functions in momentum space, to understand the "amazingly high value," as Bethe put it, of his average excitation energy for hydrogen.' [197]

After the conference, Feynman reported to his friends Herbert and Mulaika Corben in Pittsburgh: 'The meeting in Washington was very poor, don't quote me. The only interesting thing was something that Schwinger said at the end of the meeting. It was interesting because it got Oppy [Oppenheimer] so excited but I did not have time to understand exactly what Schwinger had done. It had to do with electromagnetic self-energy problems. One thing he did point out that was very interesting though, was that the discrepancy in the hyperfine structure of hydrogen noted by Rabi [the anomalous magnetic moment of the electron], can be explained on the same basis as that of electromagnetic self-energy, as can the line shift of Lamb. The rest of the meeting was concerned with gravitation and the curvature of the universe and other problems for which there are very powerful mathematical equations—lots of speculation but very little evidence.

'I met Mrs Schwinger and had hoped to come back to Princeton from Washington with them on the train. I was trying to find out from Julie [Julian] then, what he was trying to explain at the meeting. Unfortunately they did me dirt and did not come to Princeton. I stopped off at Princeton on my way back to Ithaca to talk to Pias [sic: Pais] and Bohm and used up all my time with Pias—unfortunately, because I also wanted very much to talk to Bohm.'[34]

Julian Schwinger sent his first report on renormalized quantum electrodynamics, excerpts of which we have quoted above, to the *Physical Review* on 30 December 1947. It gave the predicted additional magnetic moment of $(\alpha/2\pi)\mu$ (where $\mu = e/2mc$ is the Bohr magneton and $\alpha = e^2/\hbar c$ is the fine structure constant) and Schwinger pointed out that not only are the hyperfine structure discrepancies accounted for but also the later more accurate atomic moment measurements in states of sodium and gallium.[6] Schwinger believed correctly that the simplest example of a radiative correction was that for the energy of an electron in an external magnetic field. The detailed application of the theory showed that the radiative correction to the magnetic interaction energy corresponded to an additional magnetic moment associated with the electron spin, of magnitude $\delta\mu/\mu = (1/2\pi)(e^2/\hbar c) = 0.001162$. The recently acquired experimental data confirmed this prediction. 'Measurements on the hyperfine splitting of the ground states of atomic hydrogen and deuterium[5] have yielded values that are definitely larger than those expected from the directly measured nuclear moments and an electron moment of one Bohr magneton. These discrepancies can be accounted for by a small additional electron spin magnetic moment.[7] Recalling that the nuclear moments have been calibrated in terms of the electron moment, we find that the additional moment necessary to account for the measured hydrogen and deuterium hyperfine structures to be $\delta\mu/\mu = 0.00126 \pm 0.00019$ and $\delta\mu/\mu = 0.00131 \pm 0.00025$, respectively. These values are not in disagreement with the theoretical prediction. More precise confirmation is provided by measurement of the $g$ values for ${}^2S_{1/2}$, ${}^2P_{1/2}$ and ${}^2P_{3/2}$ states of sodium and gallium.[6] To account for these results, it is necessary to ascribe the following additional spin magnetic moment to the electron, $\delta\mu/\mu = 0.00118 \pm 0.00003$.

'The radiative correction to the energy of an electron in a Coulomb field will produce a shift in the energy levels of hydrogen-like atoms, and modify the scattering of electrons in a Coulomb field. Such energy-level displacements have recently been observed in the fine structures of hydrogen,[29] deuterium, and ionized helium.[35] The values yielded by our theory differ only slightly from those conjectured by Bethe[9] on the basis of a nonrelativistic calculation, and are, thus, in good accord with experiment. Finally the finite relativistic correction to the elastic scattering of electrons by a Coulomb field provides a satisfactory termination to a subject that has been beset with much confusion' [43]. In a footnote at the end of this paper, Schwinger referred to Hendrik Kramers' remarks about mass renormalization Shelter Island Conference.

After the Washington meeting, Richard Feynman independently calculated the radiative correction to the gyromagnetic ratio for the electron (which in Dirac's theory is given by $g_s = 2$, but the experimental value obtained by Foley and Kusch, as reported above, was $g_s = 2.000244 \pm 0.00006$). Feynman considered the radiationless scattering of the electron in the external magnetic field, and calculated the transition amplitude to the first order of perturbation in the radiative corrections.

At first, this problem had been treated by Sidney Dancoff within the non-covariant perturbation theory of the day (1939), but he had missed certain matrix elements.[36] This was the 'confusion' to which Schwinger referred above [43]. Dancoff's mistake was first established by Koba and Tomonaga,[37] and rediscovered by H. W. Lewis, who found that after mass renormalization the amplitude for radiationless scattering did not contain any ultraviolet divergences,[38] although it was infrared divergent. (We referred to Dancoff's mistake in Chapter 3.)

Using his relativistic cutoff procedure, Feynman calculated the amplitude of the radiationless scattering and obtained the result that the radiative correction to the scattering in any potential is equivalent to the first-order correction in $e^2/\hbar c$ to the potential itself. In terms of the Dirac Hamiltonian, the finite radiative corrections to the radiationless scattering were found by Feynman to be

$$\Delta H_{\mathrm{Dirac}} = \frac{e^2}{2\pi\hbar c}\left(-\frac{\hbar e}{2mc}[\beta\, \boldsymbol{\sigma}\cdot \mathrm{B} - \mathrm{i}\beta\, \boldsymbol{\alpha}\cdot \mathrm{E}]\right), \tag{7.5}$$

where E is the electric field, B is the magnetic field, $\boldsymbol{\alpha}$ and $\beta$ are the Dirac $4 \times 4$ matrices, and $\boldsymbol{\sigma}$ is the Pauli matrix. Equation (7.5) showed that the interaction of the electron with radiation changes its magnetic moment by the fraction $\alpha/2\pi = e^2/2\pi\hbar c$, which was first discovered, as mentioned above, by Schwinger in a different way (here $\alpha$ denotes, as usual, the dimensionless electromagnetic coupling constant, or the fine structure constant) [43].

## The APS meeting in New York

On his way back to Boston after the Washington conference, Julian Schwinger gave a talk on his calculation of the magnetic moment of the electron at

Columbia University. Rabi was overjoyed by Schwinger's visit, but he found it 'very regretful and melancholy' that Julian should spend his days 'in self-imposed exile, in a barren land where fish is consumed as a brain food, in large quantities, with results that fall short of highest expectations.'[39] It was still Rabi's fervent hope that Schwinger would return to Columbia. Rabi wrote to Hans Bethe: 'It certainly seems very likely that the $g$-value of the electron is greater than 2 by slightly over 1/10 of 1% and that the Schwinger theory of our hyperfine structure anomaly is as correct as your theory of the Lamb–Retherford effect—God is great.'[40] Bethe immediately replied to Rabi: 'I have heard about Schwinger's theory and find it very wonderful. Nobody so far has been able to give me a complete account of his theory of the hyperfine structure or of the $g$-factor. But I am sure it is alright. It is certainly wonderful how these experiments of yours have given a completely new slant to a theory and how the theory has blossomed out in a relatively short time. It is as exciting as in the early days of quantum mechanics.'[40]

Schwinger later rhetorically asked: 'After reporting that finite radiative corrections were attained in both bound-state and scattering calculations, why was I not specific about their precise values?'[197] Soon, however, Schwinger himself would give a complete answer publicly. The 1948 New York meeting of the American Physical Society was held from 29 to 31 January 1948 at Columbia University, and Julian Schwinger was invited to give a paper on the recent developments in quantum electrodynamics. On 31 January, Schwinger gave his talk and, by popular acclaim, it had to be repeated two more times on the same day. He reported his initial results on the Lamb shift and the calculation of the anomalous magnetic moment of the electron; he mentioned some discrepancy between his calculations of the anomalous magnetic moment in the Coulomb field in the atom and the magnetic moment of the free electron, which he had worked out to be $\alpha/2\pi = e^2/2\pi\hbar c$. Feynman, who attended Schwinger's lecture at the APS Meeting, mentioned after the lecture that he had computed the same things as Schwinger had done. He confirmed Schwinger's results about the Lamb shift and the magnetic moment of the free electron,* but he stressed the point that he had obtained the same result for the magnetic moment of the electron in the atom as for that of the free electron, contrary to Schwinger's result. The reason for this discrepancy (which was, in effect, with the second term in Eqn (7.5)) was that Schwinger's calculation was not relativistically invariant.

* As we will document below, Schwinger disagreed that Feynman had definite results this early. Ramsey confirms this impression by remarking that he did not hear of any contributions of Feynman at the 1948 APS meeting. 'It is my impression that Schwinger deserved overwhelmingly the credit for QED. I don't think Feynman had an explanation of the anomalous hyperfine [structure] before that meeting.'[32]

When the calculation procedure is relativistically invariant, there is no problem in showing that Feynman was right, and the magnetic moment of the electron in the atom also equals $\alpha/2\pi = e^2/2\pi\hbar c$. Thus the complete covariant result for the magnetic moment is as given by Feynman in Eqn (7.5).

Many years later Feynman recalled Schwinger's talk at the APS Meeting and what he had done: 'So I got up after Schwinger's talk and said, "I have computed the same thing, and I agree with Professor Schwinger in all of his results, but that the magnetic moment of the electron is the same in the atom and out of the atom."

'I was not showing off, I was just trying to say that there's no problem, for I had done the same thing that he had done, and it had all come out all right. Now, Schwinger was already well known, and many people had not heard of me. Schwinger had done many things, great things, before the war, in the theory of deuteron, scattering of neutrons by helium to produce polarized neutrons, and other things. People knew Schwinger, but most of them did not know me. I heard later from several people who were at the APS Meeting that I sounded funny to them. "The great Julian Schwinger was talking when this little squirt got up and said, 'I have already done this. Daddy, you're in no trouble at all! Everything will be OK!' " Actually, I was quite surprised when he reported that he got another value for the electron's magnetic moment in the atom. I was trying to tell him that there's no difficulty at all! I had caught up with him, and I knew that everything was fine!'[41]

At the APS Meeting, Schwinger mentioned that there was the covariant method of calculation, but he had not applied it yet, and 'no doubt that these problems in covariance would be resolved with the new formulation. That explained why [J. Robert] Oppenheimer then said that "you know, [Sin-itiro] Tomonaga has done this." I said, no, I didn't know that Tomonaga had come up with the same formulation.'[1] We will discuss Tomonaga's work, and Schwinger's reaction to it, in the next chapter.

'Until the APS Meeting in New York in early 1948, I had not heard the name of Tomonaga as a physicist. Of course, I knew about [Hideki] Yukawa, because since 1935 the idea of mesons—which Yukawa had put forth—had been around. Anyway, Oppenheimer said that this covariant formulation I had written down had already been put forward by Tomonaga.[42] I said, "That's interesting; I'll have to read the paper." I think Rabi was aware of it, and I have a feeling that Rabi had sent me a copy of Tomonaga's paper; in any event, my attention was directed to it. I did ask Tomonaga as to whom he had sent his papers, and he told me that he had sent them to Oppenheimer. [Actually, Rabi had been in Japan in 1946, where he met the important physicists and brought back papers of what they were working on.] That 1943 paper of Tomonaga's would have been one of the many; everybody then was worrying about corrections to scattering, and I am sure they—the Japanese—were doing scattering. Everybody was occupied with the divergence problem. Tomonaga didn't solve the problem by renormalization. He was doing what everybody else was doing: compensating fields, something else that provided a minus sign, what (Abraham) Pais did, for example.[43] [Schwinger's] line of development was different.'[1]

At the January 1948 APS Meeting in New York, Freeman Dyson also gave a paper on the electromagnetic shift to the spinless electron, a calculation we will describe below.[44] Schwinger recalled that he 'turned to whoever was sitting next to me, saying why on earth would anybody spend time doing that, since there was no real application. So I had seen Dyson, but my initial impression was a little strange.'[1] There is a background to the story of Dyson's work on the electromagnetic level shift of a spinless electron when he came to work with Hans Bethe at Cornell in Ithaca, and we shall refer to it in its proper context, on p. 241.

## The Pocono Conference

From 30 March to 2 April 1948, the second conference on the problems of fundamental physics was held at the Pocono Manor Inn, located approximately midway between Scranton, Pennsylvania, and the Delaware Water Gap. Pocono Manor offered the same kind of undisturbed setting as had Ram's Head Inn on Shelter Island. Twenty-eight physicists participated. Kramers, McInnes, Nordsieck, Pauling, and Van Vleck, who had attended the Shelter Island Conference, were absent. The new participants were Niels and Aage Bohr, Eugene Wigner, Gregor Wentzel, Paul Dirac, and Walter Heitler.

The Pocono Conference was Julian Schwinger's first opportunity to learn what Feynman was doing with quantum electrodynamics; earlier he had only seen his work with John A. Wheeler on classical electrodynamics, and the idea of abolishing the electromagnetic field, in a fundamental sense, did not appeal to Schwinger at all.[45] But by the time of the Pocono Conference, Feynman had reworked almost all of quantum electrodynamics by his new technique of space–time diagrams. He had reached the most important part of his new results: namely, the relativistic formulation of quantum electrodynamics and, especially, of perturbation theory, the relativistic cutoff and the renormalization of mass, closed expressions for the transition amplitude and causal propagators, a new operator calculus, rules for the calculation of the contribution to the transition amplitude in each order of perturbation theory, and the idea of corresponding visualization of these rules by diagrams. He had calculated the Lamb shift and the anomalous magnetic moment of the electron, and cross-sections of different processes. However, before the Pocono Conference, Feynman had not published anything on quantum electrodynamics and he did not have the mathematical proofs of all his results. We shall discuss Feynman's work on various aspects of his space–time approach to quantum electrodynamics and its mathematical formulation in Chapter 8. These were the investigations which he had completed during the period between the Shelter Island and the Pocono Conferences, but published only during the year between the Pocono and the Oldstone Conferences.

At the Pocono Conference, Julian Schwinger gave a marathon lecture on his version of quantum electrodynamics; his scheme was rooted in the earlier work of Dirac, Fock, and Podolsky,[46] and Schwinger proceeded to present a systematic approach based on a series of canonical transformations. (This covariant approach was published as the series 'Quantum electrodynamics,' which we will describe in Chapter 8 [50, 52, 53, 57].) He gave an exact calculation of the Lamb shift and the anomalous magnetic moment of the electron on the basis of his methods, which he described in detail. As Feynman recalled at Schwinger's sixtieth birthday celebration in 1978: 'Each of us had worked out quantum electrodynamics and we were going to describe it to the tigers. He [Schwinger] described his in the morning, first, and then gave one of these lectures which are intimidating. They are so perfect that you don't want to ask any questions because it might interrupt the train of thought. But the people in the audience like Bohr, and Dirac, Teller, and so forth, were not to be intimidated, so after a bit there were some questions. A slight disorganization, a mumbling, confusion. It was difficult. We didn't understand everything, you know. But after a while ... he would say, "perhaps it will become clearer if I proceed," so he continued this, continued it ... .'[47] Schwinger's lecture lasted well into the afternoon.

## Schwinger's lecture

Notes based on Schwinger's lectures, as well as those of the other speakers at the Pocono Conference, were widely circulated, and are still prized possessions of many physicists.[48] The notes taken of Schwinger's presentation by John Wheeler, consisting of some 37 pages, have been recounted in some detail in Schweber's book,[49] so we will concentrate on the high points. Schwinger's point was that to identify the infinite terms one had to have a treatment which was both gauge- and relativistically invariant.

He began by introducing propagation functions defined in terms of commutators, both for the photon field $A_\mu$ and the matter field $\psi$. In terms of the latter, he wrote down an expression for the vacuum expectation value of the current–current commutator in the non-interacting case. After these kinematical preliminaries, he wrote down the Schrödinger equation including the interaction; in general, for two systems with interaction Hamiltonian $H_{12}$, that equation is

$$i\hbar\frac{\partial}{\partial t}\Psi(t) = (H_1 + H_2 + H_{12})\Psi(t), \tag{7.6}$$

where the operators are time independent. But now, 'following Dirac and Tomonaga, we make a contact transformation,'

$$\Psi_{\text{New}} = \mathrm{e}^{(\mathrm{i}/\hbar)(H_1+H_2)t}\Psi_{\text{Old}} \tag{7.7}$$

which gives rise to the interaction representation, in which both the operators and the wavefunction are time dependent:

$$\mathrm{i}\hbar\frac{\partial}{\partial t}\Psi_{\text{New}}(t) = H_{12}(t)\Psi_{\text{New}}(t), \tag{7.8}$$

where

$$H_{12}(t) = \mathrm{e}^{(\mathrm{i}/\hbar)H_0 t}H_{12}\mathrm{e}^{-(\mathrm{i}/\hbar)H_0 t}, \quad H_0 = H_1 + H_2. \tag{7.9}$$

This, of course, is not a covariant formulation. The interaction picture in the Schrödinger equation may be 'regarded as the result of setting times equal in an infinite set of equations of the many-time formalism.' Thus he generalized by introducing a time for each point of a space-like hypersurface, $\sigma(\mathrm{x})$, the generalization process indicated by

$$\Psi(t)\underset{\text{Dirac}}{\longrightarrow}\Psi(t_1, t_2, \ldots)\underset{\text{Dirac--Tomonaga}}{\longrightarrow}\Psi(t(\mathrm{x})). \tag{7.10}$$

Now he introduced a Hamiltonian density $\mathcal{H}$, and obtained the functional Tomonaga–Schwinger equation

$$\mathrm{i}\hbar c\frac{\delta\Psi(\sigma)}{\delta\sigma(\mathrm{x})} = \mathcal{H}(\mathrm{x})\Psi(\sigma). \tag{7.11}$$

For the case of electrodynamics, the Hamiltonian density is $\mathcal{H} = -(1/c)j_\mu A_\mu$. A supplementary condition had to be satisfied as well,

$$\Omega\Psi(\sigma) = 0, \tag{7.12}$$

where

$$\Omega = \frac{\partial A_\mu}{\partial x_\mu}(x) + \frac{1}{\mathrm{i}c}\int_\sigma D(x - x')j_\mu(x')\,\mathrm{d}\sigma_\mu, \tag{7.13}$$

where $D$ is defined by the commutator

$$[A_\mu(x), A_\nu(x')] = \frac{\hbar c}{\mathrm{i}}\delta_{\mu\nu}D(x - x'). \tag{7.14}$$

Schwinger showed that this condition was consistent, in that it held at all points:

$$\frac{\delta}{\delta\sigma(\mathrm{x})}[\Omega\Psi(\sigma)] = 0. \tag{7.15}$$

The note-taker then stated that 'these equations contain nothing more than the Heisenberg–Pauli formalism and would not be required if one knew how to carry out H–P calculations consistently. One can get back to the Dirac many-time formalism by putting a suitable number of delta functions in the current.' Schwinger concluded this part of his lecture by showing that the theory is gauge-invariant.

Schwinger then went on to treat perturbation theory up to order $e^2$. He did this by making another transformation,

$$\Psi \rightarrow \mathrm{e}^{-\mathrm{i}S(\sigma)}\Psi(\sigma), \tag{7.16}$$

where

$$S(\sigma) = \frac{1}{\hbar c}\int_{\pm\infty}^{\sigma} \mathcal{H}(x')\,\mathrm{d}\omega'. \tag{7.17}$$

[The 4-dimensional volume element is denoted by $\mathrm{d}\omega'$.] To this order, then, the equation of motion reduced to

$$\hbar c\frac{\delta\Psi}{\delta\sigma} \approx \frac{1}{2}[S(\sigma), \mathcal{H}(x)]\Psi, \tag{7.18}$$

and the supplementary condition to

$$\frac{\partial}{\partial x_\mu}A_\mu\Psi = 0. \tag{7.19}$$

By writing Eqn (7.18) that was to be solved as

$$\mathrm{i}\hbar c\frac{\delta\Psi}{\delta\sigma} = \mathcal{H}'\Psi, \tag{7.20}$$

with

$$\mathcal{H}' = \frac{\mathrm{i}}{2}[S, \mathcal{H}] = \frac{\mathrm{i}}{2\hbar c^3}\int_{\pm\infty}^{\sigma} [j_\mu(x')A_\mu(x'), j_\nu(x)A_\nu(x)]\,\mathrm{d}\omega', \tag{7.21}$$

Schwinger then broke this interaction down into two parts, one of which described the Møller interaction (when two particles are present), and the self-energy (if only one particle is present), while the second 'accounts for virtual pair production, self-energy of the photon, and real interactions between light quanta and electrons.' At this point Schwinger remarked that this treatment could be extended to processes involving arbitrarily many electrons, but Bohr objected that 'one may not be able to treat all physical problems without a fundamentally new idea.'

Schwinger then went on to treat the photon self-energy, and showed that it could be rendered finite, and therefore, necessarily zero. The electron self-energy is logarithmically divergent, but *independent of the state of motion of the* *electron.* Schwinger obtained the same result as Weisskopf had (cf. Eqn (6.23)), apart from some numerical errors in his paper:

$$\delta mc^2 = \frac{3}{2\pi}\alpha mc^2 \left[\ln \frac{2}{mc\sqrt{\epsilon}} - \frac{1}{2}\gamma - \frac{1}{6}\right], \tag{7.22}$$

where $\epsilon \to 0$ is the lower limit of an integration, and $\gamma = 0.577\ldots$ is Euler's constant.

Schwinger finally turned to electrons moving in given external fields. Again, using a method of canonical transformation, he arrived at the following relativistic formula for the Lamb shift, that is the ${}^2S_{1/2}$–${}^2P_{1/2}$ splitting in hydrogen:

$$\Delta E \propto \left[\ln \frac{mc^2}{\Delta W} - \ln 2 + \frac{3}{8} + \frac{1}{8}\right]\frac{8\alpha}{9\pi}, \tag{7.23}$$

where the logarithmic term is that obtained by Bethe, Eqn (7.3). As we will discuss in the next chapter, this result contained an error. (The $\frac{1}{8}$ is probably a transcription error by the note-taker; it should be $\frac{1}{2}$ coming from the anomalous magnetic moment coupling. Apparently the vacuum polarization contribution, which gives a term of $-\frac{1}{5}$, is not included here.)

The impact of Schwinger's lecture at Pocono spread far and wide. C. N. Yang recalled: 'I did not make it to the meeting. I was just a graduate student. From Chicago, Fermi, Teller, and Wentzel went. Fermi did not usually take notes when he went to a conference. But this time, he took voluminous notes because he was aware that it was a historical event to listen to what Schwinger had to say. After they came back to Chicago, there was the question of how to digest these notes. Fermi gathered Teller and Wentzel and four graduate students, viz., Geoffrey Chew, Murph Goldberger, Marshall Rosenbluth, and myself, into his office, and we spent weeks trying to digest what Fermi had written down as what Schwinger had said. This lasted from April to May, 1948. Murph kept notes. I still have a copy of these; it totals 49 pages. After about six weeks of meeting several times a week in Fermi's office for something like two hours each session we were all very tired, and none of us felt that we had understood what Schwinger had done. We only knew that Schwinger had done something brilliant, namely he had produced this $(\alpha/2\pi)$ and he was also already into the calculations of the Lamb shift.

'At the end of our six weeks of work, somebody asked, "Wasn't it true that Feynman also talked?" All three said, "Yes, yes, Feynman did talk." "What did he say?" None of them could say. All they remembered was Feynman's strange notation: $p$ with a slash through it.'[50]

### Feynman's lecture

All those present at the Conference—including the new participants—were deeply impressed by Schwinger's ideas and talk. Afterwards, Feynman gave his lecture, entitled 'Alternative formulation of quantum electrodynamics.' As he recalled: 'This meeting at Pocono was very exciting, because Schwinger was going to tell how he did things and I was to explain mine. I was very nervous there and didn't sleep well at all, I don't know why. But the meeting was very exciting. Schwinger and I would talk to each other, and we would compare notes as to our respective results. He would tell me where his terms came from, and I would tell him my result for the same; we did not know how each of us had done it, but we agreed on the answer. We would talk about the physical ideas, and see what the result of our respective calculations was. We could talk back and forth, without going into details, but nobody there understood either of us. But Schwinger and I could talk back and forth with each other. When he tried to explain his theory, he encountered great difficulty. Now and then he would remark: "Well, let's look at it physically." As soon as he would try to explain the ideas physically, the wolves would descend on him, he had great difficulty. Also, people were getting more and more tired.

'Taking a cue from the response that Schwinger got, Bethe said to me: "You should better explain things mathematically and not physically, because every time Schwinger tries to talk physically he gets into trouble." Now the problem for me was that all my thinking was physical. I did things by cut and try methods, which I had myself invented. I didn't have a mathematical scheme to talk about. Actually I had discovered one mathematical expression, from which all my diagrams, rules and results would come out. The only way I knew that one of my formulas worked was when I got the right result from it. So, in a sense, I did have a mathematical scheme, but it was not organized in a way that I could explain it in terms that would be familiar to other people; it could not be put into any familiar mathematical language. My way of looking at things was completely new, and I could not deduce it from other known mathematical schemes, but I knew what I had done was right.

'So, following Bethe's advice, I said in my talk: "This is my mathematical formula, and I'll show you that it produces all the results of quantum electrodynamics." Immediately I was asked: "Where does the formula come from?" I said, "It doesn't matter where it comes from; it works, it's the right formula!" "How do you know it's the right formula?" "Because it works, it gives the right results!" "How do you know it gives the right answers?" "It will become evident from what I do with it. I'll show you how the formula works, and I'll do one problem after another with its help." So I tried to explain the meaning of the symbols I had employed, and I applied it to solve the problem of the self-energy of the electron. They got bored when I tried to go into the details. Then Bethe tried to help me by asking: "Don't worry about the details, explain to us how the formula works," and so on. Question: "What made you think that the formula was right in the first place?" Then I tried to go into the physical ideas. I got deeper and deeper into difficulties, everything became chaotic. I tried to explain the tricks I had employed. For instance, take the exclusion principle, which says that you can't have two electrons in the same state; it turns out that you don't have to pay much attention to that in the intermediate states in the perturbation theory. I had discovered from empirical rules that if you don't pay attention to it, you get the right answers anyway, and if you do pay attention to it then you have to worry about this and that. Then they asked: "But what about the exclusion principle?" "It doesn't make any difference in the intermediate states!" Then Teller asked: "How do you know?" "I know because I have worked it out!" Then Teller said: "How could that be? It is fundamentally wrong that you don't have to take the exclusion principle into account." I replied: "We'll see that later."

'Already in the beginning I had said that I'll deal with single electrons, and I was going to describe this idea about a positron being an electron going backward in time, and Dirac asked, "Is it unitary?" I said, "Let me try to explain how it works, and you can tell me whether it's unitary or not!" I didn't even know then what "unitary" meant. So I proceeded further a bit, and Dirac repeated his question: "Is it unitary?" So I finally said: "Is what unitary?" Dirac said: "The matrix which carries you from the present to the future position." I said, "I haven't got any matrix which carries me from the present to the future position. I go forwards and backwards in time, so I don't know what the answer to your question is."

'Every one of these people had something in mind, and they acted as if I should know what they thought. Dirac had proved somewhere that in quantum mechanics, since you progress only forwards in time, you have to have a unitary operator. But there is no unitary way of dealing with a single electron. Dirac could not think of going forwards and backwards, and he wanted to know whether the theorem concerning unitarity applied to it. Each one of them, for different reasons, thought that there were too many gimmicks in what I was doing, and it proved to be impossible to tell them that you could actually go ahead with what I was doing.

'Bohr was also at the meeting. After I had tried many times to explain what I was doing and didn't succeed, I talked about trajectories, then I would swing back—I was being forced back all the time. I said that in quantum mechanics one could describe the amplitude of each particle in such and such a way. Bohr got up and said: "Already in 1925, 1926, we knew that the classical idea of a trajectory or a path is not legitimate in quantum mechanics; one could not talk about the trajectory of an electron in the atom, because it was something not observable." In other words he was telling me about the uncertainty principle. It became clear to me that there was no communication between what I was trying to say and what they were thinking. Bohr thought that I didn't know the uncertainty principle, and was actually not doing quantum mechanics right either. He didn't understand at all what I was saying. I got a terrible feeling of resignation. I said to myself, I'll just have to write it all down and publish it, so that they can read it and study it, because I know it's right! That's all there is to it.

'Of course, there was no personal criticism in all this, no personal antagonism. Dirac was mumbling, "Is it unitary?," Teller was excited about the exclusion principle, and Bohr was concerned about the uncertainty principle and the proper use of quantum mechanics. To tell a guy that he doesn't know quantum mechanics—well, it didn't make me angry, it just made me realize that he [Bohr] didn't know what I was talking about, and it was hopeless to try to explain it further. I gave up, I simply gave up, and decided to publish my work because I knew it was all right.

'Obviously, I had started backwards and I hadn't explained my ideas rightly in the first place; everything was tumbled around, and all the pieces were out of joint. I was trying to explain the pieces of the puzzle rather than explaining the pattern. However, with regard to Schwinger things were different. In the lunch periods, and at other times outside the meeting and discussions, he and I would compare notes on formulas for special problems, and see that both of us had the same results. We knew where everything came from and we both knew that each of us was right, that we were both respectable. I could trust him, and he could trust me. We came at things entirely differently, but we came to the same end. So there was no problem with my believing that I was right and everything was OK. That I did not explain things properly is correct, but the rumors that I was depressed were not quite true; I just felt that there had been no communication.'[22, 24] Feynman's subsequent successful publications will be described in the following chapter.

## The summer and fall of 1948

### Vacuum polarization

For Feynman, vacuum polarization remained the main unsolved problem of quantum electrodynamics in the spring of 1948. Feynman recalled the situation at the time of the Pocono Conference at the end of March 1948 as follows: 'When it was my turn to talk, I began by saying, "I can do everything but I can't do closed loops, the self-energy of the photon." Schwinger immediately got up and said, "I can do everything including vacuum polarization." And he worked something out; he got a term which looked something like vacuum polarization, and he was able to treat it... .Actually, I had everything, too, only it took me just a little longer to realize that I had it.'[24]

Schwinger maintained that 'as for vacuum polarization, he [Feynman] did not have it. He simply did not have it and [there is] nothing to be said about it.

Obviously, I had it. If one could ever discover the [actual] notes of my lecture at Pocono, one would see that when I talk about the Lamb shift I give specific contributions to the various pieces and there's is a $-\frac{1}{5}$ that's vacuum polarization.* Remember my 1939 work [with Oppenheimer] in which an excited fluorine atom emits an electron and a positron, that's vacuum polarization too [15]. And so I was not likely to leave it out. It was very important psychologically, because I had known it for many years. Now Feynman often said that in contrast with other people who write down equations and solve them, I write down solutions; this is what puts people off. How do you know the solutions? Of course, if you write down the solutions, then you're doing it piece by piece; you have no general theory to refer to, and you realize that vacuum polarization has difficulties; therefore you leave it out. This was the problem, and it went back to his [Feynman's] attitude. He thought that he rendered his theory finite by putting in a form factor between the coupling of the charges and the electromagnetic field, and if you do that then you would get a finite electron mass and so forth. So he didn't know what to do with vacuum polarization and said, well, maybe it isn't there.

'Vacuum polarization [originally was] a phenomenon [in which] out of the decaying nucleus there comes an electron–positron pair. Vacuum polarization is just a handy word for meaning that there are phenomena in which electron–positron pairs are created; it is a catchword to indicate that class of phenomena and you can't get rid of it. It does not mean more than the fact that an electron–positron combination is coupled to the electromagnetic field and it may show itself really or virtually . . . . I put the vacuum polarization in because it was there; Feynman found difficulty with it in his formulation and therefore speculated that it was not to be included. When the experiments had advanced to a greater level of accuracy, such as the Lamb shift, then there was no doubt that vacuum polarization was there, that it was a real phenomenon, and it had to be included.'[1]

Schwinger recalled that 'the subject of vacuum polarization is a point on which, throughout [the] 1948 period and beyond, Feynman and I disagreed, a point not of individual mathematical style but of fundamental physics. [As] Bethe said, "the polarization of the vacuum is consciously omitted in Feynman's theory."[51] The reasoning went this way: A modification of the electromagnetic interaction made the electromagnetic mass [of the electron] finite but did nothing for the apparently more severely divergent—here it is again—photon mass. Therefore, things would be simpler if all such effects (as closed loops, in Feynman's graphical, acausal language) were omitted. But I knew that the

* In fact, that particular term does not appear explicitly in the rather sketchy extant notes from Pocono, although vacuum polarization is treated briefly.[48]

virtual photon emitted by the excited oxygen [or fluorine] nucleus created an electron–positron pair; the vacuum is polarizable. In a later paper [64] I would use this very example to illustrate a manifestly gauge-invariant treatment of vacuum polarization.' [197]

At the Pocono Conference, Schwinger and Feynman 'got together in the hallway and although we'd come from the ends of the earth with different ideas, we had climbed the same mountain from different sides and we could check each other's equations . . . . Our methods were entirely different. I didn't understand about those creation and annihilation operators. I didn't know how these operators that he was using worked, and I had some magic from his point of view. We compared our results because we worked out problems and we looked at the answers and kind of half described how the terms came. He would say, "Well, I got a creation and then annihilation of the same photon and then the potential goes . . . . " "Oh, I think that might be that," and I'd draw a picture. He didn't understand my pictures and I didn't understand his operators, but the terms corresponded and by looking at the equations we could tell, and so I knew, in spite of being refused admission by the rest, by conversation with Schwinger, that we both had come to the same mountain and that it was a real thing and everything was all right.'[47]

The discussions between Feynman and Schwinger continued after the Pocono Conference. In fact, several weeks after the conference, they discussed these problems during Feynman's visit to MIT.[20, 22] 'We discussed matters at Pocono and later also over the telephone and compared results. We did not understand each other's method but trusted each other to make sense—even when others did not trust us. We could compare final quantities and vaguely see in our own way where the other fellow's terms or error came from. We helped each other in several ways. For example, he showed me a trick for integrals that led to my parameter trick, and I suggested to him that only one complex propagator function ever appeared rather than his two separate real functions. Many people joked we were competitors—but I don't remember feeling that way.'[52]

### The French–Weisskopf calculation

Feynman was so impressed by the coincidence of his results with the ones that Schwinger had obtained in a different way that he did not foresee the possibility of any common mistakes. As Schwinger recalled, 'Sometime in 1948, [V. F.] Weisskopf and [J. B.] French completed their non-covariant calculation of the bound-state energy shift, using every possible clue to maintain relativistic invariance, including the known effect of a magnetic field. Their result was similar to, but not quite identical with, what the covariant calculations of Feynman and myself had produced, which were the same apart from Feynman's omission of the vacuum polarization. Somewhat shaken, French and Weisskopf retreated to their blackboards and pondered. I, of course, believed the covariant calculation. But then I happened to chance on the almost forgotten outcome of my own noncovariant calculation using the right spin–orbit coupling [which he had sketched at the January 1948 APS meeting]. It was identical with the French–Weisskopf result! That shook me up to the point that, as Freeman Dyson in 1949 attested, I found the careless slip in use of the [artificially introduced] photon mass.[53] This reconciled all the calculations, vacuum polarization aside [53]. And so, as far as the relativistic energy shift is concerned, although Weisskopf was not the first to find the correct result, he was the first to insist on its correctness.' [197] And Feynman recalled the same event as follows: 'At the same time (as Schwinger and myself), Weisskopf [and French] also calculated the Lamb shift. That was a rather pedestrian, plodding, hard-working, old-fashioned, but careful way of doing it. Weisskopf [and French] made their calculation by following the logic of [the earlier work of] Bethe; it was accurate thinking but old-fashioned. They got a different answer than I did. He called me up on the telephone to tell me about the difference and how his formula compared with mine. His calculation was so complicated that I felt sure that he had made a mistake. And so, for a long time Weisskopf and French hesitated to publish their result; since my method of calculation was so much more efficient, they were [also] sure that they had made an error. They kept on checking and re-checking their calculation, which delayed [the publication]. It made me very unhappy, because they were right and had made no error.'[47,*]

Weisskopf himself recalled this incident as follows: 'J. B. French and I calculated the difference [between $2^2S_{1/2}$ and $2^2P_{1/2}$ energy levels in hydrogen] carefully and got a well-defined result in agreement with the experiment. We believe that we were the first to arrive at the result. Then followed a tragicomical episode. We showed our method and result to Julian Schwinger and to Richard P. Feynman. They independently tried to repeat our calculations but found a result differing by a small additive numerical constant. [In fact, Feynman and Schwinger had already completed their incorrect covariant calculations.] Having both Feynman and Schwinger against us shook our confidence, and we tried to find a mistake in our calculation, without success. Only seven months later Feynman informed us that it was he and Schwinger who had made a mistake! We published our paper,[54] but in the meantime, a similar calculation was made by [Norman M.] Kroll and [Willis E.] Lamb (1948), which appeared a few months earlier than ours.[21] Self-confidence is an important ingredient that makes for a successful physicist.'[55] Feynman acknowledged his error

* In the meantime, the correct answer was independently obtained by Kroll and Lamb.[21] 'For reasons beyond [his] editorial control, it appears as the last paper' in Schwinger's collection [83].

in a footnote, 'appropriately numbered 13,' in his paper on the 'Space–Time approach to quantum electrodynamics.'[30] This whole episode will be discussed in more detail in the next chapter.

### A note on Freeman J. Dyson

It was in the early summer of 1948 that Richard Feynman travelled by car to Albuquerque, New Mexico. On a completely unplanned trip, Freeman Dyson drove from Cleveland to Albuquerque with Feynman for three or four days, 'and that was the time when I really got to talk with Feynman—twenty-four hours at a stretch. We talked about everything: his theory and his whole approach to life and physics.'[56] Dyson had been aware of Feynman's approach to quantum electrodynamics since September 1947, when he arrived at Cornell from Cambridge, England, to work with Hans Bethe as a graduate student.

Freeman J. Dyson was born on 15 December 1923 at Crowthorne, Berkshire, England, the son of George Dyson, a music teacher at Wellington College (a boys' school near the village Crowthorne), and his wife Mildred Lucy Dyson. George Dyson, an accomplished musicologist, accepted the position of Master of Music at Winchester College in 1924, and in 1928 he became the director of the Royal College of Music. Freeman had an older sister Alice. Both George and Mildred Dyson cared very deeply about intellectual matters, and this love of intellectual life was transferred to their son. When Freeman was almost nine years old he was sent to Twyford, a boarding school located a few miles from home, which he found 'strange and forbidding.'[56, 49] At the age of 12, in summer 1936, Freeman sat for the scholarship examination for Winchester College, one of the best English public schools; Freeman stood first in all the papers and in the order of merit, and won the scholarship. At Winchester College, which has been in continuous operation since 1394, Freeman took interest not only in mathematics but also in biology and the other sciences. Together with M. J. Lighthill (later the successor to P. A. M. Dirac as Lucasian Professor of Mathematics at Cambridge, a position which he gave up to become President of University College London), Dyson worked through the three volumes of Camille Jordan's *Cours d'analyse*, which they found on the upper shelves of the library. Jordan's *Cours* had probably been donated by the Cambridge mathematician G. H. Hardy, who had been a student at Winchester College. Hardy had a passion for mathematics, which was also true of Dyson; Freeman taught himself 'all the calculus and most of complex function theory' by studying the 28-page entry on 'Functions,' written by the geometer Henry F. Baker of Cambridge in the eleventh edition of the *Encyclopaedia Britannica*. In his 1979 book, *Disturbing the universe*,[57] Dyson gave revealing accounts of some chapters of his life, and recounted that he spent his 1938 Christmas vacation working from 6 in the morning till 10 in the evening with short breaks for meals, going through

Piaggio's *Differential equations* and solving more than 700 problems,[56, 49] and has always manifested the 'same intensity, stamina, and power of concentration in all his undertakings.'[49]

Dyson left Winchester College in summer 1941, and in the fall of that year he went up to Cambridge, where he had been admitted to Trinity College with a scholarship. At Winchester, Dyson had already studied Eddington's *The mathematical theory of relativity* and had worked through Georg Joos's *Theoretical physics*, and physics was his first choice for field of study. All the Cambridge physicists, other than Dirac, had left to work on war-related projects, but Hardy and [Abram S.]Besicovitch, two of Cambridge's great mathematicians, were there. Dyson and Lighthill attended many lectures of Dirac, Hardy, and Besicovitch; in fact, he and Lighthill were Hardy's only two students from 1941 to 1943.[56] Dirac and Dyson became good friends, and they often went together on long walks. At Cambridge, Dyson enjoyed night climbing the college buildings, and discovered girls.[49]

At the age of 15, with a deep awareness of the world around him, Freeman Dyson had become a staunch pacifist and was firmly convinced that Gandhi had shown a moral way of life. He knew that a world catastrophe was coming but doubted that the methods of nonviolence and civil disobedience would work with fervent German Nazi soldiers. The Second World War broke out in September 1939, but it did not change Freeman's pacifist views; he had long discussions with his father about the feasibility of pacifism, but George Dyson had welcomed Britain's entry into the war and saw it as the only means of stopping the spread of Hitler's Nazism. Freeman himself struggled with his pacifist beliefs, but the great 'courage and good humour'[49] displayed by the British in their daily struggle for survival convinced him that he too must help his country win the war. In 1943 he joined the Royal Air Force Bomber Command as a civilian scientist, and was sent to the Bomber Command Headquarters at High Wycombe; this was a base from which each night, weather permitting, squadrons of Lancaster bombers would be ordered to bomb cities in Germany. Dyson was 19 years old, with two years of Cambridge behind him; C. P. Snow, who was responsible for placing technical personnel into appropriate jobs, had selected Dyson after an interview, and he was assigned to the Operational Research Section of the Bomber Command. Dyson's assignment was to analyze the factors that would increase the efficiency of the airplanes in their sorties and minimize their losses, and he had to make appropriate recommendations. He rapidly got 'into the swing of things,'[49] and characterized most of the work he did at the Bomber Command as 'problems of common sense.'[49] His findings often went against the accepted wisdom, and 'all the good advice we gave had no result.'[49] He felt very inadequate, and believed that 'he was not making any dent in minimizing bomber losses or in bombardiers' lives,' and he 'also felt keenly that he should have been fighting like the other young men of his age.'[49]

Freeman Dyson worked hard and spent long hours at the Bomber Command base, but he still found time to continue some of his studies and researches in mathematics and physics. He had obtained John von Neumann's book on the mathematical foundations of quantum mechanics[58]—a book which he found 'frustrating' because 'it did not make quantum mechanics any clearer than Dirac had, and it did not give any clue on how to connect with the real world.'[49] He also acquired the second edition of Heitler's *Quantum theory of radiation*,[18] and found it 'enormously refreshing, because you could calculate with it.'[49, 56] In August 1945, before Japan's surrender, Dyson's unit with the Bomber Command was all set to fly to Okinawa, to carry out bombing raids on Japan, but the dropping of atomic bombs on Hiroshima and Nagasaki changed these plans.

At war's end, Dyson accepted the position of a demonstrator in mathematics at the Imperial College of Science and Technology, where his duties consisting of correcting and grading papers and answering questions of students.[49, 56] However, he spent most of his time reading in the old science library. Once a week he would go to meet Harold Davenport at Birkbeck College; Davenport was an outstanding number theorist, who suggested to Dyson that he tackle the problem of Minkowski's 'second conjecture'—the work on the problem went slowly, but ultimately Dyson solved it.[56]* During the year he was at the Imperial College, Dyson wrote his fellowship thesis for Trinity College on problems in mathematics, and worked primarily as a mathematician. However, upon his return to Trinity as a fellow, he began to concentrate his efforts on physics and obtained a desk at the Cavendish Laboratory, interacting with many young theoreticians there.[49, 56]

Nicholas Kemmer, a former student of Gregor Wentzel's at Zurich, who had done important work on meson theory before the war,[60] was a lecturer at Cambridge at the time. Dyson, together with other budding theorists, attended Kemmer's lectures on 'Nuclear physics' and on 'Particles and fields,' and Kemmer became Dyson's 'first real physics teacher.'[49, 56] Wentzel had sent his 1943 book, *Einführung in der Quantentheorie der Wellenfelder*, to Kemmer, and both Kemmer and Dyson carefully studied it. Kemmer confirmed the story that after one of his lectures he, Dyson, and Harish-Chandra were walking along King's Parade in Cambridge and going to lunch. Harish-Chandra, who up to that time had been a doctoral student in physics of Dirac's in Cambridge—he completed a thesis in 1947 on 'The irreducible representations of the Lorentz group'—made the following remark: 'Theoretical physics is in such a mess, I

* 'The novelty of this paper[59] lies in the fact that it is the first introduction of algebraic topology to the geometry of numbers.'[49]

have decided to switch to pure mathematics' (which he did when he came to the USA and did distinguished research on 'semi-simple Lie groups'), whereupon Dyson remarked, 'That's curious. I have decided to switch to theoretical physics for precisely the same reason!' (which he did when he came to the USA and did important work on quantum electrodynamics and other fields). 'End of conversation!'[61] Dyson had gathered from his readings* that theoretical physics was flourishing in the United States, so he went to see Sir Geoffrey Ingram Taylor—the famous hydrodynamicist and expert in turbulence—at the Cavendish for advice as to where to go. Taylor, who had spent the war years at Los Alamos, where he had known Hans Bethe and members of his group and knew what Bethe was doing, told Dyson, 'You must go to Cornell to work with Bethe.' He also said, 'You might go to talk to Peierls at Birmingham. He might be in a better position to advise you.'[49, 56]

After conferring candidly with Rudolf Peierls and Nicholas Kemmer, Freeman Dyson immediately applied for a Commonwealth Fund Fellowship to study with Hans Bethe at Cornell University. With Geoffrey Taylor's recommendation, directly addressed to Bethe,[49] Dyson was accepted as his regular graduate student and a doctoral committee consisting of Bethe (chairman) and Robert Wilson was assigned to monitor his progress. Dyson enrolled in Bethe's course on 'Advanced quantum mechanics' and attended Wilson's lectures on 'Experimental nuclear physics,' as well as a course on the theory of solids, which was taught by an instructor named Smith. Dyson was aware of Bethe's calculation of the level shift in hydrogen in a simplified model, in which relativity and the spin of the electron had been ignored; Bethe had turned the resulting infinite answer into a plausible finite one which agreed with experiment, and he handed over the problem of the exact calculation to Richard Scalettar, one of his graduate students, just a couple of days before Dyson arrived at Cornell. Hence Bethe assigned him as an interim problem to work out the calculation of the Lamb shift for a spin-0 electron by using the correct relativistic wave equations; all Dyson had to do was to take Bethe's nonrelativistic calculation and repeat it by using relativistic electrodynamics and doing the mass renormalization a little bit more carefully. It was Dyson's first research problem in physics; of course, there was no experiment to compare it with since there were no spin-0 electrons (however, the spin corrections are small): his paper on 'The electromagnetic shift of energy levels' appeared in the 15 March 1948 issue of the *Physical Review*,[44] in which he made full use of his knowledge of the quantum theory of fields he had gained from Wentzel's book which he had studied earlier with Kemmer in Cambridge.

* He also read the Symthe report,[62] which had much to do with his conversion to theoretical physics.

After the Shelter Island Conference, Richard Feynman began to perfect his space–time approach to quantum electrodynamics, in which the path-integral formulation of nonrelativistic quantum mechanics played a fundamental role.[31] During the academic year 1947–1948, Feynman often discussed his methods with anyone who would listen. One attentive listener was Freeman Dyson, who interacted with Feynman 'mostly just by listening. At that time he was working extremely hard to develop his version of quantum electrodynamics; it was still not finished. He had the relativistic cutoff and he knew how to deal with positrons, pair creation and closed loops by means of his diagrams. But he hadn't yet got it all together into a workable scheme. It was still something that only he knew about how to do, and he had problems communicating with other people. He had these ideas that were so different from the conventional ones. I listened a great deal to him and I was convinced that he had something valuable, but that it needed to be understood. That was one of the things I set out to do. During that year I spent a fair amount of time just listening to Feynman talk about all kinds of things. I was in Cornell just nine months, from September to June; during that time I picked up everything from Feynman.'[56] But Dyson did not complete his PhD!

After Feynman and Dyson's joint automobile trip West from Cleveland to Albuquerque, Dyson came back on the bus to Ann Arbor, Michigan, and from 19 July to 7 August 1948, he spent a period of three weeks at the University of Michigan Summer School, where Julian Schwinger gave 'his very polished lectures describing his way of doing the Lamb shift and his version of recent developments in quantum electrodynamics.'[56]

As Schwinger himself recalled, 'It seems that I supplied the notes for the first part of the course, which must have been the manuscript for the paper received by the *Physical Review* on July 29 [50]. The notes for the second part were taken by David Park. I have read... words to the effect that what I presented there was like a cut and polished diamond, with all the rough edges removed, brilliant and dazzling. Or, if you don't care for that simile, you can have "a marvel of polished elegance, like a violin sonata played by a virtuoso—more technique than music." I gather I stand accused of presenting a finished elaborate formalism from which had been excised all the physical insights that provide signposts to its construction.... The paper to which I have referred has a long historical and physical introduction that motivates the development and sets out the goals of relativistic renormalization theory [50]. Beyond that, the lectures presented the explicit working out of the interaction of a nonrelativistic electron with the radiation field, in the dipole approximation. The canonical transformation that isolates the electromagnetic mass is an elementary one, and the further details leading to the solution of the bound state and scattering problems were provided. This was the simple model on which the relativistic theory was erected. It was good enough for the immediate purposes but ... still quite primitive. I needed no one to tell me that it was but a first step to an aesthetically satisfactory and effective relativistic theory of coupled fields.' [197]

Dyson recalled that Schwinger's lectures were in the mornings, and 'I sat in the afternoons working through them, calculating myself and reproducing what he had done, and at Ann Arbor I had very close contact with Schwinger. So I understood it, so to say, from the inside. The methods of Schwinger and Feynman led to the same results, but it was not at all clear why, because they looked so different. [By the beginning of 1948, the early papers of Sin-itiro Tomonaga, published in the first two issues of the Japanese journal *Progress of Theoretical Physics*, had become available in the United States.[42, 63]] Also Tomonaga was doing essentially the same thing that Schwinger was doing, only Tomonaga explained things in a much less elaborate fashion so that it was easier to understand, but he did not go so much into detail. But Tomonaga's way and Schwinger's way were essentially the same. They were based on the standard field theory formalism translated into covariant language and that basic formalism was the same, but Feynman's was totally different. He didn't even write down a Hamiltonian or anything; he just wrote down the answer, just gave you a set of rules for writing down the answer.'[56]

### The Michigan Summer School 1948: Julian Schwinger's lectures

Schwinger's lectures at the Summer Symposium at University of Michigan in July and August 1948 are recorded in notes preserved in the UCLA archive.[64] The notes for Chapter 1 were written in Schwinger's hand, as they constituted the completed manuscript of 'QED I' [50]; David Park took the notes for Chapter 2. The content was largely that given in the 'Quantum electrodynamics' papers which we will describe in the following chapter: the covariant formulation of quantum electrodynamics, which he had already described in the Pocono lectures, and like those lectures, he ends by computing the Lamb shift [cf. Eqn (7.23)]:

$$\Delta E \propto \ln - \ln 2 + \frac{3}{8} - \frac{1}{5} + \frac{1}{2}, \tag{7.24}$$

where ln stands for the Bethe logarithmic term, $-\frac{1}{5}$ for the vacuum polarization, and the $\frac{1}{2}$ for the magnetic moment effect, which is now correctly incorporated. As we know, the correct French–Weisskopf calculation replaced the $\frac{3}{8}$ by $\frac{5}{6}$; a few months later, Schwinger would discover his error.

Dyson recalled his experience with Schwinger in Ann Arbor vividly. He first met Schwinger at 'the summer school in Michigan in the summer of 1948. We were both there for five or six weeks, I guess. This was sort of the main event of the summer. He lectured three times a week, or something of that kind and it was a leisurely affair. People stayed for several weeks, only two or three lectures a day, maybe fewer, so we had lots of time. Uhlenbeck and, I think, Chandrasekhar also lectured.

'[Schwinger's] lectures were almost incomprehensible. I always like the quote of Robert Oppenheimer—Schwinger was after all his protégé—that "other people give talks to tell you how to do it, but Julian gives talks to tell you how only he can do it." He gave this extraordinarily elaborate formalism and never really explained very much why he did it that way. It was a rather bewildering morass. But when I got him quietly alone, then it was very different. So after the talk I would work very hard working through it all and making sure that I had all the equations right and so on. Then afterwards I would go and talk to him and he was very friendly and then he talked in a totally different way, telling me what it was all about, why he did it that way. So it was strange that his public persona was so different from his private one. When I had him to myself it was actually delightful. Of course, he also talked a lot about other things, such as what he'd been doing in classical electromagnetism, and so on. I don't remember any details but I have a feeling he had just done the synchrotron radiation calculation at that time [described in Chapter 5]. I found that very interesting.

'We were there for five weeks. At the end of the time, I felt I understood what Schwinger had done (that had not yet been published). I was very lucky to have that completely explained, clear and understandable, more or less in detail, so I could go on and do my own work.

'That was the good luck. I'd been with Feynman all through the winter and had gone with Feynman to Albuquerque just before Michigan. I spent the rest of the summer in Berkeley, and I think it was in September that things sort of came together.'[65]

Upon the completion of Julian Schwinger's lectures at Ann Arbor, Freeman Dyson went to Berkeley, California, on a vacation, as part of sightseeing required by the terms of his Commonwealth Fund Fellowship which had taken him to Cornell. He recalled that on the bus ride back from Berkeley to Chicago, where he was going to stay with friends for a week, 'it became clear in my head what the situation was with Feynman, what Feynman's theory really was. Since I was more in contact with Feynman than anybody else, I realized quite soon that it was a very great opportunity to translate Feynman into the language that other people could use. That was essentially my job . . . . Then in Chicago I really worked out the essential outline of the paper which I put together.'[56, 66]

Dyson was more concerned with Schwinger being offended by his prior publication than with Feynman. 'I remember I wrote some sort of polite apology to Julian for stealing his thunder, because I was publishing his stuff before he had got around to publishing it. He was very friendly. He never took any umbrage. I remember feeling more anxious with Schwinger than about

Feynman. I remember saying I was going to reverse the tactics of Marc Antony, saying "I come to praise Schwinger not to bury him." That was when I was giving a talk at the New York meeting of the Physical Society in January 1949. I think I was having a conversation with Oppenheimer at that point. I was careful to be extremely polite and as admiring as I could.'[65]

Robert Finkelstein, who attended the Michigan Summer School that year, aptly summarized Schwinger's remarkable achievement that summer. 'It was in the 1948 Michigan lectures that Julian first described his breakthrough in quantum electrodynamics to a wider audience. Among the young people present were Dyson, Kroll, Lee, and Yang. (Yang told me that he had never heard anyone speak English so rapidly.) One may give a feeling for the impact of these lectures by quoting Dyson who wrote home that "in a few months we shall have forgotten what pre-Schwinger physics was like." The work Julian was then describing grew out of the experimental discoveries of Lamb, Rabi, and Kusch and led to the mid-century revolution in theoretical physics. Bethe at that time described this period as the most exciting in physics since the great days of 1925–30 when quantum mechanics was being discovered.

'Although very many others, and of course particularly Tomonaga and Feynman, contributed to this development, it was Julian who made the major breakthrough by first understanding the full consequence of the new experiments, by constructing the first manifestly covariant theory, and by first calculating in lowest order all the previously inaccessible consequences in QED. Other simpler formalisms soon followed: Feynman's Michigan lectures came the following summer, and Dyson's lectures came the third year, but a special place in our Pantheon belongs to Julian who first climbed the mountain and dominated the earliest developments. To show that this mountain could be climbed at all was a very great achievement because up to that time quantum electrodynamics appeared to be fatally flawed.'[67]

## The Charles L. Mayer Nature of Light Award, 1948[68]

On 9 May 1949, at a special meeting of the Physics and Applied Science Colloquium at Jefferson Physical Laboratory at Harvard University, Julian Schwinger was presented with the Charles L. Mayer Nature of Light Award, 1948. Isidor I. Rabi had prepared the citation which read: 'Dr. Schwinger has extended the scope and power of quantum electrodynamics of Heisenberg and Pauli by reformulating it in a relativistic covariant form.

'In this way he was able to eliminate in a systematic manner the infinities which made the statements of this theory ambiguous in application. The application of this theory to the Lamb shift of the 2$S$ level in hydrogen gives a theoretical basis to the explanation of this effect by H. A. Bethe. The further application of the theory to the intrinsic magnetic moment of the electron, which was discovered by Kusch and Foley, gave results in agreement with experiment.

'The methods Schwinger has introduced have found further application in radiation theory, collision theory, and in the theory of mesons and nucleons.' (I. I. Rabi. quoted in S. S. Schweber [49], p. 342)

In 1943 Charles L. Mayer had presented to the National Academy of Sciences funds for 'a prize of $4000 to be awarded in the period 1943–1945 for an outstanding contribution to our basic understanding of the nature of light and other electromagnetic phenomena which provides a unified understanding of the two aspects of these phenomena which are at present separately described by wave and corpuscular theories.'[68] E. U. Condon, I. I. Rabi, R. A. Millikan, and K. K. Darrow were invited in May 1946 by William J. Robbins, the chairman of the National Science Fund, to help in administering the award. The advisory committee concluded that two prizes of $2000 each should be awarded rather than one prize of $4000. The criteria for the awards were: 1. One prize of $2000 for a paper that provides 'in terms *intelligible to the community of scientists at large* a unified understanding of the two aspects of these phenomena which are at present jointly described by wave and by corpuscular theories.' 2. One prize of $2000 for a 'comprehensive contribution to a logical, consistent theory of the interaction of charged particles with an electromagnetic field including the interaction of particles moving with high relative speeds.'[68]

On 23 April 1946, four papers, out of a total of 28 entries, were deemed worthy of awards: those of 1. C. J. Eliezer on 'The interaction of particles and an electromagnetic field'; 2. Peter Havas, 'On the interaction of radiation and matter'; 3. Giulio Racah, 'On the self-energy of the electron'; and 4. J. A. Wheeler and R. P. Feynman, 'Classical electrodynamics in terms of action at a distance.' All these papers related to the No. 2 award, and subsequently the No. 2 prize was awarded to C. J. Eliezer.

It was also decided by the committee to propose to the donor that the 'prize be opened for another two or three year period and be awarded for some contribution to original knowledge in this general field,' to which Charles Mayer agreed, and suggested that the remaining $2000 could be awarded to the scientist who... 'makes the best contribution either for the first or second subject,' which was acceptable to the committee.

K. K. Darrow resigned from the committee in 1947, and in August 1947 Richard Feynman was invited to become a member of the advisory committee; furthermore, an announcement for the extension of the 'Charles L. Mayer Nature of Light Award' was issued, 'indicating that a prize of $2000 would be awarded in 1948 for a contribution satisfying the previously announced criteria' 1 or 2,[68] and the date of submission of the entries was extended to 1 October 1948.

Sometime in mid-1948, Julian Schwinger became aware that the National Academy of Sciences was offering a prize for 'an outstanding contribution to our knowledge of the nature of light,' and the entries had to have been published or submitted before 1 October 1948. 'When I noticed that Feynman was on the committee to award the prize, and therefore ineligible to receive it, I decided that someone out there had me in mind. The reason I mention this "ain't the money, it's the principle of the thing." '[69] Schwinger submitted the manuscripts of two completed papers [50, 52] and the incomplete provisional version of a third paper [57]. Schwinger's third paper 'began with the relativistic treatment of radiative corrections to Coulomb scattering, a topic that was experimentally remote at the time, but is now a routine aspect of interpreting high-energy experiments that employ electron and positrons. Then the manuscript took up the topic "Radiative Corrections to Energy Levels," beginning as follows: "In solutions that do not permit the treatment of the external field as a small perturbation, it is convenient to employ a representation in which the matter field spinors obey equations that correspond to a particle moving under the influence of the external potential." This is what, several years later, would be called the Furry representation.[70] The manuscript went on to study solutions of those field equations and, in the process, exhibited integral equations that were the space–time, relativistic versions of what Lippman and I would present, more symbolically, a year or so later [60]. The manuscript ended abruptly in the middle of a sentence; deadline time had arrived' [197]. This manuscript was not published as a paper (the published 'QED III' [57] was somewhat different); however, for his entry, Schwinger received the Charles L. Mayer Light Award of $2000 and the accolade presented by Rabi, cited in the beginning of this section. 'Vicki Weisskopf said that [the money] would more than pay for our way to Europe, [but] he was wrong!'[1] as we will recount in Chapter 9.

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# Schwinger, Tomonaga, Feynman, and Dyson: the triumph of renormalization

## Schwinger's method of canonical transformations

Barely six months after the Shelter Island Conference, which reawakened his interest in quantum electrodynamics, and just three months after returning to Harvard from his extended honeymoon to the West Coast, Schwinger published a one-page note in the *Physical Review* entitled 'On quantum electrodynamics and the magnetic moment of the electron' [43]. A preliminary account of this work was presented by Schwinger at the 10th Washington Conference on Theoretical Physics in November 1947,* which, as we saw in the previous chapter, attracted the interest of Oppenheimer and Feynman.[2] Schwinger recalled this meeting as the first time he actually significantly interacted with Feynman,[1] while Feynman was impressed by Schwinger's presentation on the anomalous magnetic moment of the electron, and on the Lamb shift. In the published

* At that meeting, which was held at George Washington University, Schwinger recalled doing clandestine calculations, in lieu of note-taking, using hydrogenic wavefunctions to understand the large value of the Bethe logarithm in the Lamb shift, obtaining an estimate within about 10% of the exact value [199]. Here is another version of the story we reported in the previous chapter. 'It was a rather small group as I remember, sitting around a table. I must, at the same time, have been thinking not only about the magnetic moment, which I had carried through to the point where I could see there was an answer, but there were a few integrals to do, and so forth, but I was also thinking about what Bethe had done— you know there's a logarithm involving summation over the energy levels of hydrogen and Bethe said numerically the answer is this, and I wasn't very satisfied. While I was listening to lectures at George Washington University of no great interest to me, listening to people speculate about the universe and other such things, I was sitting there and doing a little scribbling and calculation of my own, to get a qualitative feeling for how that number came out. I was astonished that Bethe didn't actually do the numbers, because he was perfectly capable of doing it.'[1]

paper Schwinger stated the result of a calculation of 'an additional magnetic moment associated with the electron spin, of magnitude'*

$$\frac{\delta\mu}{\mu} = \frac{e^2}{2\pi\hbar c} = \frac{\alpha}{2\pi} = 0.001162, \tag{8.1}$$

$\mu$ being the Dirac magnetic moment, and $\alpha = e^2/\hbar c \approx 1/137$ the fine structure constant, a result completely consistent with the recent results on hyperfine splitting,[4] most precisely in agreement with Kusch and Foley's results for sodium and gallium,[5] which could be interpreted as an additional magnetic moment for the electron of $\delta\mu/\mu = 0.00118 \pm 0.00003$.

In the last of the four paragraphs of this one-page paper, he mentioned the result of a relativistic calculation of the Lamb shift. 'The values yielded by our theory differ only slightly from those conjectured by Bethe[6] on the basis of a nonrelativistic calculation,† and are, thus, in good accord with experiment.' As we know, all was not so well. 'Finally, the finite radiative correction to the elastic scattering of electrons by the Coulomb field provides a satisfactory termination to a subject that has been beset with much confusion.' This is a reference to the incorrect Dancoff calculation,[7] to which we alluded in Chapters 3 and 7. 'In 1939 Oppenheimer, I presume, suggested to Dancoff that he do a relativistic calculation of the electrodynamic corrections to scattering of an electron by a nucleus. He did that calculation and made a mistake, as a result of which it was not immediately obvious that all the electrodynamic corrections could be explained by uniting an electromagnetic mass with a mechanical mass. History might have been very different if that mistake had not been made. I think the Lamb shift could have been predicted.'[1]

Schwinger concluded by promising a paper detailing the theory and the applications. Alas, that was not to come to pass. By the time of the Pocono Conference

* The formula was unfortunately mistyped by the journal to read $\left(\frac{1}{2}\pi\right) e^2/\hbar c$, which was copied in Rosenfeld's book.[3]

† Schwinger was slightly upset by Bethe's publication. We have recounted in the previous chapter how Weisskopf was annoyed with Bethe for single-handedly taking credit for this result. 'It struck me roughly the same way, but not quite as forcibly as it struck Weisskopf, who I think has been quoted as being rather angry at Bethe's so rapidly stealing the thunder. Because the essence of it was what Weisskopf and I talked about and I think we had a somewhat different version of it, that if one calculated the two energy levels, the $S$ and $P$ levels, and looked at their difference, all the ultraviolet divergences would cancel and one would end up with a finite result. I was not personally upset about it, because to me the challenge was the relativistic calculation, which Bethe did not touch. Clearly a large part of the Lamb shift was nonrelativistic so my interest shifted to what was clearly a totally relativistic effect, the magnetic moment.'[1]

four months later, he had already constructed a covariant formulation, making the technique underlying this first paper obsolete.

It is important to recognize that, of course, Schwinger was well aware of the problems of electrodynamics from his earliest student days.* Moreover, he wrote a paper when he was 16, which he never submitted to a journal, entitled 'On the interaction of several electrons' [0], which discussed the Møller interaction,[9] based on the Dirac–Fock–Podolsky electrodynamics of 1932;[10] Schwinger's first effort was noteworthy for the introduction of the interaction representation [199]. Later, when he went to Berkeley, he discovered that Oppenheimer was obsessed with the subject. Indeed, Oppenheimer and Schwinger wrote a joint paper on 'Pair emission in the proton bombardment of fluorine' [15], where the explanation of the observed effect turned out to be the existence of vacuum polarization, the virtual creation, for short periods of time, of electron–positron pairs. (Although, to Schwinger's annoyance, Oppenheimer insisted on inserting remarks about a possible (non-existent) non-electromagnetic coupling between electrons and nuclear particles.) Thus he began with an advantage over Feynman, who failed to recognize the reality of vacuum polarization for the first few years of the development of quantum electrodynamics.

Crucial for Schwinger's stunning progress in quantum electrodynamics after the war was his development of electromagnetic theory at the MIT Radiation Laboratory, and in particular his perfection of the theory of synchrotron radiation immediately after the end of the war. 'What was significant was the radiation emitted by relativistic electrons moving in circular paths under magnetic field guidance. It is an old problem, but the quantitative implication of relativistic energies had not been appreciated. In attacking this classical relativistic situation, I used the invariant proper-time formulation of action, including the electromagnetic self-action of a charge. That self-action contained a resistive part and a reactive part, to use the engineering language I had learned. The reactive part was the electromagnetic mass effect, here automatically providing an invariant supplement to the mechanical action and thereby introducing the physical mass of the charge. Incidentally, in the paper on synchrotron radiation that was published several years later, a more elementary expression of this method is used, and the reactive effect is dismissed as "an inertial effect with which we are not concerned" [56, 37]. But here was my reminder that electromagnetic self-action, physically necessary in one context, was not to be, and need not be, omitted in another context. And in arriving at a relativistically invariant result, in a subject where relativistic invariance was notoriously

* Recall that his student notebooks at City College contained detailed notes of major papers on field theory by Dirac, Heisenberg, and Pauli and Weisskopf from the 1920s and 1930s.[8]

difficult to maintain, I had learned a simple but useful lesson: to emerge with relativistically invariant physical conclusions, use a covariantly formulated theory, and maintain covariance throughout the calculation.' [199]

Hendrik Kramers is usually mentioned as the father of the concept of renormalization. Yet his approach was overly classical.* 'Of course, the concept of electromagnetic self-action, of electromagnetic mass, had not entirely died out in that age of subtraction physics; it had gone underground, to surface occasionally. Hans Kramers must be mentioned in this connection. In a book published in 1938 he suggested that the correspondence-principle foundation of quantum electrodynamics was unsatisfactory because it was not related to a classical theory that already included the electromagnetic mass and referred to the physical electron.[11] He proposed to produce such a classical theory by eliminating the proper field of the electron, the field associated with uniform motion. Very good—if we lived in a nonrelativistic world. But it was already known from the work of Victor Weisskopf and Wendell Furry that the electromagnetic mass problem is entirely transformed in the relativistic theory of electrons and positrons, then described in the unsymmetrical hole formulation—the relativistic electromagnetic mass problem is beyond the reach of the correspondence principle.[12]† Nevertheless, I must give Kramers very high marks for his recognition that the theory should have a structure-independent character. The relativistic counterpart of that was to be my guiding principle, and over the years it has become generalized to this commandment: Thou shalt not entangle that which is known, and reliable, with that which is unknown, and speculative. The effective range treatment of nuclear forces, which evolved just after the war, also abides by this philosophy [40, 58]' [199]. In a book review, Schwinger summarized his position on Kramers: 'It is a common mistake to think that Kramers had anticipated post-war mass renormalization. His idea was to begin with the classical nonrelativistic Hamiltonian expansion in terms

* 'Kramers wrote a book on quantum mechanics[11] in which he goes through some pedestrian development and, I believe, points out the infinite self-energy and then says that clearly we have quantized the wrong classical theory. The correct classical theory should already have removed from it this deficiency of classical electromagnetism, namely the infinite mass of a point charge. And when you corrected the classical theory, then that is the proper thing to quantize.' But this approach to mass renormalization does not work, 'Because you cannot find a classical theory on which you can superimpose phenomena like pair creation and other things which are necessary and part of the relativistic quantum electrodynamics. It is a dead end. Nevertheless, it looks superficially as though Kramers invented mass renormalization.'[1]

† Schwinger noted that 'it was W. Furry who first appreciated the logarithmic nature of the divergence of the electromagnetic mass in the hole theory of electrons and positrons.' [199]

of the physical mass, and quantize it. But quantum relativistic effects change the nature of this self-mass.'[8]

We will not discuss Schwinger's first approach to quantum electrodynamics in any detail, in part because it was so quickly superseded by covariant methods, and thus of only historical importance, and secondly because it is discussed in mathematical detail in Schweber's book.[2] (Notes on this development may be found in the UCLA archive.[8]) It consists of the application of canonical or contact transformations to isolate the effects of mass and charge renormalization.

As a result of this remarkable advance, Schwinger was invited to give a lecture at the 1947 Annual Meeting of the American Physical Society, which took place at Columbia University at the end of January 1948. Schwinger's lecture took place on Saturday 31 January. That meeting, and Feynman's interaction with Schwinger there, was described in Chapter 7. No printed or manuscript version of the lecture apparently exists, but it was by all accounts a brilliant success.* In the minutes of the meeting, Karl K. Darrow, secretary of the APS, noted the unprecedented event that Schwinger's lecture 'was repeated by popular request.'[13] In his diary, Darrow stated that he 'heard no paper but Schwinger's, given too rapidly for my apprehension but given with great gusto which implied a great advance.'[14] Freeman Dyson was in attendance, and in writing about it to his parents noted the birth of a new formulation: 'The great event came on Saturday morning, and was an hour's talk by Schwinger, in which he gave a masterly survey of the new theory which he has the greatest share in constructing and at the end made a dramatic announcement of a still newer and more powerful theory, which is still in embryo. This talk was so brilliant that he was asked to repeat it in the afternoon session, various unfortunate lesser lights being displaced in his favour. There were tremendous cheers when he announced that the crucial experiment had supported his theory; the magnetic splitting of two of the spectral lines of gallium . . . were found to be in the ratio 2.00114 [sic] to 1; the old theory gave for this ratio exactly 2 to 1, while the Schwinger theory gave 2.0016 [sic] to 1.'[15]

Indeed, by the time of the January meeting, the new covariant theory was far advanced. Again, in Schwinger's words, 'The third stage, the development of the first covariant theory, had already begun at the time of the New York meeting in

* The only entry in the list of abstracts for the meeting under Schwinger's name was that for a paper presented with Weisskopf 'On the electromagnetic shift of energy levels' [45], which was a description of Schwinger's first, flawed, relativistic treatment of the Lamb shift. This was not based on a collaboration between Weisskopf and Schwinger, but merely on discussions the two shared over lunches in a good French restaurant. After some time, the atmosphere was spoiled because crowds of students started tagging along, as we described in Chapter 5. Schwinger simply stopped coming.[1]

January. I have mentioned that the simple idea of the interaction representation had presented itself 14 years earlier, and the space–time treatment of both electromagnetic and electron–positron fields was inevitable. I have a distinct memory of sitting on the porch of my new residence [in his wife's mother's house] during what must have been a very late Indian summer in the fall of 1947 and with great ease and great delight arriving at invariant results in the electromagnetic-mass calculation for a free electron. I suspect this was done with an equal-time interaction.* The spacelike generalization, to a plane, and then to a curved surface, took time, but all that was in place at the New York meeting. I must have made a brief reference to these covariant methods; the typed copy [of Ref. [43]] contains such an equation on another back page, and I know that Oppenheimer told me about Sin-itiro Tomonaga after my lecture.† [199]

On a human note, we also recall from Chapter 2 the story of Rabi's teasing of the unfortunate Professor LaMer in the elevator in the Faculty Club at Columbia, after the third repeated lecture; LaMer was the only man who had dared to flunk the prodigy Schwinger for not following the rules.

## Schwinger's covariant approach

The covariant approach to quantum electrodynamics, which Schwinger presented in "Quantum electrodynamics. I" [50], "II" [52], and "III" [57] was essentially identical to that first described at the Pocono Conference, at the Washington Meeting of the American Physical Society [47], also held in April, 1948, and then given in detail at the Michigan Summer School that year. These presentations have been recounted in the previous chapter. These papers were also the basis for his successful application for the Charles L. Mayer Nature of Light Award in October of that year, which we described in the previous chapter. The first of these papers was submitted just over six months after his announcement of the solution of the problems of quantum electrodynamics in [43], in July of 1948, with the second and third reaching the hands of the editors of *Physical Review* in November, and the following May, respectively.

Why was it necessary for Schwinger to abandon the non-covariant approach which so successfully yielded the $\alpha/2\pi$ correction to the magnetic moment of

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* This may have occurred earlier, in September, when he first calculated an invariant electromagnetic mass shift in his original non-covariant approach.[1]

† Schwinger recalled that Rabi was somehow involved in bringing the work of the Japanese to his attention. 'Rabi was in Japan and obviously talked to the important Japanese physicists and must have brought back papers of what they were working on.'[1]

the electron? It was the difficulty of correctly carrying out a relativistic calculation of the Lamb shift, that is, the electrodynamic displacement of hydrogen energy levels from the values predicted by the Dirac equation. Although Schwinger advertized in his note [43] success on this front, it was not satisfactory. Let us quote Schwinger himself, from his introductory remarks in his collection of the most important papers in the field, *Quantum electrodynamics* [83]: first, he recounted the progress since Kramers,[11] spurred by experiment. 'Exploiting the wartime development of electronic and microwave techniques, delicate measurements disclosed that the electron possessed an intrinsic magnetic moment slightly greater than that predicted by the relativistic quantum theory of a single particle,[5] while another prediction of the latter theory concerning the degeneracy of states in the excited levels of hydrogen was contradicted by observing a separation of the states.[16] (Historically, the experimental stimulus came entirely from the latter measurement; the evidence on magnetic anomalies received its proper interpretation only in consequence of the theoretical prediction of an additional spin magnetic moment [by Schwinger].) If these new electron properties were to be understood as electrodynamic effects, the theory had to be recast in a usable form. The parameters of mass and charge associated with the electron in the formalism of electrodynamics are not the quantities measured under ordinary conditions. A free electron is accompanied by an electromagnetic field which effectively alters the inertia of the system, and an electromagnetic field is accompanied by a current of electron–positron pairs which effectively alters the strength of the field and of all charges. Hence a process of renormalization must be carried out, in which the initial parameters are eliminated in favor of those with immediate physical significance. The simplest approximate method of accomplishing this is to compute the electrodynamic corrections to some property and then subtract the effect of the mass and charge redefinitions. While this is a possible nonrelativistic procedure,[6] it is not a satisfactory basis for relativistic calculations where the difference of two individually divergent terms is generally ambiguous. It was necessary to subject the conventional Hamiltonian electrodynamics to a transformation designed to introduce the proper description of single electron and photon states, so that the interaction among these particles would be characterized from the beginning by experimental parameters. As a result of this calculation [43], performed to the first significant order of approximation in the electromagnetic coupling, the electron acquired new electrodynamic properties, which were completely finite. These included an energy displacement in an external magnetic field corresponding to an additional spin magnetic moment, and a displacement of energy levels in a Coulomb field. Both predictions were in good accord with experiment, and later refinements in experiment and theory have only emphasized that agreement.'

But the calculation of the energy shift in the field of the nucleus, the Coulomb field, revealed the deficiency in the technique. Schwinger went on, 'However, the Coulomb calculation disclosed a serious flaw; the additional spin interaction that appeared in an electrostatic field was not that expected from the relativistic transformation properties of the supplementary spin magnetic moment, and had to be artificially corrected.[17,18,*] Thus, a complete revision in the computational techniques of the relativistic theory could not be avoided. The electrodynamic formalism is invariant under Lorentz transformations and gauge transformations, and the concept of renormalization is in accord with these requirements. Yet, in virtue of the divergences inherent in the theory, the use of a particular coordinate system or gauge in the course of computation could result in a loss of covariance. A version of the theory was needed that manifested covariance at every stage of the calculation. The basis of such a formulation was found in the distinction between the elementary properties of the individual uncoupled fields and the effects produced by the interaction between them[19] [50]. The application of these methods to the problems of vacuum polarization, electron mass, and the electromagnetic properties of single electrons now gave finite, covariant results which justified and extended the earlier calculations [57]. Thus, to the first approximation at least, the use of a covariant renormalization technique had produced a theory that was devoid of divergences and in agreement with experience, all high energy difficulties being isolated in the renormalization constants. Yet, in one aspect of these calculations, the preservation of gauge invariance, the utmost caution was required,[20] and the need was felt for less delicate methods of evaluation. Extreme care would not

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* Schwinger later remarked about his repeated APS lecture at Columbia in January 1948: 'The only record I have of that event is a typed copy of my already submitted report [43], on the back page of which is written a formula for the energy shift of hydrogenic levels. One of the terms is a spin–orbit coupling, which should be the relativistic electric counterpart of the $\alpha/2\pi$ additional magnetic-moment effect. But it is smaller by a factor of 3; relativistic invariance is violated in the non-covariant theory. ... But the back of the page also contains something else—the answer to the obvious question: What happens if the additional magnetic-moment coupling to the electric field is given its right value, no other change being introduced? What emerges, and therefore was known in January 1948, is precisely what other workers using non-covariant methods would later find, which is also the result eventually produced by the covariant methods. Of course, until those covariant methods were developed and applied, there could be no real conviction that the right answer had been found.' [199] As we noted in the previous chapter, Feynman, at that meeting, announced that he had no such difficulty in obtaining the correct magnetic moment effect using his own, totally different, but covariant, technique. But Feynman apparently did not have a value for the Lamb shift at that time—and his subsequent covariant result, like Schwinger's, was erroneous.

be necessary if, by some device, the various divergent integrals could be rendered convergent while maintaining their general covariant features. This can be accomplished by substituting, for the mass of the particle, a suitably weighted spectrum of masses, where all auxiliary masses eventually tend to infinity.[21] Such a procedure has no meaning in terms of physically realizable particles. It is best understood, and replaced, by a description of the electron with the aid of an invariant proper-time parameter. Divergences appear only when one integrates over this parameter, and gauge invariant, Lorentz invariant results are automatically guaranteed merely by reserving this integration to the end of the calculation [64].' This last remark was a reference to the well-known Pauli–Villars regularization technique, and Schwinger's reaction to it, the magnificent 'Gauge invariance and vacuum polarization' [64] paper, which we shall describe in detail in the following chapter.

However, at first Schwinger's covariant calculation of the Lamb shift contained another error, the same as Feynman's.[22] 'By this time I had forgotten the number I had gotten by just artificially changing the wrong spin–orbit coupling. Because I was now thoroughly involved with the covariant calculation and it was the covariant calculation that betrayed me, because something went wrong there as well. That was a human error of stupidity.' French and Weisskopf[23] had gotten the right answer, 'because they put in the correct value of the magnetic moment and used it all the way through. I, at an earlier stage, had done that, in effect, and also got the same answer.' But now he and Feynman 'fell into the same trap. We were connecting a relativistic calculation of high energy effects with a nonrelativistic calculation of low energy effects, *à la* Bethe.' Based on the result Schwinger had presented at the APS meeting in January 1948, Schwinger claimed priority for the Lamb shift calculation: 'I had the answer in December of 1947. If you look at those [other] papers you will find that on the critical issue of the spin–orbit coupling, they appeal to the magnetic moment. The deficiency in the calculation I did [in 1947] was [that it was] a non-covariant calculation. French and Weisskopf were certainly doing a non-covariant calculation. Willis Lamb[24] was doing a non-covariant calculation. They could not possibly have avoided these same problems.'[1] The error Feynman and Schwinger made had to do with the infrared problem that occurred in the relativistic calculation, which was handled by giving the photon a fictitious mass. 'Nobody thought that if you give the photon a finite mass it will also affect the low energy problem. There are no longer the two transverse degrees of freedom of a massless photon, there's also a longitudinal degree of freedom. I suddenly realized this absolutely stupid error, that a photon of finite mass is a spin-1 particle, not a helicity-1 particle.'[1]

An indication of the impact of Schwinger's breakthroughs, as seen by his peers at the time, is given by J. Robert Oppenheimer's remarks at the 1948 Solvay Conference in Brussels, to which Schwinger was invited, but did not attend, due to some mixup in the invitation. After reviewing the failures of the old quantum field theory, Oppenheimer stated, 'Such a procedure would no doubt be satisfactory, if cumbersome, were all quantities involved finite and unambiguous. In fact, since mass and charge corrections are in general represented by logarithmically divergent integrals, the above outlined procedure serves to obtain finite, but not necessarily unique or correct, reactive corrections for the behavior of an electron in an external field; and a special tact is necessary, such as that implicit in Luttinger's derivation[25] of the electron's anomalous gyromagnetic ratio, if results are to be, not merely plausible, but unambiguous and sound. Since, in more complex problems, and in calculations carried to higher orders in *e*, this straightforward procedure becomes more and more ambiguous, and the results are more dependent on the choice of Lorentz frame and of gauge, more powerful methods are required. Their development has occurred in two steps, the first largely, the second wholly, due to Schwinger [50].'[18]

'Quantum electrodynamics. I. A covariant formulation' [50] received by *Physical Review* on 29 July 1948, is a comprehensive development of the theory first presented in an all-day* talk at the Pocono Conference on 31 March of that year, which we described in the in preceding chapter, and which, with the first draft of 'Quantum electrodynamics II' [52] was used as the basis for his lectures at the Michigan Summer School during the period 19 July through 7 August 1948. Among those present in the audience for the Michigan lectures was again Freeman Dyson.

The paper begins with an extended abstract that summarizes the matter brilliantly: 'Attempts to avoid the divergence difficulties of quantum electrodynamics by multilation of the theory have been uniformly unsuccessful. The lack of convergence does indicate that a revision of electrodynamic concepts at ultrarelativistic energies is indeed necessary, but no appreciable alteration of the theory for moderate relativistic energies can be tolerated. The elementary phenomena in which divergences occur, in consequence of virtual transitions involving particles with unlimited energy, are the polarization of the vacuum and the self-energy of the electron, effects which essentially express the interaction of the electromagnetic and matter fields with their own vacuum fluctuations. The basic result of these fluctuation interactions is to alter the constants characterizing the properties of the individual fields, and their mutual coupling, albeit by infinite factors. The question is naturally posed whether all divergences can be isolated in such unobservable renormalization factors; more specifically, we inquire whether quantum electrodynamics can account unambiguously for the recently observed deviations from the Dirac electron theory, without the introduction of fundamentally new concepts. This paper, the first in a series devoted

* Schwinger recalled that it was a long talk, but maybe only of three hours duration.[1]

to the above question, is occupied with the formulation of a completely covariant electrodynamics. Manifest covariance with respect to Lorentz and gauge transformations is essential in a divergent theory since the use of a particular reference system or gauge in the course of calculation can result in a loss of covariance in view of the ambiguities that may be the concomitant of infinities. It is remarked, in the first section, that the customary canonical commutation relations, which fail to exhibit the desired covariance since they refer to field variables at equal times and different points of space, can be put in covariant form by replacing the four-dimensional surface $t =$ const. by a space-like surface. The latter is such that light signals cannot be propagated between any two points on the surface. In this manner, a formulation of quantum electrodynamics is constructed in the Heisenberg representation, which is obviously covariant in all its aspects. It is not entirely suitable, however, as a practical means of treating electrodynamic questions, since commutators of field quantities at points separated by a time-like interval can be constructed only by solving the equations of motion. This situation is to be contrasted with that of the Schrödinger representation, in which all operators refer to the same time, thus providing a distinct separation between kinematical and dynamical aspects. A formulation that retains the evident covariance of the Heisenberg representation, and yet offers something akin to the advantage of the Schrödinger representation, can be based on the distinction between the properties on non-interacting fields, and the effects of coupling between fields. In the second section, we construct a canonical transformation that changes the field equations in the Heisenberg representation into those of non-interacting fields, and therefore describes the coupling between fields in terms of a varying state vector. It is then a simple matter to evaluate commutators of field quantities at arbitrary space-time points. One thus obtains an obviously covariant and practical form of quantum electrodynamics, expressed in a mixed Heisenberg–Schrödinger representation, which is called the interaction representation. The third section is devoted to a discussion of the covariant elimination of the longitudinal field, in which the customary distinction between longitudinal and transverse fields is replaced by a suitable covariant definition. The fourth section is concerned with the description of collision processes in terms of an invariant collision operator, which is the unitary operator that determines the overall change in state of a system as a result of interaction. It is shown that the collision operator is simply related to the Hermitian reaction operator, for which a variational principle is constructed.'

The interaction representation indeed seems to have been Schwinger's invention, although he notes in a footnote that 'The interaction representation can be regarded as a field generalization of the many-time formalism, from which point of view it has already been considered by S. Tomonaga.'[19] In that representation, the evolution of the state vector $\Psi$ on a particular spacelike surface $\sigma$ is given by a covariant Schrödinger equation,

$$i\hbar c \frac{\delta \Psi[\sigma]}{\delta \sigma(x)} = \mathcal{H}(x)\Psi[\sigma], \tag{8.2}$$

where $\mathcal{H}$ is the interaction Hamiltonian,

$$\mathcal{H}(x) = -\frac{1}{c} j_\mu(x) A_\mu(x), \tag{8.3}$$

$j_\mu$ being the electric current density of the electrons, and $A_\mu$ the electromagnetic vector potential. (In this paper, and implicitly in all his early papers, Schwinger used an imaginary fourth-component of the four-vector position: $x_\mu = (\mathbf{r}, ict)$.) This evolution equation would later be referred to by Oppenheimer as the Tomonaga equation. Indeed, as we will see, exactly this equation, in very similar notation, appears in Tomonaga's 1946 paper,[19] to which Schwinger refers.

Schwinger's first paper is largely devoted to setting up the machinery. Most interesting, perhaps, is the final section, which begins with the words: 'While the interactions between fields and their vacuum fluctuations are conveniently regarded as modifying the properties of the non-interacting fields, other types of interactions are often best viewed as producing transitions among the states of the individual fields. We shall conclude this paper with a brief discussion of a covariant manner of describing such transitions.' Thus, the state vector on an arbitrary spacelike surface $\sigma$ is related to that on an initial surface $\sigma_1$ by a unitary operator:

$$\Psi[\sigma] = U[\sigma, \sigma_1]\Psi[\sigma_1], \tag{8.4}$$

where $U$ satisfies the equation of motion,

$$i\hbar c \frac{\delta}{\delta \sigma(x)} U[\sigma, \sigma_1] = \mathcal{H}(x) U[\sigma, \sigma_1], \tag{8.5}$$

subject to the initial condition

$$U[\sigma_1, \sigma_1] = 1. \tag{8.6}$$

This differential equation is equivalent to a functional integral equation,

$$U[\sigma, \sigma_1] = 1 - \frac{i}{\hbar c} \int_{\sigma_1}^{\sigma} \mathcal{H}(x') U[\sigma', \sigma_1] d\omega', \tag{8.7}$$

where the last term is a space–time integral over the volume between the two surfaces $\sigma_1$ and $\sigma$. If we let those surfaces recede to $\mp\infty$, respectively, we obtain the collision operator $S$, which 'determines the overall change in state of the system as the result of interaction,'

$$S = U[\infty, -\infty]. \tag{8.8}$$

This unitary operator may be written in terms of a Hermitian reaction operator $K$,

$$S = \frac{1 - \mathrm{i}K}{1 + \mathrm{i}K}. \tag{8.9}$$

Schwinger concludes this paper by showing that $K$ satisfies a variational principle, of a type which he had used in scattering problems [40,49], which we described in Chapter 5—see Eqn. (5.31).

'Quantum electrodynamics. II. Vacuum polarization and self-energy' [52] reached the editors of *Physical Review* on 1 November 1948. Now Schwinger got down to work: 'The covariant formulation of quantum electrodynamics, developed in a previous paper, is here applied to two elementary problems—the polarization of the vacuum and the self-energies of the electron and photon.' He first defined 'the vacuum of the isolated electromagnetic field to be that state for which the eigenvalue of the energy, or better, an arbitrary time-like component of the energy–momentum four-vector, is an absolute minimum.' In that state, the energy–momentum tensor has vanishing expectation value, 'the only result compatible with the requirement that the properties of the vacuum be independent of the coordinate system,' because the energy–momentum tensor the electromagnetic field is traceless. As for the matter—that is, the electron—fields, the vacuum must be such that the vacuum expectation value of the electromagnetic current density vanish; while the vacuum expectation value of the electron energy–momentum tensor is not necessarily zero, but can be so redefined.

Armed with these properties, Schwinger went on to compute the polarization of the vacuum. That is, as a consequence of fluctuations in the electron–positron fields, the vacuum expectation value of the electromagnetic current is no longer zero in the presence of an external current $J_\mu$. The result is particularly simple if the latter is time independent, and then has the form

$$\langle j_\mu(x)\rangle = -\frac{\alpha}{6\pi^2}\int K(\mathrm{r}-\mathrm{r}')\nabla'^2 J_\mu(\mathrm{r}')\mathrm{d}\tau', \tag{8.10}$$

where $\mathrm{d}\tau'$ is an element of volume, and (unlike Schwinger, we set $\hbar = c = 1$)

$$K(\mathrm{r}) = \frac{3}{2}\frac{1}{|\mathrm{r}|}\int_0^1 \frac{1-\frac{1}{3}v^2}{1-v^2}\mathrm{e}^{2m|\mathrm{r}|(1-v^2)^{-1/2}}v^2\mathrm{d}v. \tag{8.11}$$

Note that Schwinger in 1948 is using what would later universally be referred to as a Feynman parameter $v$, which Feynman would introduce only in 1949 to combine his propagators in momentum space.[26,*] This result can be expressed as a correction to the Coulomb potential, for short distances,

$$\mathcal{D}(\mathrm{r}) = \frac{1}{|\mathrm{r}|}\left[1 + \frac{2\alpha}{3\pi}\left(\log\frac{1}{m|\mathrm{r}|} - \gamma - \frac{5}{6}\right)\right], \quad 2m|\mathrm{r}| \ll 1. \tag{8.12}$$

(Here, we have not followed Schwinger's original notation, but have used the usual notation that $\gamma = 0.57721\ldots$ is Euler's constant.) This result had first been found by Uehling in 1935.[27] (See Eqn (6.25).)

Schwinger next went on to calculate the self-energy of the electron. The outcome was a 'logarithmically divergent result for the electromagnetic mass of the electron or positron.' Either by using the lower limit of a parameter integral, $w_0 \to 0$, or a large momentum scale, $K \to \infty$, to define the divergent integral, he found for the ratio of the electromagnetic mass $\delta m$ to the bare mass $m_0$

$$\frac{\delta m}{m_0} = \frac{3\alpha}{4\pi}\left[\log\frac{1}{w_0} + \text{constant}\right] = \frac{3\alpha}{4\pi}\left[\log\frac{K^2}{m_0^2} + \text{constant}\right], \tag{8.13}$$

where the constants are different with the two different 'cutoffs.' He then showed that $m = m_0 + \delta m$ may be consistently used as the actual electron mass. (As we will see, this result, which was first derived in the hole theory by Weisskopf[12] with Furry's help[2] (See Eqn (6.23)), was actually given a covariant derivation nearly six months earlier by Feynman.[22])

The old guard in Europe was not altogether satisfied with Schwinger's breakthroughs. Gregor Wentzel objected to Schwinger's claim at the Pocono Conference that the photon self-energy vanished; in the meantime Schwinger had developed an improved treatment of this question, which he had presented at the Michigan Summer School, and which appears in QED II, but Wentzel still had mathematical objections.[28]

Not surprisingly, more confrontational was the reaction of Wolfgang Pauli. Schwinger sent a copy of QED II to him, and Pauli wrote back a detailed letter in January 1949. Pauli also objected to certain details of the vacuum polarization calculation, and strongly advocated his own regularization technique,[21] which, as we have seen, Schwinger loathed. An extract of this letter appears in Schweber's book.[2] Schwinger did not reply, but rather passed the letter on to his student Bryce DeWitt, who responded without consulting Schwinger further. It was a reasonable argument involving the requirement of gauge invariance. Pauli then wrote a caustic letter to Oppenheimer: 'My discussion with Schwinger, in

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* In fact, in the Appendix to Feynman's paper in which he introduces a parameter integral to combine denominators, he states that it was 'suggested by some work of Schwinger's involving Gaussian integrals.'[26] As Schwinger stated, 'The technique of invariant parameters was the technique that Feynman borrowed from me.'[1]

which he never participated himself, makes me think on "His Majesty's" psychology. (An evening seminar on this subject—ladies admitted—would be very funny. I can also tell experimental material from earlier times.) His Majesty permitted one of his pupils (B. Seligmann) [B. DeWitt] to break the "blockade" of the ETH/Zurich by Harvard and write to me a letter, but he refused to read the letter himself! [In fact, DeWitt never showed Schwinger the letter.] The content of this diplomatic note (it was a very long one) is only this, that His Majesty had a kind of revelation on some Mt. Sinai, to put always, $\partial\Delta^{(1)}/\partial x_\nu = 0$ for $x = 0$ (in contrast to $\partial\delta(x)/\partial x_\nu$ which has same symmetry properties) wherever it occurs. We are calling here this equation "the revelation" but it did not help our understanding. The B. Seligmann and also a Mr. Glauber want to come here next spring, but both are unable to obtain a scientific recommendation from His Majesty who prefers to "sacrifice" both of them rather than write to me. I am enjoying this situation very much.'[29] In fact, Schwinger did write strong letters of recommendation for both DeWitt and Glauber, and the following summer Schwinger visited Pauli in Zurich in an attempt to smooth ruffled feathers. We will describe that visit in the next chapter.

Six months after writing 'QED II', Schwinger submitted the third of this monumental series, 'Quantum electrodynamics. III. The electromagnetic properties of the electron—radiative corrections to scattering' [57]. It is important to recognize that Schwinger was also involved in several other completely independent projects at the same time. He submitted a paper on diffraction [54] with Harold Levine in January 1949 (and a correction [55] to an earlier paper with Levine in March), and submitted the important 'Classical radiation of accelerated electrons' [56] to *Physical Review* in March as well. (We have discussed these papers in Chapters 4 and 5.) But clearly QED was now the focus. It may be helpful to quote the opening paragraphs of 'QED III': 'A covariant form of quantum electrodynamics has been developed, and applied to two elementary vacuum fluctuation phenomena in the previous articles of this series. These applications were the polarization of the vacuum, expressing the modifications in the properties of the electromagnetic field arising from its interaction with the matter field vacuum fluctuations, and the electromagnetic mass of the electron, embodying the corrections to the mechanical properties of the matter field, in its single particle aspect, that are produced by the vacuum fluctuation of the electromagnetic field. In these problems, the divergences that mar the theory are found to be concealed in unobservable charge and mass renormalization factors.

'The previous discussion of the polarization of the vacuum was concerned with a given current distribution, one that is not affected by the dynamical reactions of the electron–positron field. We shall now consider the more complicated situation in which the original current is that ascribed to an electron or positron—a dynamical system, and an entity indistinguishable from the particles associated with the matter field vacuum fluctuations. The changed electromagnetic properties of the particle will be exhibited in an external field, and may be compared with the experimental indications of deviations from the Dirac theory that were briefly discussed in [QED] I. To avoid a work of excessive length, this discussion will be given in two papers. In this paper we shall construct the current operator as modified, to the second order, by the coupling with the vacuum electromagnetic field. This will be applied to compute the radiative correction to the scattering of an electron by a Coulomb field [53]. The second paper will deal with the effects of radiative corrections on energy levels.' However, that second paper was never written, largely because soon Schwinger would begin work on a third reformulation of quantum electrodynamics, which we shall describe in the following chapter.

Let us concentrate on the results given in this monumental paper. After removing a spurious infrared divergence, Schwinger obtained first the additional spin magnetic moment he had first given a year and a half earlier,

$$\delta\mu = \frac{\alpha}{2\pi}\mu_0, \tag{8.14}$$

which is the same as Eqn (8.1). Then he turned to radiative corrections to electron scattering. He obtained a result for the differential scattering cross-section by an electron scattered by a fixed charge $Ze$ (a nucleus) through an angle $\vartheta$, in which the energy loss (due to unseen low-energy photons radiated) is less than an amount $\Delta E$,

$$\frac{\mathrm{d}\sigma(\vartheta, \Delta E)}{\mathrm{d}\Omega} = \left(\frac{Z\alpha}{2|\mathrm{p}|\beta}\csc^2\frac{\vartheta}{2}\right)^2 \left(1 - \beta^2\sin^2\frac{\vartheta}{2}\right)(1 - \delta(\vartheta, \Delta E)), \tag{8.15}$$

where p is the electron momentum and $\beta c$ its speed. A general expression for the radiative correction $\delta$ is given; for a slowly moving particle it takes on the simple form

$$\delta(\vartheta, \Delta E) \approx \frac{8\alpha}{3\pi}\beta^2\sin^2\frac{\vartheta}{2}\left[\log\frac{mc^2}{2\Delta E} + \frac{19}{30}\right], \quad \beta \ll 1, \Delta E \ll E - mc^2, \tag{8.16}$$

where $E$ is the electron energy. Schwinger concluded that these radiative corrections could amount to several per cent at the energies then available.

The above result (8.16) had, in fact, been derived nearly six months earlier. In January 1949, Schwinger had written a Letter to the *Physical Review* in which he discussed 'Radiative corrections to electron scattering' [53] There, he also discussed the Lamb shift, mentioned his earlier error due to the improper magnetic moment contribution, and stated that the result, equal to 1051 MHz for the splitting of the $2^2S_{1/2}$ and $2^2P_{1/2}$ levels of hydrogen, was now in agreement with the calculations of French and Weisskopf[23] and of Kroll and Lamb.[24] As we remarked above, when Schwinger first did the covariant calculation, he made an additional error in matching the high- and low-energy contributions. Feynman made the same mistake,[22] resulting in a significant delay in publication of the French and Weisskopf paper. Schweber criticized Schwinger for being less than forthright in acknowledging his error, unlike Feynman;[2] but in Schwinger's defense it should be noted that he never published his wrong result, giving the incorrect formula only at the Michigan Summer School.[8] Schwinger gave a detailed account of his and Feynman's 'goof' in his historical talk 'Renormalization theory of quantum electrodynamics: an individual view' [199] in which he concluded, 'And so, although Weisskopf was not the first to find the correct result, he was the first to insist on its correctness.'

We will have more to say about the story of the Lamb shift when we turn to Feynman.

## Tomonaga's covariant formulation of quantum field theory

Amid the devastation of the last years of the war in Japan, and in the immediate post-war period, a remarkable group around Sin-itiro Tomonaga in Tokyo made enormous progress in formulating a consistent, Lorentz invariant quantum electrodynamics, although the participants were completely cut off from the developments in America. This approach turned out to be remarkably similar to the covariant approach of Schwinger, although, in large measure due to their isolation, they were not able to carry the program fully through to a theory capable of producing reliable calculations. The West learned of this work through Tomonaga's communication to Oppenheimer, delivered by hand by the first Japanese students to visit the US after the war, particularly Katsumi Tanaka.* Oppenheimer arranged for a brief note to be published in the *Physical Review* by Tomonaga.[20] Oppenheimer evidently thought highly of Tomonaga's work, and referred to it in glowing terms in his Solvay report.[18] He attributed the origin of what Schwinger referred to as the 'interaction representation' to Tomonaga, and wrote down what he called the Tomonaga equation, namely Eqn. (8.2). He then described Schwinger's program as removing the 'virtual' transitions from the right-hand side of this equation by contact transformations.

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* Tanaka, who became a professor at The Ohio State University, recalled that he was one of the first group of Japanese students sent to the US after the war, and served as courier from Tomonaga to Oppenheimer.[30] Marshak[31] recalled that Tanaka delivered the Sakata–Inoue paper,[32] which suggested the existence of two mesons (see Chapter 12), to Oppenheimer in November 1947, and presumably the QED papers as well.

Dyson recalled that in Spring 1948 Tomonaga sent two copies of *Progress of Theoretical Physics* to Bethe. The second issue included Tomonaga's paper,[19] which contained a remarkable footnote stating that the work had been published in Japanese in 1943.[33] Dyson went on to say, 'The implications of this were astonishing. Somehow or other, amid the ruin and turmoil of the war, totally isolated from the rest of the world, Tomonaga had maintained in Japan a school of research in theoretical physics that was in some respects ahead of anything existing anywhere else at that time. He had pushed on alone and laid the foundations of the new quantum electrodynamics, five years before Schwinger and without any help from the Columbia experiments. He had not, in 1943, completed the theory and developed it as a practical tool. To Schwinger rightly belongs the credit for making the theory into a coherent mathematical structure. But Tomonaga had taken the first essential step. There he was, in the spring of 1948, sitting amid the ashes and rubble of Tokyo and sending us that pathetic little package. It came to us as a voice out of the deep.'[34]

What was the background of this remarkable accomplishment? We cannot, in a short space, do justice to the achievements of Sin-itiro Tomonaga. A brief history of his life and accomplishments is given in Schweber's book,[2] and further details may be found in the collected memoirs and reminiscences edited by Makinosuke Matsui.[35] The following synopsis of Tomonaga's life is extracted from Matsui's contributions to that book.

Tomonaga was born in Tokyo in 1906, the first son of a philosophy professor, Sanjuro Tomonaga, and his wife Hide. The next year his father was offered a post at Kyoto Imperial University, so the family moved to Kyoto. In 1909 Sanjuro went to Heidelberg to study, and remained there for four years; the family stayed in Tokyo with Hide's parents until 1913. On the father's return that year, they moved back to Kyoto, where they lived on the grounds of the Shogoin temple for more than a decade. Tomonaga was a sensitive child, in his own words, a 'crybaby,' and suffered from poor health. As a result, the family had to spend expensive summer vacations at seaside resorts. In 1918 Tomonaga graduated from Kinrin Elementary School, and enrolled in Kyoto First Middle School, a premiere academically-oriented school. Because his family was of samurai lineage, he was brought up strictly, in spite of his delicate health. The affection he did not receive from his immediate family, he received from his uncle, Masuzo Tomonaga.*

* Years later, because of this uncle's actions, Tomonaga would be unable to attend the Nobel Prize ceremony in Stockholm. After learning of his nephew being awarded the prize, Masuzo visited with a bottle of sake, and after drinking for some hours, Tomonaga slipped in the bath, breaking six ribs, making traveling impossible. But he always recalled

Tomonaga recalled that in elementary school he was not very good at physical education. 'But once, after we had run a lap around Heian Shrine in a sort of marathon, my teacher praised me because I had managed to run the entire course without dropping out. Running the course through was nothing extraordinary and dropping out was actually unusual, mind you, but children are always delighted to be praised whether or not what they have done is remarkable.'[36] He had fond memories of chemistry demonstrations in school. He initially had some difficulty in middle school, largely because he missed the entire first term because of illness. He recalled being stimulated by his mathematics teacher's educational style there. He also remembered a science teacher telling them about radioactivity, and the uranium that supposedly could be found in the mountains around Kyoto. A group of students decided to go on an expedition to find some, but Tomonaga caught a cold and was unable to go; in any case no uranium was found, at least no sample that glowed in the dark.

His brother, Yojiro Tomonaga, recalled that Tomonaga liked to make models and craft objects, even as an adult, which he attributed to his reading the magazine *Science for boys*. He also liked to take trick photographs. He started to paint from nature as a child.[37] Schweber recounts Tomonaga's early electric and optical projects, in which he built everything from scratch.[2]

In 1923 Tomonaga entered the Third High School, in Science Department B.* In high school, he still missed classes frequently because of illness. Although quiet, he was not always well-behaved; Masatada Tada recalled that he once smeared chalk dust all over a teacher's chair, but confessed and accepted the punishment willingly.[35]

From high school, Tomonaga was admitted in 1926 to the Faculty of Science of the Kyoto Imperial University. Fellow students included Hideki Yukawa, who had attended the same middle school as Tomonaga and who would later achieve fame for proposing the existence of the meson. Yukawa and Tomonaga learned quantum mechanics on their own by reading the original papers. After graduation from the University in 1929, Tomonaga and Yukawa stayed on as unpaid assistants. Tomonaga recalled attending lectures of Dirac and Heisenberg in Tokyo.[2] But in 1930 Yoshio Nishina returned from Europe, where he had been studying with Niels Bohr, and gave a lecture in Kyoto based on Heisenberg's book.[38] Tomonaga was inspired, and asked penetrating questions of Nishina,

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this incident with affection: 'You see, my uncle came around first thing in the morning with a bottle of sake, and . . . .'

* The educational system, modeled on Germany's, at the high school level consisted of five schools, numbered First through Fifth, located in Tokyo, Sendai, Kyoto, Kanazawa, and Kumamoto, respectively. The B in the department designation indicated that the first foreign language was German, rather than English or French.

who as a result offered him a position at the Institute of Physical and Chemical Research (Riken) in Tokyo the following year (1932). After a three-month trial period, Tomonaga, with some agonizing, joined the institute on a permanent basis, and gradually came out of his shell, and engaged in sports and social life.

There was an exchange agreement between the institute and Leipzig University, and in 1937 Tomonaga went to Leipzig to study with Heisenberg. Apparently, he was rather depressed there [200]. Two years later, he returned and, the following year, married Ryoko Sekiguchi. In 1939 he became a professor at Tokyo Bunrika University. There, he started the research program that eventually earned him the Nobel Prize. As the war intensified, the Japanese navy asked him to work on radar, and he developed a powerful magnetron, starting in 1943. In one of the remarkable parallels of history, at the same time, Julian Schwinger, in Cambridge, Massachusetts, was carrying out very similar work on microwave cavities. Both men independently developed the theory of the S, or scattering, matrix for waveguides, which would later have important implications for field theory and particle physics. Of course, at the time, neither had any knowledge that the other existed.

Just after the war, with the US Army occupying Japan, an American soldier drove up to Tokyo Bunrika University, asking for Professor Tomonaga. Since arrests of war criminals were in the news, some alarm was registered. It turned out that the soldier was physicist Philip Morrison, who had been involved in the dropping of the first bomb on Hiroshima, and was visiting to assess the damage wrought by the nuclear explosion. He was merely calling on Tomonaga to express his regards.

In 1946 Tomonaga won the Asahi Prize, for his work on meson theory and on the super-many-time theory, the proceeds of which (10,000 yen) he used to buy tatami mats to furnish a miserable abandoned building on the Okubo campus (which had previously belonged to the Japanese Imperial Army) for his family's residence. His group was already established in another concrete building on this site. For a graphic account of the facilties, see the article by Daisuki Ito in Ref. 35.

## Tomonaga's papers

Tomonaga's 1946 paper in *Progress of Theoretical Physics*[19] was entitled 'On a relativistically invariant formulation of the quantum theory of wave fields,' and was noted as having first been published in Japanese in 1943.[33] It generalized the Schrödinger equation by proceeding from the many-time formulation of Dirac.[39] That is, there were as many time variables as there were particle coordinates in the state vector. This suggested the introduction of infinitely many time variables, one for each space point, $t_{xyz}$, a local time, an idea which had also been introduced by Stückelberg.[40] From this perspective, he was able to define the state vector as a functional of the space-like surface $C$, $\Psi(C)$, which satisfied the functional Schrödinger equation

$$\left\{H_{12}(P) + \frac{\hbar}{\mathrm{i}}\frac{\delta}{\delta C_P}\right\}\Psi(C) = 0. \tag{8.17}$$

Here $H_{12}$ is the interaction Hamiltonian between the two fields that Tomonaga was considering and $C_P$ is a surface passing through the point $P$. Indeed, this is the same equation (8.2) that Schwinger would obtain five years later, in 1948, in his 'Quantum electrodynamics I' [50]. A form equivalent to the integral equation (8.7) was also given by Tomonaga in this paper, with nearly the same notation. Tomonaga rounded out the paper by giving generalized probability amplitudes and transformation functions.

To assess this work in context, it may be useful to quote the 'Concluding remarks': 'We have thus shown that the quantum theory of wave fields can be really brought into a form which reveals directly the invariance of the theory against Lorentz transformations. The reason why the ordinary formalism of the quantum field theory is so unsatisfactory is that it has been built up in a way much too analogous to the ordinary nonrelativistic mechanics. In this ordinary formalism of the quantum theory of fields the theory is divided into two distinct sections: the section giving the kinematical relations between various quantities at the same instant of time, and the section determining the causal relations between quantities at different instants of time. Thus the commutation relations belong to the first section and the Schrödinger equation to the second.

'As stated before, this way of separating the theory into two sections is very unrelativistic, since here the concept "same instant of time" plays a distinct role.

'Also in our formalism the theory is divided into two sections, but now the separation is introduced in another place. One section gives the laws of behaviour of the fields when they are left alone, and the other gives the laws determining the deviation from this behavior due to interactions. This way of separating the theory can be carried out relativistically.

'Although in this way the theory can be brought into more satisfactory form, no new contents are added thereby. So, the well-known divergence difficulties of the theory are inherited also by our theory. Indeed, our fundamental equations (8.17) admit only catastrophic solutions, as can be seen directly from the fact that the unavoidable infinity due to non-vanishing zero-point amplitudes of the fields inheres in the operator $H_{12}(P)$. Thus, a more profound modification of the theory is required in order to remove this fundamental difficulty.

'It is expected that such a modification of the theory could possibly be introduced by some revision of the concept of interaction, because we meet no such difficulty when we deal with the non-interacting fields. The revision would then have the result that in the separation of the theory into two sections, one for free fields and one for interaction, some uncertainty would be introduced.

This seems to be implied by the very fact that, when we formulate the quantum field theory in a relativistically satisfactory manner, this way of separation has revealed itself as the fundamental element of the theory.'

So, although Tomonaga indeed discovered the Tomonaga–Schwinger equation first, he was in 1943 still far from seeing how to resolve the fundamental problems of the theory. In particular, he believed that the solution lay in additional interactions. In contrast, Schwinger's conservative bent five years later led to the insistence on retaining the electromagnetic interaction, with the divergences absorbed by the process of renormalization.

As we recall, in 1948, Oppenheimer arranged to have a brief note published in the *Physical Review*, summarizing the progress in quantum electrodynamics which had occurred in Japan since the end of the war.[20] This note expressed the reaction to the news* of the experimental discovery of the Lamb shift, and Bethe's[6] and Schwinger's [43] theoretical contributions. Tomonaga first reported on his group's unsuccessful attempt[41] to use the method of compensation.[42] Then, after seeing the work of Bethe,[6] they were able to absorb infinities into a reinterpretation of the mass and charge of the electron, i.e. renormalization of these physical quantities. However, they made an error, and found additional divergences in the $e^2$ correction to the Klein–Nishina formula for Compton scattering. As Oppenheimer remarked in an attached comment, 'From manuscripts kindly sent by Tomonaga, I would conclude that the difficulties referred to in this note result from an insufficiently cautious treatment, and therefore inadequate identification, of light quantum self-energies.' The letter concludes with a statement that a calculation of the Lamb shift was in progress (by Yoichiro Nambu), which included the anomalous magnetic moment effect found by Schwinger [43]. By September 1948 Tomonaga's group had reproduced the correct relativistic Lamb shift calculation of French and Weisskopf,[23] albeit using non-covariant techniques.[43] The paper appeared in 1949, as did Schwinger's [53] and Feynman's[26] papers on the Lamb shift.

### Tomonaga after the war

As we have seen, Oppenheimer was very impressed with Tomonaga's accomplishments in wartime Japan, and invited him to spend a year at the Institute for Advanced Study. This he did during the academic year 1949–50. When he returned to Japan, he soon abandoned scientific work for the role of science administrator. In part, this was because of Nishina's death in January 1951, so Tomonaga became the chairman of the Liaison Committee for Nuclear Research. He was responsible for the establishment of various national research institutes, such as Yukawa Memorial Hall, renamed first the Research Institute for Fundamental Physics, and then the Yukawa Institute for Theoretical Physics, in Kyoto. He was evidently a skillful administrator, known for the 'Tomonaga method of arbitration,' making 'decisions when everyone gets tired.' Yet, there was a great loss to physics when this genius stopped making original contributions to fundamental knowledge.

Tomonaga was modest about his contributions. In a letter he wrote from Princeton in 1950 to Ziro Koba, 'At the end of last year, I listened to Dr. J. Schwinger's lecture at Columbia.* He also seems to have a new idea. It is a very ambitious plan to put Dr. F. J. Dyson's argument in a closed form without a series expansion in powers of $e^2/\hbar c$. (I hear that Toichiro Kinoshita, too, has the same ambition. I myself wanted to do the same and struggled long and hard, but in vain. I am disgusted with my lack of progress. . . . ) Anyway, the three, J. Schwinger, R. Feynman and F. J. Dyson, are great men, and I must admit defeat. (I have not met Dyson yet—he, too, might come to Princeton next school year, I hear.) People refer to the Tomonaga–Schwinger theory or the Schwinger–Tomonaga theory (especially in Japan), but the comparison of the two may be likened to the one between H. A. Lorentz and G. F. FitzGerald of the Lorentz–FitzGerald contraction (no need to tell which one corresponds to Lorentz and which to FitzGerald). I hear that my name has appeared in various magazines and newspapers following the awarding of the Nobel Prize to Hideki Yukawa, and I find it very embarrassing.'[35]

### Schwinger's view of Tomonaga

Ultimately, Schwinger did not feel that the work of Tomonaga made a significant impact, at least on his own work. 'I had nothing to learn from what Tomonaga and his group had done, because when it came to things like the Lamb shift, they were just dutifully following previous success. I have no doubt that they were still using a subtraction theory—it was not satisfactory. It was done in a pseudo-physical context by having a spin-zero particle or something that would produce a negative mass change that would cancel the positive mass change, which does nothing for vacuum polarization. Oppenheimer then pointed out in response to this shipment of papers that whatever [Tomonaga] was doing had no effect on

* In a December 1949 letter to Tatuoki Miyazima, he says about Schwinger's lecture: 'His lucid talk was very impressive. Until I heard his lecture, I thought him to be adept only in steamroller-tactic calculations, and not so sharp. However, I realized that he was not such a man as I had imagined, but one who was working with a very clear concept of physics. His lecture was on renormalization—how we can renormalize the mass and the electric charge in closed form. Dyson subtracted infinity by using a series expansion in powers of $\alpha(= e^2/\hbar c)$. Therefore, his method is good only for collision problems, but not appropriate for dealing with the bound state. Schwinger tried ambitiously to subtract infinity in a way appropriate for bound-state problems, and to reduce the subtraction to renormalizations of mass and charge. Of course, I was able to understand only the basic gist of his thinking.'[35]

the divergences, so-called, in the photon mass.'[1] Writing down the Tomonaga–Schwinger equation was not the major step: 'I'd like to make the point that from a covariant formulation to a covariant calculation is a big step. It seems to me the formulation was trivial. It was carrying through the calculation that was the important thing. The thing that surprises me is that so many people refer to this 1943 paper as anticipating the whole line of development of renormalization theory and so on. That is a formalism without any physical content. The idea of renormalization is a very specific strategy of isolating parts of the result, identifying them as being altered properties of the individual particles, and going on. To point to a vacuous covariant theory and say that's the whole thing is patently wrong.'[1] This feeling of paucity of the Japanese contribution at the time would present Schwinger with difficulties years later when he would deliver a memorial lecture in Tokyo in honor of Tomonaga. Nevertheless, he did deliver a moving tribute, which we will describe in Chapter 16.

In his history of his development of quantum electrodynamics, Schwinger elaborated on this point: 'I have read remarks to the effect that if scientific contact had not been broken during the Pacific war, the theory that we are reviewing here would have been significantly advanced. Of course, lacking an unlimited number of parallel universes in which to act out all possible scenarios, such statements are meaningless. Nevertheless, I shall be bold enough to disagree. The preoccupation of the majority of involved physicists was not with analyzing and carefully applying the known relativistic theory of coupled electron and electromagnetic fields but with changing it. The work of Tomonaga and his collaborators, immediately after the war, centered about the idea of compensation, the introduction of the fields of unknown particles in such a way as to cancel the divergences produced by the known interactions.[41] Richard P. Feynman also advocated modifying the theory, and he would later intimate that a particular, satisfactory modification could be found.[26] My point is merely this: A formalism such as the covariant Schrödinger equation is but a shell awaiting the substance of a guiding physical principle. And the specific concept of the structure-independent renormalized relativistic electrodynamics, while always abstractly conceivable, in fact required the impetus of experiments to show that electrodynamic effects were neither infinite nor zero, but finite and small, and demanded understanding.' [199]

## Feynman's theory of positrons, and the space–time approach to quantum electrodynamics

Much has been written about Feynman's scientific accomplishments, both at the popular level,[44] and from the scholarly point of view. For the latter, we refer the reader to Schweber's book[2] and to Mehra's biography.[45] As we will see, Feynman's approach to quantum electrodynamics seemed to be totally different from that of Schwinger and Tomonaga, or indeed from that of any of the field theorists of the 1930s and 1940s. His approach was far more intuitive (to him at least), less mathematical (on the surface anyway), and apparently revolutionary (as opposed to Schwinger's conservative road); yet remarkably, as both Feynman and Schwinger came to realize in 1948, the two procedures were equivalent. Freeman Dyson proved that equivalence in 1949.

Feynman started on his unorthodox path at Princeton, while working on his PhD with John Archibald Wheeler. Already, while he was an undergraduate at MIT, he was concerned with the infinities of electrodynamics, in particular the infinite self-action of the electron on itself. Perhaps, he thought, one could just impose a rule that a given electron does not interact with itself. But that could not be correct, because radiation reaction, which must be present to preserve the energy balance between the electron and the electromagnetic field, would then not occur either. Feynman and Wheeler got the idea that the self-action could be eliminated by making what seemed like an outrageous change in the boundary conditions of ordinary classical electrodynamics: Instead of having only retarded waves, in which the waves reach the observer from the past, they proposed having a classical electrodynamics in which one had half-retarded and half-advanced waves, waves which come from the future. This had the theoretical advantage of being time-symmetric, that is, invariant under the change of the sense of the flow of time, from past to future, to future to past, so that the boundary conditions in time mirror the symmetry in Maxwell's equations. It was not quite as simple as that, in that perfectly absorbing boundaries had to be assumed as well. But then radiation reaction could be accounted for, as Feynman noted later: 'It became clear that there was the possibility that if we assume all actions are via half-advanced and half-retarded solutions of Maxwell's equations and assume that the sources are surrounded by material absorbing all the light which is emitted, then we could account for radiation resistance as direct action of the absorber acting back by the advanced waves on the source.'[46]

Wheeler proposed that Feynman give a colloquium in the fall of 1940 at Princeton on their joint work, and Wheeler would follow later with a colloquium on the corresponding quantum theory, which Wheeler, but not Feynman, thought would be an easy generalization. Feynman was terrified, because Pauli would be in the audience, but Wheeler promised to take care of all of Pauli's questions. Although Pauli indeed asked questions, no one in attendance could, in later years, recall them. Einstein remarked that it would be difficult to follow the same path in gravitation theory, but since that was a much less well-established theory, that was not a serious argument against the approach.[45,2] Later, in February 1941, Feynman gave a talk on time-symmetric electrodynamics at a meeting of the American Physical Society in Cambridge.[47]

Only after the war did the Wheeler–Feynman paper[48] finally appear, a long paper written almost entirely by Wheeler. It described only classical electrodynamics, as an action-at-a-distance theory. Each charged particle was the source of an advanced and a retarded field, which only acted on other particles. Only the particles are fundamental entities. 'From the overall space–time view of the least action principle, the field disappears as nothing but bookkeeping variables insisted on by the Hamiltonian method.'[46]

Early in the collaboration between Wheeler and Feynman, an idea occurred to Wheeler that would be very important for Feynman's later thinking about quantum electrodynamics. The question was why do all electrons possess the same mass and charge. 'Because,' said Wheeler in 1940, 'they are all one and the same electron.'[46,49] By this, Wheeler meant that there was only one worldline of an electron, which zig-zagged, sometimes going forward in time, in which case it was an electron, and sometimes going backwards in time, in which case we saw it as a positron, with the same mass as the electron, but with the opposite charge. Feynman doubted there was but one such electron in the world (if so, the number of electrons and positrons would seem to have to be the same, manifestly in contradiction to experience), but very much liked the idea that a positron was merely an electron going backward in time. It seemed a much more attractive idea than Dirac's holes in a filled electron sea. This notion would play a crucial role in Feynman's diagrammatic interpretation of quantum electrodynamics at the end of the decade. (See Fig. 8.1 below.)

The next step in Feynman's journey was the principle of least action. The action, for a single classical particle with coordinate $q(t)$, is given by the integral

$$S[q(t)] = \int_{t_1}^{t_2} L(\dot{q}, q, t)\mathrm{d}t, \tag{8.18}$$

where $t_1$ and $t_2$ are the initial and final times, and $L$ is the Lagrangian of the system. The classical stationary action principle states that the trajectory of the particle is such that the action $S$ is an extremum, which yields the Lagrange equation,

$$\frac{\partial L}{\partial q} - \frac{\mathrm{d}}{\mathrm{d}t}\frac{\partial L}{\partial \dot{q}} = 0. \tag{8.19}$$

These equations may be immediately extended to a system described by an arbitrary number of generalized coordinates, $q_a$.

Feynman's inspiration for the quantum theory, as had Schwinger's, came from Dirac. In this case it was his paper 'The Lagrangian in quantum mechanics,'[50] a paper which would be the springboard for Schwinger's later action-principle-based field theory, which we will describe in the next chapter.* In that paper Dirac stated that the transformation function in the coordinate representation between two different times, $(x_{t_2}|x_{t_1})$, 'is *analogous to*' $\exp[(i/\hbar)S(x_2, t_2; x_1, t_1)]$, the exponential factor being the action carrying a particle from an initial position $x_1$ at time $t_1$ to a final position $x_2$ at time $t_2$. No one, including Dirac, seemed to know what 'analogous to' meant in this case. Perhaps, Feynman thought in 1941, that it meant 'equal [or, rather, proportional] to.' Thus was born Feynman's famous path integral.

In fact the transformation function and $e^{iS/\hbar}$ were proportional if the time interval were short, $t_2 - t_1 \ll t_1$. To calculate the transformation function $K(X, T; x, t)$ that carries one from a wavefunction $\psi(x, t)$ to a wavefunction $\psi(X, T)$ required breaking up the interval into a great many steps, say $N$, and integrating over each intermediate position:

$$K(X, T; x, t) =$$
$$\int \exp\left[\frac{i}{\hbar}\sum_{i=0}^{N-1} L\left(\frac{x_{i+1} - x_i}{t_{i+1} - t_i}, x_{i+1}\right)(t_{i+1} - t_i)\right]\frac{dx_N}{A_N}\cdots\frac{dx_i}{A_i}, \tag{8.20}$$

where $A$'s are some constants, which can be easily worked out in simple cases, but which are usually irrelevant. One is supposed to take the limit as the number of intervals $N$ goes to infinity, at the same time as the size of all the time intervals goes to zero; in that sense it resembles the definition of a Reimann integral.

In 1942, Feynman wrote up his PhD thesis which consisted of the work on the new approach to quantum mechanics and the action-at-a-distance electrodynamics. (These were only fully described after the war, the first in the joint paper with Wheeler,[48] and the second in an article by himself, entitled 'Space–time approach to nonrelativistic quantum mechanics.'[51]) He then spent his full time on war work, and soon, after his marriage to Arline Greenbaum, departed

* Schwinger would later remark, 'Dirac was central to this in the connection between quantum mechanics and classical mechanics, shall we say. Action in general. There are two different ways of looking at it. Feynman picked up the integral aspect of it in which you combine little steps in time into an integral formulation. I picked up another remark in that very same paper, namely the differential aspect, the quantum aspects and analogies with Hamilton–Jacobi and so forth. So ultimately to the extent that we finally diverged with attitudes about reformulations of quantum mechanics—which is what I think this is all really about—we were both inspired by Dirac, but took two different avenues, which are equivalent in limited contexts. I like to think that the differential aspect is more fundamental, because it is not based on mimicking of a classical situation. If everything is classical, then what do you do about non-classical degrees of freedom, like Fermi–Dirac fields and spins and such things. Whereas the differential aspect allows both possibilities, it is not so confining in the nature of the system to which it refers.'[1]

for Los Alamos, where he was placed in charge of Theoretical Computations. It would not be until he was well settled as a professor at Cornell, in 1946, that he would again resume fundamental research. But his debt to his advisor, John Wheeler, with his tremendous geometrical way of thinking, was incalculable, for it would lead to Feynman's space–time view of electrodynamics.

### Feynman after Shelter Island

Like Schwinger, Feynman was excited by the experimental results announced at the Shelter Island Conference in June 1947. He set to work, and by the time of the Pocono Conference the following March, he, like Schwinger, had a relativistically invariant computational scheme. We have described Feynman's presentation at Pocono in the last chapter. But that conference belonged to Schwinger, and Feynman's unconventional approach was not received with much favor. He realized that only through publication could he hope to convince the community that he was on the right track.

As we described in Chapter 7, at the January 1948 APS meeting in New York, after Schwinger's famous repeated lecture on the anomalous magnetic moment and the preliminary unsatisfactory situation with the relativistic Lamb shift calculation, Feynman got up and stated that he agreed with Schwinger's results, but he, unlike Schwinger, had the correct value of the anomalous magnetic moment for an electron in the atom. (Actually, the discrepancy was with the corresponding electrical coupling obtained from the magnetic one by a relativistic transformation.) He was at that time feeling a tremendous sense of competition with Schwinger, who had got a head start on him, but now Feynman felt, probably overconfidently, that he had caught up.[2,45]

Feynman published two relatively short papers bearing on this subject in the summer of 1948. The first was entitled 'A relativistic cut-off for classical electrodynamics,'[52] which was based on an expanded version of a manuscript he had written in 1941.[45,2] This paper dealt largely with the action-at-a-distance formulation he worked on before getting involved in the war effort, but now with a density of field quanta playing the role of a regulator, so that the self-energy of a particle was made finite. A similar idea was present in the second paper, 'Relativistic cut-off for quantum electrodynamics.'[22] He used this to calculate the self-energy of the electron, ($\mu$ = electron mass)

$$\delta\mu = \mu\frac{e^2}{\pi}\left[\frac{3}{2}\ln\frac{\lambda_0}{\mu} + \frac{3}{8}\right], \tag{8.21}$$

where $\lambda_0$ is a cutoff, which in conventional electrodynamics would tend to infinity. This is the result (8.13), first obtained in the old quantum field theory by Weisskopf,[12] published by Schwinger five months later [52]. In fact, this paper directly precedes Schwinger's 'Quantum electrodynamics I' in the *Physical*

*Review*, which was received by the journal about two weeks after Feynman's paper. Since it uses old-fashioned methods, which Feynman used in part to make it acceptable to other physicists,[2] this paper is mainly remembered for its incorrect discussion of the relativistic Lamb shift, which we will describe below.

The moment when Feynman achieved confidence in the power of his methods came at the January 1949 APS meeting in New York. This is the famous story of Murray Slotnick, who had spent six months calculating a certain interaction between electrons and neutrons using either a pseudoscalar or a pseudovector interaction. The first form gave a finite result, while the second was divergent. After his talk, Oppenheimer challenged Slotnick: 'What about Case's theorem?' Ken Case, a former student of Schwinger's who was then a postdoc at the Institute for Advanced Study, had a proof that the pseudovector and pseudoscalar theories were equivalent. Feynman was intrigued, so he talked to Slotnick, and that evening he worked out the general result for arbitrary momentum transfer. When he talked to Slotnick the next day, Feynman found that Slotnick only had the result for zero momentum transfer. But in that limit they agreed. Feynman was ecstatic: 'That was the moment when I got my Nobel prize, when Slotnick told me he had been working for two years. When I got the real prize, it was really nothing, because I already knew I was a success.'[2] Later, after he learned the meaning of creation and annihilation operators, Feynman found the error in Case's theorem.[2,45,53]

Feynman's substantial papers on quantum electrodynamics appeared in 1949. These were 'The theory of positrons,'[54] received by *Physical Review* on 8 April 1949, and 'Space–time approach to quantum electrodynamics,'[26] received a month later. The validity of the rules given in these two papers was demonstrated in a third paper, 'Mathematical Formulation of the Quantum Theory of Electromagnetic Interactions,'[55] which arrived at *Physical Review* over a year later, on 8 June 1950. (All three of these papers are reprinted in Schwinger's collection [83].)

'The theory of positrons' is summarized in the abstract. 'The problem of the behavior of positrons and electrons in given external potentials, neglecting their mutual interaction, is analyzed by replacing the theory of holes by a reinterpretation of the solutions of the Dirac equation. It is possible to write down a complete solution of the problem in terms of boundary conditions on the wave function, and this solution contains automatically all the possibilities of virtual (and real) pair formation and annihilation together with the ordinary scattering processes, including the correct relative signs of the various terms.

'In this solution, the "negative energy states" appear in a form which may be pictured (as by Stückelberg[56]) in space–time as waves traveling away from the external potential backwards in time. Experimentally, such a wave corresponds to a positron approaching the potential and annihilating the electron. A particle moving forward in time (electron) in a potential may be scattered forward in time (ordinary scattering) or backward (pair annihilation). When moving backward (positron) it may be scattered backward in time (positron scattering) or forward (pair production). For such a particle the amplitude for transition from an initial to a final state is analyzed to any order in the potential by considering it to undergo a sequence of such scatterings.

'The amplitude for a process involving many such particles is the product of transition amplitudes for each particle. The exclusion principle requires that antisymmetric combinations of amplitudes be chosen for those complete processes which differ only by exchange of particles. It seems that a consistent interpretation is only possible if the exclusion principle is adopted. The exclusion principle need not be taken into account in intermediate states. Vacuum problems do not arise for charges which do not interact with one another, but these are analyzed nevertheless in anticipation of application to quantum electrodynamics.

'The results are also expressed in momentum–energy variables. Equivalence to the second quantization theory of holes is proved in an appendix.'

Feynman began by considering a classical picture of pair production, followed by positron annihilation. An electron–positron pair is produced at time $t_1$, after which two world lines, corresponding to the electron and positron, advance forward in time. At some later time $t_2$ the positron is annihilated by another electron. The picture might be as sketched in Fig. 8.1. As he said, 'Following the charge rather than the particles corresponds to considering this continuous world line as a whole rather than breaking it up into pieces. It is as though a bombardier flying low over a road suddenly sees three roads and it is only when

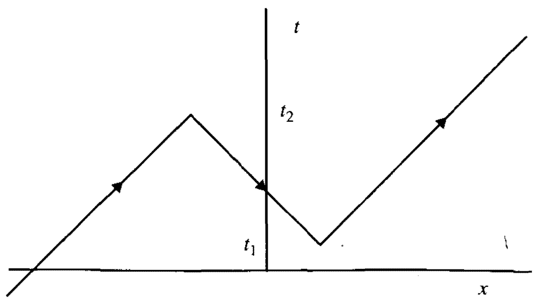


Fig. 8.1 Space–time diagram of electron–positron pair production, followed by annihilation of the positron by another electron. The arrows pointing in an upward sense denote electrons moving forward in time, while arrows pointing in a downward sense denote electrons moving backward in time, or positrons moving forward in time.

two of them come together and disappear that he realizes that he has simply passed over a long switchback in a single road.'*

Feynman went on to consider the Green's function for Schrödinger's equation, which he defines as relating the wavefunction at two different space–time points:

$$\psi(\mathrm{x}_2, t_2) = \int K(\mathrm{x}_2, t_2; \mathrm{x}_1, t_1)\psi(\mathrm{x}_1, t_1)\mathrm{d}^3\mathrm{x}_1. \tag{8.22}$$

He proceeded to solve the Dirac equation for a particle of mass $m$ in an external potential $A_\mu$ (here he used the notation $\mathrm{A} = \gamma_\mu A_\mu$, $\nabla = \gamma_\mu \partial_\mu$)

$$(\mathrm{i}\nabla - m)\psi = \mathrm{A}\psi \tag{8.23}$$

in terms of the Green's function, which satisfies

$$(\mathrm{i}\nabla_2 - \mathrm{A} - m)K_+^{(A)}(2, 1) = \mathrm{i}\delta(2, 1). \tag{8.24}$$

Here Feynman had adopted a compressed notation, in which the numbers 2 and 1 stand for the space–time coordinates with the respective index. It is clear that this differential equation is equivalent to the integral equation

$$K_+^{(A)}(2, 1) = K_+(2, 1) - \mathrm{i}\int K_+(2, 3)\mathrm{A}(3)K_+^{(A)}(3, 1)\mathrm{d}\tau_3, \tag{8.25}$$

where the Green's function without the superscript is a solution to Eqn. (8.24) with $A = 0$. The subscript here refers to the appropriate boundary conditions in time. In order that Feynman's theory be equivalent to the hole theory, he had to choose the free Green's function so that it involved a sum over positive energy states for positive time differences, and a sum over negative energy states for negative time differences:

$$\begin{aligned} K_+(2, 1) &= \sum_{\mathrm{pos}E_n} \phi_n(2)\bar{\phi}_n(1)\mathrm{e}^{-\mathrm{i}E_n(t_2-t_1)}, \quad t_2 > t_1, \\ &= \sum_{\mathrm{neg}E_n} \phi_n(2)\bar{\phi}_n(1)\mathrm{e}^{-\mathrm{i}E_n(t_2-t_1)}, \quad t_2 < t_1. \end{aligned} \tag{8.26}$$

Here $\phi_n$ is an eigenfunction of the free Dirac Hamiltonian, with energy $E_n$, and $\bar{\phi}_n = \phi_n^*\beta$ is the Dirac conjugate.

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* In an interview with Schweber, Feynman stated that this metaphor 'was suggested to me by some student at Cornell (who had actually been a bombardier during the war) when I was writing up the paper and was asking for opinions of how to explain it and only had poor or awkward metaphors.'[2]

Quantum electrodynamics proper is the subject of the second paper, 'Space–time approach to quantum electrodynamics.'[26]* The first paragraph of the abstract gives a good summary: 'In this paper, two things are done. (1) It is shown that a considerable simplification can be attained by writing down matrix elements for complex processes in electrodynamics. Further, a physical point of view is available which permits them to be written down for any specific problem. Being simply a restatement of conventional electrodynamics, however, the matrix elements diverge for complex processes. (2) Electrodynamics is modified by altering the interaction of electrons at short distances. All matrix elements are now finite, with the exception of those relating to problems of vacuum polarization. The latter are evaluated in a manner suggested by Pauli and Bethe, which gives finite results for these matrices also. The only effects sensitive to the modification are changes in mass and charge of the electrons. Such changes could not be directly observed. Phenomena directly observable, are insensitive to the details of the modification used (except at extreme energies). For such phenomena, a limit can be taken as the range of the modification goes to zero. The results then agree with those of Schwinger. A complete, unambiguous, and presumably consistent, method is therefore available for the calculation of all processes involving electrons and photons.'

In this paper, Feynman gives the famous Feynman rules and the Feynman diagrams. These may be illustrated in the momentum–space diagram, representing the 'interaction of an electron with itself,' shown in Fig. 8.2. This diagram has a precise mathematical correspondence with a quantum mechanical amplitude, in this case, the divergent integral,

$$\frac{e^2}{\pi i}\int \gamma_\mu(\mathrm{p}-\mathrm{k}-m)^{-1}\gamma_\mu \mathrm{k}^{-2} d^4k. \tag{8.27}$$

Indicated in the figure are the various factors that are assembled in order to construct the amplitude (8.27).

As he stated, Feynman's second step was a modification of electrodynamics so that these integrals would be rendered convergent. He does this, in effect, by modifying the photon propagator $1/k^2$ by multiplying it with a convergence

* In a remarkable demonstration of how close the competition was between Feynman and Schwinger, this paper appeared in the *Physical Review* directly before Schwinger's 'QED III,' which was received exactly 17 days later, on 26 May 1949. Recall that Feynman's 'Relativistic cut-off in quantum electrodynamics' had also appeared directly before Schwinger's 'QED I,' which again was received by the journal exactly 17 days after Feynman's paper, on 29 July 1948.

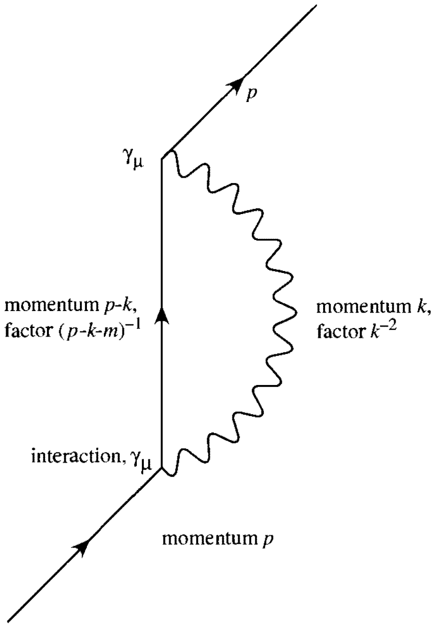


**Fig. 8.2** Feynman diagram representing the electron self-energy in momentum space.

factor $C(k^2)$ which falls off at least as fast as $1/k^2$, so now integrals such as that in Eqn (8.27) converge. For example, we could take $C(k^2) = \lambda^2/(\lambda^2 - k^2)$, which tends to unity as $\lambda \to \infty$. (Actually, Feynman proposed averaging over a weight function $G(\lambda)$, with the property $\int_0^\infty \lambda^2 G(\lambda)\mathrm{d}\lambda = 0$.) Doing so for the case of the process here, given by Eqn (8.27), gave a result for the electron mass shift exactly of the form (8.21), as given first by Feynman and then by Schwinger the year before.

Feynman next considered radiative corrections to scattering, in particular the Lamb shift. There he admitted the error he had previously published in the 'Relativistic cut-off for quantum electrodynamics.'[22] The story is recounted in his famous footnote 13: 'That the result given in B[22] was in error was repeatedly pointed out to the author, in private communication, by V.F. Weisskopf and J.B. French, as their calculation, completed simultaneously with the author's early in 1948, gave a different result. French has finally shown that although the expression for the radiationless scattering . . . is correct, it was incorrectly joined onto Bethe's nonrelativistic result. He shows that the relation $\ln 2k_{\mathrm{max}} - 1 = \ln \lambda_{\mathrm{min}}$ used by the author should have been $\ln 2k_{\mathrm{max}} - 5/6 = \ln \lambda_{\mathrm{min}}$. This results in adding a $-1/6$ to the logarithm in B, Eqn. (8.19), so that the result now agrees with that of J.B. French and V.F. Weisskopf[23] and N.M. Kroll and W.E. Lamb.[24] The author feels unhappily responsible for the very considerable delay in the publication of French's result occasioned by this error. This footnote is appropriately numbered.' *

However, Feynman faced a real difficulty with vacuum polarization. His 'regularization' scheme did nothing to remove the divergence associated with a closed electron loop, as given by the amplitude

$$J_{\mu\nu} = -\frac{e^2}{\pi \mathrm{i}} \int \mathrm{Sp}[(\mathrm{p} + \mathbf{q} - m)^{-1} \gamma_\nu (\mathrm{p} - m)^{-1} \gamma_\mu] \mathrm{d}^4 p, \tag{8.28}$$

where Sp = Spur is the old notation for trace. He continued to suggest that perhaps such closed loops did not exist, harking back to his collaboration with Wheeler, and the suggestion that there be but one electron in the universe. That view made the idea of closed electron loops 'unnatural.' Of course, Schwinger knew better,† as did Feynman. He realized that in the hole theory they were necessary for probability conservation. He suggested that the Lamb shift measurement be sufficiently improved so that the vacuum polarization contribution, which amounted to −27 MHz compared to a total splitting of 1050 MHz, could be experimentally confirmed.

He did finally discuss a method of regularizing vacuum polarization which he attributed (without reference) to Bethe and Pauli. This evidently was the Pauli–Villars technique,[21] which Feynman call 'the superposition of the effects of quanta of various masses (some contributing negatively).' This gave rise to a renormalization of the charge, again depending logarithmically on a cutoff $\lambda$,

$$\frac{\Delta e^2}{e^2} = -\frac{2e^2}{3\pi} \ln \frac{\lambda}{m}, \tag{8.29}$$

equivalent to Eqn (6.25).

Feynman closed the paper by discussing spin-0 particles, and meson theories in this language. This was a payoff from the Slotnick episode. He was able to reproduce all sorts of meson-theoretic calculations using his rules to order $g^2$

* According to footnote 8 in an earlier-published paper of Dyson,[57] it was Schwinger who detected the incorrect use of the insertion of a 'photon mass' to match the high-energy with the low-energy contributions to the Lamb shift.

† Schwinger remarked: 'Vacuum polarization means no more than that an electron–positron combination is coupled to the electromagnetic field and it may show itself really or virtually as you like.' Schwinger knew that vacuum polarization was real from his work with Oppenheimer at Berkeley [15]. And his work on classical electrodynamics was invaluable: as with the resistive and reactive parts in synchrotron radiation, 'the overtly physical and the implicitly physical parts are all connected together, you don't keep one and throw the other one away. In other words, I had lots of preparation in other areas of physics. I'm not sure Feynman did. He was too abstract.'[1]

very easily, much to his delight. But comparison with experiment was not very fruitful, because of the largeness of the coupling.

Feynman's paper on the 'Mathematical formulation of the quantum theory of electromagnetic interaction'[55] was designed to justify the space–time procedure given in the previous papers, and supply the 'proof of the equivalence of these rules to the conventional electrodynamics.' (In fact, the first four sections of this paper were written in 1947, much of which duplicated the work in Feynman's thesis.[2]) It was followed a year later by 'An operator calculus having applications in quantum electrodynamics,'[58] which was completed while Feynman was on leave of absence in Brazil, before taking up his new permanent appointment at Caltech. It is important to note Feynman's leisurely publication schedule. As Feynman said, 'Dates don't mean anything. It was published in 1951, but it had all been invented by 1948.'[59]

This last paper remains of some interest.* Feynman began by discussing the ordering of operators, in particular the meaning of $e^{A+B}$ when $A$ and $B$ are non-commuting. The question is, how is this 'disentangled' into its dependence on the individual operators, for only if $A$ and $B$ commute is it equal to $e^A e^B$. This is a subject for which the work of Schwinger is justly famous.† Feynman went on to apply his calculus to quantum mechanics, in particular to a system coupled to a harmonic oscillator, and to field theory, quantum electrodynamics in particular. He supplied his own derivation of the Tomonaga–Schwinger equation (8.2). He used his procedure to supply 'an independent deduction of all the main formal results in quantum electrodynamics, by use of the operator notation.' He then rederived the quantum-mechanical amplitudes for processes he had computed by his intuitive technique in Ref. 26.

These papers completed Feynman's program in quantum electrodynamics. 'With this paper I had completed the project on quantum electrodynamics. I didn't have anything remaining that required publishing. In these two papers[55,58] I put everything I had done and thought should be published on the subject. And that was the end of my published work in the field.'[59]

Feynman left the field of quantum electrodynamics in triumph, but personally he was dissatisfied. He thought that he would solve the problem of the divergences in the theory, that he would 'fix' the problem, but he didn't. 'I invented a better way to figure, but I hadn't fixed what I wanted to fix. I had

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* According to the *Science Citation Index*,[60] this paper had a very respectable 19 citations in 1997 alone.

† This general problem was discussed in an appendix to a paper Schwinger wrote with Robert Karplus, with the unlikely title of 'A note on saturation in microwave spectroscopy' [44], received by *Physical Review* on 9 January 1948. We mentioned this paper briefly in Chapter 5.

kept the relativistic invariance under control and everything was nice ... but I hadn't fixed anything. ... The problem was still how to make the theory finite.... I wasn't satisfied at all.'[2] In fact, in 'Space–time approach to quantum electrodynamics' he apologized for not having solved the problem: 'The desire to make the methods of simplifying the calculation of quantum electrodynamic processes more widely available has prompted this publication before an analysis of the correct form for the [cutoff function] $f_+$ is complete.'[26] He was also disappointed that his space–time picture of electrodynamics wasn't really new, that it was, in fact, equivalent to the conventional field theory of Schwinger and Tomonaga. He had hoped to eliminate fields entirely as fundamental entities in favor of particles, but field theory had triumphed in the end.

Schwinger's perspective

Let us conclude this section by giving Schwinger's perspective on Feynman's contributions to the development of quantum electrodynamics, extracted from the Preface to *Quantum electrodynamics* [83]. Referring to his own line of attack he stated: 'Throughout these developments the basic view of electromagnetism was that originated by Maxwell and Lorentz—the interaction between charges is propagated through the field by local action. In its quantum-mechanical transcription it leads to formalisms in which charged particles and fields appear on the same footing dynamically. But another approach is also familiar classically; the field produced by arbitrarily moving charges can be evaluated, and the dynamical problem reformulated as the purely mechanical one of particles interacting with each other, and themselves, through a propagated action at a distance. The transference of this line of thought into quantum language[54,26,55] was accompanied by another shift in emphasis relative to the previously described work. In the latter, the effect on the particles of the coupling with the electromagnetic field was expressed by additional energy terms which could then be used to evaluate energy displacements in bound states, or to compute corrections to scattering cross-sections. Now the fundamental viewpoint was that of scattering, and in its approximate versions led to a detailed space–time description of the various interaction mechanisms. The two approaches are equivalent; the formal integration of the differential equations of one method supplying the starting point of the other.[61] But if one excludes the consideration of bound states, it is possible to expand the elements of a scattering matrix in powers of the coupling constant, and examine the effects of charge and mass renormalization, term by term, to indefinitely high powers. It appeared that, for any process, the coefficient of each power in the renormalized coupling constant was completely finite.[57] This highly satisfactory result did not mean, however, that the act of renormalization had, in itself, produced a more correct theory. The convergence of the power series is not established, and the series doubtless has the significance of an asymptotic expansion. Yet, for practical purposes, in which the smallness of the coupling constant is relevant, this analysis gave assurance that calculations of arbitrary precision could be performed.'

## Dyson and the equivalence of the radiation theories of Schwinger, Tomonaga, and Feynman

As we have mentioned, already in 1948 (although the proof was only published in 1950[55]) Feynman had proved, to his satisfaction, the equivalence of his space–time approach to quantum electrodynamics, and the more conventional, yet equally brilliant, canonical approach of Schwinger. But Feynman never received the credit for this demonstration, largely because of his slow publication schedule. In fact, as we have just seen, it is invariably Freeman Dyson who is credited with proving the equivalence of the two, seemingly very different, approaches to quantum field theory.

We have recounted Dyson's interactions with Schwinger and Feynman in the previous chapter. When Bethe showed Dyson the letter Tomonaga had written to Oppenheimer, Dyson was delighted, for he found Tomonaga's exposition transparent, whereas the notes from Schwinger's lectures at Pocono seemed complicated, and penetrable only by the master himself.[2] Dyson attended the Michigan lectures of Schwinger in the summer of 1948, finding them 'unbelievably complicated.' Dyson felt Schwinger's approach 'couldn't be the way to do it,' for it was 'something that needed such skills that nobody besides Schwinger could do it. If you listened to the lectures you couldn't see the motivation; it was all hidden in this wonderful apparatus.'[2] In contrast, by this time he was already on very friendly terms with Feynman, with whom he had driven across the country. So before he took up his new residence in Princeton, he had already established his allegiance.

He saw early on, perhaps more explicitly than did either Feynman or Schwinger, the connection between the two methodologies. What is remarkable is that he published his papers, 'The radiation theories of Tomonaga, Schwinger, and Feynman,'[61] and 'The *S* matrix in quantum electrodynamics,'[57] received by *Physical Review* on 6 October 1948 and 14 February 1949, well before Feynman's central paper, 'The theory of positrons,'[54] received on 8 April 1949. Moreover, the first appeared before Schwinger's 'QED II' [52], which established the divergence structure of the theory, and both before 'QED III,' Schwinger's definitive paper of the triad. It could be argued that Dyson's alacrity in publication ensured his place in history, whereas had he published after the principals had completed their expositions, his contributions would have appeared more minor.

In his first paper, Dyson started from the Tomonaga–Schwinger equation (8.2), which makes reference to the interaction representation. He then gave a perturbative solution to that equation for the time-evolution operator in powers of the interaction Hamiltonian. This expansion is, in general, only possible for that part of the interaction referring to the coupling of matter to the radiation field, given by Eqn. (8.3). He then went on to contrast, and relate, the approaches of Schwinger and Feynman. The former is characterized by an operator which 'represents the interaction of a physical particle with an external field, including radiative corrections,' which may be expressed in terms of 'characteristic' repeated commutators:

$$H_T(x_0) = \sum_{n=0}^{\infty} \left(\frac{i}{\hbar c}\right)^n \int_{-\infty}^{\sigma(x_0)} dx_1 \int_{-\infty}^{\sigma(x_1)} dx_2 \cdots \int_{-\infty}^{\sigma(x_{n-1})} dx_n \\ \times [H^I(x_n), [\cdots, [H^I(x_2), [H^I(x_1), H^e(x_0)]] \cdots ]]. \tag{8.30}$$

Here $H^I$ is the interaction Hamiltonian (8.3) with a mass shift term removed,

$$H^I(x) = -\frac{1}{c} j_\mu(x) A_\mu(x) - \delta mc^2 \bar{\psi}(x)\psi(x), \tag{8.31}$$

and $H^e$ is the remaining part of the interaction Hamiltonian, for example, the interaction to the Coulomb field of the nucleus. In Dyson's perhaps critical words, 'The repeated commutators in this formula are characteristic of the Schwinger theory, and their evaluation gives rise to long and rather difficult analysis.'* (In a note added in proof, Dyson noted he had given an incorrect interpretation of Schwinger's formulation, and in fact Schwinger's approach, like Feynman's, was symmetric between past and future.) But Dyson's main point here was not an explication of Schwinger's methods, but of Feynman's.

Dyson's key innovation was the introduction of a time ordering operator $P$. 'If

$$F_1(x_1), \cdots, F_n(x_n) \tag{8.32}$$

are any operators defined, respectively, at the points $x_1, \ldots, x_n$ of space–time, then

$$P(F_1(x_1), \cdots, F_n(x_n)) \tag{8.33}$$

will denote the product of these operators, taken in the order, reading from right to left, in which the surfaces $\sigma(x_1), \ldots, \sigma(x_n)$ occur in time.' The Feynman theory was then seen to be given in terms of a time-ordered product of interaction

* To which Schwinger responded: 'Well, it wasn't so long and it wasn't so difficult, but nevertheless it was not the most economical way of going on to higher order effects. That I not only grant, but I insist on.... He did recognize that, as I think Feynman probably didn't, that the Feynman theory does operate with a statement about initial and final states, which is a concentration on the overall evolution of the system. And that was a useful thing. No question about it. And as soon as I understood that, I immediately incorporated it into my own next version as well.'[1]

operators:

$$H_F(x_0) = \sum_{n=0}^{\infty} \left(\frac{-i}{\hbar c}\right)^n \frac{1}{n!} \int_{-\infty}^{\infty} dx_1 \cdots \int_{-\infty}^{\infty} dx_n \times P(H^e(x_0), H^I(x_1), \cdots, H^I(x_n)). \tag{8.34}$$

Dyson went on to calculate matrix elements. In so doing, he used his time-ordering notation to define the 'Feynman' propagators for the photon and the electron:

$$\begin{aligned} \langle P(A_\mu(x), A_\nu(y))\rangle_0 &= \frac{1}{2}\hbar c \delta_{\mu\nu} D_F(x-y), \\ \langle P(\bar{\psi}_\alpha(x), \psi_\beta(y))\rangle_0 &= \frac{1}{2}\eta(x,y) S_{F\alpha\beta}(x-y), \end{aligned} \tag{8.35}$$

where $\eta(x, y)$ is $\pm 1$ depending on whether $\sigma(x)$ is later than or earlier than $\sigma(y)$, and the subscripts on the fermion fields are Dirac indices. In terms of these, Dyson was able to derive Feynman's graphical rules.

Note that in fact Dyson had made a major break with Feynman, who insisted on the particle nature of electrons, while Dyson, like Schwinger and Tomonaga, saw everything as fields. 'Nobody at Cornell understood that the electron field was a field like the Maxwell field. That was something that was in Wentzel[62] but was nowhere else. That was what was lacking in the old fashioned way of calculating. The electron was a particle, the photon was a field, and the two were just totally different. This notion of just two interacting fields with the simple interaction term $\bar{\psi}\gamma_\mu A^\mu \psi$ was essentially what I brought to Cornell with me from England out of Wentzel's book.'[62,2]

This paper appeared shortly after Dyson assumed his visiting fellowship at the Institute for Advanced Study, whose director was J. Robert Oppenheimer. Dyson was invited to present several seminars on this work. Oppenheimer, although initially expressing interest, was very hostile until Bethe intervened; then Oppenheimer capitulated and became a believer. But Dyson was not happy with him: 'Oppenheimer was a great disappointment. He hadn't time for the details. As compared to Hans Bethe, Oppenheimer was completely superficial. To talk to Oppenheimer was interesting. It was like meeting some very famous person who had interesting things to say but I just never got anything that you could really call guidance. I wasn't needing much guidance. . . . He had a bad effect on other people who needed the guidance more than I did.'[2] These remarks are not dissimilar to those of Schwinger concerning his interactions with Oppenheimer in Berkeley a decade earlier.

It was the second paper of Dyson, 'The $S$ matrix in quantum electrodynamics'[57] that assured his fame. In this paper he recast Schwinger's and

Feynman's electrodynamics into what has become the standard form. As Dyson stated in the introduction, 'The present paper deals with the relation between the Schwinger and Feynman theories when the restriction to one-electron problems is removed. In these more general circumstances, the two theories appear as complementary rather than identical. The Feynman method is essentially a set of rules for the calculation of elements of the Heisenberg $S$ matrix corresponding to any physical process, and can be applied with directness to all kinds of scattering problems. The Schwinger method evaluates radiative corrections by exhibiting them as extra terms appearing in the Schrödinger equation of a system of particles and is suited especially to bound-state problems. In spite of the difference of principle, the two methods in practice involve the calculation of closely related expressions; moreover, the theory underlying them is in all cases the same. The systematic technique of Feynman, the exposition of which occupied the second half of I[61] and occupies the major part of the present paper, is therefore now available for the evaluation not only of the $S$ matrix, but also of most of the operators occurring in the Schwinger theory.'

Dyson gave a systematic exposition of the perturbation theory of quantum electrodynamics. He did so by giving the so-called Schwinger–Dyson equations. These consisted of an infinite set of coupled integral equations for the Green's functions of the theory. For example, the full electron and photon propagators, $S'_F$, $D'_F$, satisfied by the equations

$$S'_F(p) = S_F(p) + S_F(p)\Sigma^*(p)S'_F(p), \tag{8.36}$$

$$D'_F(p) = D_F(p) + D_F(p)\Pi^*(p)D'_F(p), \tag{8.37}$$

where $\Sigma^*$ and $\Pi^*$ denoted the 'proper electron (photon) self-energy parts,' respectively. Although these equations are algebraic in momentum space, the self-energy parts are given by integral equations (which were not stated explicitly in Dyson's paper, but rather they were given by a graphical description). For example, vacuum polarization is in general given by*

$$\Pi_{\mu\nu}(q) = \mathrm{i}e^2 \int \frac{\mathrm{d}^4 k}{(2\pi)^4} \mathrm{Tr}\gamma_\mu S'_F(k)\Gamma_\nu(k, k+q)S'_F(k+q), \tag{8.38}$$

where $\Gamma_\nu(k, k+q)$ is a vertex amplitude coupling a vector potential $A_\nu(q)$, corresponding to a photon with momentum $q$ to incoming and outgoing electrons with momenta $k$ and $k+q$, respectively, which in turn is determined by still further integral equations. The perturbative solution to this system of

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* This equation appears explicitly in Schwinger's 1951 paper, 'On the Green's function of quantized fields' [66].

equations, where in lowest order $\Gamma_\nu = \gamma_\nu$, leads to Feynman's rules for the construction of all quantum mechanical amplitudes for computing scattering processes in QED.*

Dyson concluded his paper by discussing renormalization. He showed that if the propagators and the vertices were multiplied by certain constants,

$$\begin{aligned}\tilde{S}'_F(p) &= Z_2^{-1} S'_F(p),\\ \tilde{D}'_F(q) &= Z_3^{-1} D'_F(q),\\ \tilde{\Gamma}_\mu(p', p) &= Z_1 \Gamma_\mu(p', p),\end{aligned} \tag{8.39}$$

the (infinite) constants $Z_i$ could be so chosen as to cancel the divergences occurring in perturbation theory, and the resulting Green's functions of the theory were entirely finite. The finite renormalized charge $e$ was given in terms of the bare charge $e_0$ by $e = Z_3^{1/2} e_0$.

Dyson continued his contributions to field theory with a series of major papers published in 1951, dealing with what he called Heisenberg operators. This was somewhat in the spirit of Schwinger's canonical transformation designed to isolate the renormalization effects, but unlike Schwinger, Dyson did not use the adiabatic (slowly varying) approximation. He gave an exposition of this program at the Michigan Summer School in 1950. When the papers were published the following year[64] Dyson felt that he had made a major contribution that would 'get radiation theory moving forward again' and would allow the application of field-theory methods to meson problems. Unfortunately for Dyson, more effective methods rapidly became available, including Schwinger's Green's function techniques [66], so these papers had negligible impact at the time.[2,†]

A concluding paper published by Dyson in 1952 had significant repercussions on the view of the meaning of perturbation theory in quantum field theory.

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* Glauber recounted an embarrassing error that Schwinger made in this connection. In fall 1949 Schwinger gave a long sequence of lectures at the joint theoretical seminar hosted by Harvard and MIT on the Green's functions of quantum electrodynamics; in effect he claimed to have found a closed integral expression for the vertex function $\Gamma_\mu$. John Blatt took notes of these seminars, and they reached Norman Kroll at Columbia, who discovered a crucial error: the scattering of light by light had been inadvertently omitted. Shortly thereafter, Pauli visited Harvard from the Institute for Advanced Study, having heard of this error from Kroll, and visited Schwinger in his office. Sometime later, Schwinger emerged, 'badly shaken: Pauli was delighted to be the bearer of bad news.' Of course, in those early days, the structure of field theory was poorly glimpsed, so it is understandable that such an error could escape even the master.[63]

† These four papers of Dyson's had no citations in 1997 according to the *Science Citation Index*.[60]

This was 'Divergence of perturbation theory in quantum electrodynamics.'[65] There he gave a simple argument that perturbation theory could not result in a convergent series. The argument went as follows: suppose one computed a Green's function as a series in powers of $e^2$ (Apart from an overall factor, any Green's function has an expansion in powers of $e^2$ or $\alpha$.) If the series were convergent for sufficiently small values of $e^2$ it would have to converge even if $e^2$ were small but negative. But this cannot be, for if $e^2$ were negative like charges would attract, and the vacuum would be unstable to decay into an arbitrarily large number of electron–positron pairs. At best then, perturbation theory must result in an asymptotic series, which nowhere converges, but for which a finite number of terms gives an optimal approximation to the true Green's function. This is not an obstacle in practice for quantum electrodynamics, since the coupling constant, $\alpha = 1/137$, is so small. But the proof was discouraging to Dyson: 'That was, of course, a terrible blow to all my hopes. It really meant that this whole program [of perturbative quantum field theory] made no sense.'[2] Nowadays, no one is seriously disturbed about the asymptotic nature of perturbation theory, although it does raise the unresolved issue of the importance of non-perturbative effects in field theories, be they quantum electrodynamics or quantum chromodynamics (the theory of strong interactions). There is also the beginning of a recognition that Dyson's argument may be wrong, because it fails to take into account boundary conditions.[66]

### The impact of Dyson's work

The predominant view of the impact of Dyson's work was beautifully given by C.N. Yang. 'The papers of Tomonaga, Schwinger, and Feynman did not complete the renormalization program since they confined themselves to low-order calculations. It was Dyson who dared to face the problem of high orders and brought the program to completion. In two magnificently penetrating papers, he pointed out and resolved the main problems of this very difficult analysis. Renormalization is a program that converts additive subtractions into multiplicative renormalization. That it works required a highly non-trivial proof. That proof Dyson supplied. He defined the concepts of primitive divergences, skeleton graphs, and overlapping divergences. Using these concepts, he pushed through an incisive analysis and completed the proof of renormalizability of quantum electrodynamics. His perception and power were dazzling.'[67]

But the inventors of renormalized quantum electrodynamics were less impressed. In a later interview, Schwinger expressed his view of the contributions of Dyson to quantum electrodynamics. He began by paraphrasing Feynman: ' "Of course, neither you nor I needed to be told that our theories were equivalent and we didn't need Dyson." And, of course, that was true. Dyson was writing not for us, but for the rest of the world. What Dyson contributed was . . . the utility of a formal construction of that unitary operator in terms of time-ordering. There is the point that Dyson recognized that Feynman throughout was always dealing with scattering problems, that his theory in principle was incapable of dealing with bound states. Dyson recognized that I had a more complete theory. It was a Hamiltonian theory; you could deal with energy eigenvalues and so forth. Dyson did contribute something in his recognition of the importance of the time-ordering formulation. And that is what underlay the particular propagation function that Feynman and, as we know, Stückelberg before him, had introduced. From a practical point of view, I think he was simply translating his understanding of what Feynman was trying to do—and it's not clear that Feynman would necessarily have agreed with all that—into the ordinary language of operators and so forth. And pointing out that the different handling of the operators would produce the Feynman result. Valuable. Not world-shaking, but valuable.'[1]

In fact, as we have noted, Schwinger reacted positively to Dyson's introduction of time-ordering, recognizing its superiority: 'If you look at my own work you will see not time-ordering, but a concern with symmetrical and antisymmetrical products. Therefore, two functions. Whereas the complex time-ordered [propagation] function ultimately turns out to be the more convenient thing.'[1] In Schwinger's view it was fortunate that Dyson's paper[61] was published before Feynman's.[54] 'Feynman's paper published by itself would probably never have communicated very well. Dyson recognized what quantum-mechanical formulation Feynman was implicitly using, which was very valuable because nobody else could possibly have understood it without that recognition.' Dyson's papers were useful 'as one of the gospels, the interpretation of the mystical words to the masses.'[1] But Schwinger was unhappy at the success of the Feynman–Dyson approach: 'I confess it utterly astonished me that his method became so popular. That, of course, was not Feynman's doing but Dyson's. Without Dyson using my language to translate Feynman it never would have been understood.'[1]

Feynman, perhaps, had more cause for unhappiness, because Dyson's papers appeared before his. For a while, people even talked about 'Dyson graphs.' But Feynman was not too concerned. In later remarks, he commented, 'He wasn't trying to steal anything from me; he hadn't claimed they were his. All he was trying to do was tell everyone that there was something good in my theory, that he had discovered the connection with the work of Tomonaga and Schwinger, and that all these different approaches were equivalent. This greatly helped people to understand the different theories. His paper had some crazy language which I couldn't understand, but others could understand it. It was like a translation of my theory, my language, for other people; of course, it's a mistake to translate something for the author. I was bothered only slightly, and

I would be more concerned today if they were still called "Dyson graphs". That would not make me miserable, but I would complain a little bit about it.

'A little later, the diagrams came to be called "Dyson–Feynman graphs", with some others calling them the "Feynman graphs" through a number of people who knew about their origin a little better. Now, of course, it is as it should be. "We write down *the* diagram for this or that process." And that's the best, because it's anonymous, its *the* diagram. It makes me feel better than the "Feynman diagram", because it is *the* rule for something, and that's just fine.'[59]

## Feynman and Schwinger—cross-fertilization

Although Schwinger and Feynman never collaborated, and talked together rather rarely, it is clear that there was a certain synergism between these two innovators who nearly simultaneously scaled the peak of electrodynamics.

They had of course rather different goals: Schwinger was interested in understanding the experimental situation. 'I was concentrating on understanding these electromagnetic phenomena. I developed a formalism adequate enough to account for it, period. Feynman had something more grandiose in mind from the very beginning, a reconstruction of quantum mechanics using more intuitive ideas, and these same electromagnetic problems were for him simply a way of understanding what he was trying to do. These particular problems were not the center of his interest as they were for me. They were just another bit of experimental data in order to evolve his ideas. So Feynman was aiming at a more general method to begin with, but he could not have gotten there without the concrete answers, shall I say, that I provided and which he could then adapt and on the basis of which put forward his more general method. I don't know quite how to say it except that his aim was ultimately more far-reaching, but he needed—we were complementing each other. We were not in competition. Our ambitions were different. I got to these answers very quickly, which rather contradicts the general opinion that I used very complicated incomprehensible methods. They went fast and I don't ascribe it to any particular talents that I have. The machinery was perfectly okay for the purpose that it was being invented for. Whereas Feynman was looking for something more general.'[1]

Feynman indeed influenced Schwinger to find a better method to work out higher-order effects. 'Let's face it, the method that I had got clumsier and clumsier. Any method does at higher order. Perhaps a little more rapidly, which is why, when I finally realized what Feynman was trying to do, I took a look at it and went back and found a more general method myself. Which is perfectly reasonable. I'm emphasizing the point that what I did was more than adequate for the limited questions being asked, it explained the Lamb shift and the magnetic moment to the accuracy at which they were then measured. When the accuracy increased, the theory had to go to higher orders. Then came the question of which way of formulating it was most efficient and something along Feynman's line was no question [more efficient] and I adapted myself to it' albeit with a differential rather than an integral attitude.[1] In the year 1948–49 'I was certainly deeply involved in trying to look for more general formulations and seeing what there was in the Feynman–Dyson things that I should have to adopt, to find a synthesis. These were clearly not so different paths, but variations on each other. [The question was] what ultimately was the best version. I spent a lot of time on that. Particularly looking at all kinds of higher-order effects, for example, the two-particle differential equation, which became known as the Bethe–Salpeter equation, which I was talking about a year earlier and describing in lectures at Harvard. That was certainly the future, not the past.'[1]

Schwinger expressed regret that his interactions with Feynman had not been stronger. 'We were kind of moving in similar directions. It's too bad we couldn't have interacted earlier. We could have saved the world a lot of time. If he had gone to Columbia, we would have worked together at a much earlier stage. The reformulation of quantum mechanics might have occurred earlier and then that would have vastly simplified the application to electrodynamics.'[1]

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# 9

# Green's functions and the dynamical action principle

## The Greening of quantum field theory

In a remarkable lecture Schwinger delivered at the University of Nottingham on 14 July 1993, on the occasion of his receiving an honorary degree, entitled 'The Greening of quantum field theory: George and I' [229], he summarized the central role Green's function played throughout his career. Since this was his last public speech, and it provides a quick summary of Schwinger's career, we quote from it extensively. He began by a statement in favor of maturity:

'The young theoretical physicists of a generation or two earlier subscribed to the belief that: If you haven't done something important by age 30, you never will. Obviously, they were unfamiliar with the history of George Green, the Miller of Nottingham.

'Born, as we all know, exactly two centuries ago, he received, from the age of 8, only a few terms of formal education. Thus, he was self-educated in mathematics and physics, when in 1828, at age 35, he published, by subscription, his first and most important work: *An Essay on the Applications of Mathematical Analysis to the Theory of Electricity and Magnetism.* The Essay was dedicated to a noble patron of the "Sciences and Literature," the Duke of Newcastle. Green sent his own copy to the Duke. I do not know if it was acknowledged. Indeed, as Albert Einstein is cited as effectively saying, during his 1930 visit to Nottingham, Green, in writing the Essay, was years ahead of his time.

'There are those who cannot accept that someone, of modest social status and limited formal education, could produce formidable feats of intellect. There is the familiar example of William Shakespeare of Stratford upon Avon. It took almost a century and a half to surface, and yet another century to strongly promote, the idea that Will of Stratford could not possibly be the source of the plays and the sonnets which had to have been written by Francis Bacon. Or was it the Earl of Rutland? Or perhaps it was William, the sixth Earl of Derby? The most recent pretender is Edward deVir, seventeenth Earl of Oxford, notwithstanding the fact that he had been dead for 12 years when Will was put to rest.

'I have always been surprised that no one has suggested an analogous conspiracy to explain the remarkable mathematical feats of the Miller of Nottingham. So I invented one. "Descended from one of the lines of the Earl of Nottingham was the branch of the Earls of Effindham, which was separated from the Howards in 1731. The fourth holder of the title died in 1816, with apparently no claimant. In that year, George Green, age 23, could well have reached the maturity that led, 12 years later, to the publication of the Essay. And what of the remarkable fact that, in the same year that the earldom was revived, 1837, George Green graduated fourth wrangler at Cambridge University?" The conspiracy at which I hint darkly is one in which I believe quite as much as I think Edward deVir is the real Shakespeare.

'I consider myself to be largely self-educated. A major source of information came from my family's possession of the *Encyclopaedia Brittanica*, Eleventh Edition. I recently became curious to know what I might have, and probably did, learn about George Green, some 65 years before. There is no article detailing the life of George Green. There are, however, 4 brief references that indicate the wide range of Green's interests.

'First, in the article "Electricity," as a footnote to the description of Lord Kelvin's work, is this: "In this connexion the work of George Green (1793–1841) must not be forgotten. Green's *Essay on the application of mathematical analysis to the theories of electricity and magnetism*, published in 1828, contains the first exposition of the theory of potential. An important theorem contained in it is known as Green's theorem, and is of great value." It was, of course, Lord Kelvin (or rather William Thomson) who rescued Green's work from total obscurity.

'Then, in the article "Hydromechanics," after several applications of Green's transformation, which is to say, the theorem, there appears, under the heading *The Motion of a Solid through a Liquid*: "The ellipsoid was the shape first worked out, by George Green, in his *Research on the vibration of a pendulum in a fluid medium* (1833)."

'On to the article "Light" under the heading *Mechanical Models of the Electromagnetic Medium*. After some negative remarks about Fresnel, one reads: "Thus, George Green, who was the first to apply the theory of elasticity in an unobjectional manner. . . . " This is the content of *On the Laws of Reflexion and Refraction of Light* (1837).

'Finally, the paper *On the Propagation of Light in Crystallized Media* (1839) appears in the *Brittanica* article "Wave" as follows: "The theory of waves diverging from a center in an unlimited crystaline medium has been investigated with a view to optical theory by G. Green." The word "propagation" is a signal to us that, in little more than 10 years, George Green had significantly widened his physical framework. From the static three-dimensional Green's function that appears in potential theory, he had arrived at the concept of a dynamical, four-dimensional Green's function. It would be invaluable a century later.'

Schwinger then went on to recount his experience at the Radiation Laboratory during World War II. 'To continue the saga of George Green and me—my next step was to trace the influences of George Green on my own works. Here I spent no time over ancient documents. I went directly to a known source: THE WAR.

'I presume that in Britain, unlike the United States, *the war* has a unique connotation. Apart from a brief sojourn in Chicago, to see if I wanted to help develop The Bomb—I didn't—I spent the war years helping to develop microwave radar. In the earlier hands of the British, that activity, famous for its role in winning the Battle of Britain, had begun with electromagnetic radio waves of high frequency, to be followed by very high frequency, which led to very high frequency, indeed.

'Through those years in Cambridge (Massachusetts, that is), I gave a series of lectures on microwave propagation. A small percentage of them is preserved in a slim volume entitled *Discontinuities in Waveguides*. The word *propagation* will have alerted you to the presence of George Green. Indeed, on pages 10 and 18 of an introduction there are applications of two different forms of Green's identity. Then, on the first page of Chapter 1, there is Green's function, symbolized by *G*. In the subsequent 138 pages the references to Green in name or symbol are more than 200 in number.

'As the war in Europe was winding down, the experts in high power microwaves began to think of those electric fields as potential electron accelerators. I took a hand in that and devised the microtron which relies on the properties of relativistic energy. I have never seen one, but I have been told that it works. More important and more familiar is the synchrotron.

'Here I was mainly interested in the properties of the radiation emitted by an accelerated relativistic electron. I used the four-dimensionally invariant proper time formulation of action. It included the electromagnetic self-action of the charge, which is to say that it employed a four-dimensionally covariant Green's function. I was only interested in the resistive part, describing the flow of energy from the mechanical system into radiation, but I could not help noticing that the mechanical mass had an invariant electromagnetic mass added to it, thereby producing the physical mass of an electron. I had always been told that such a union was not possible. The simple lesson? To arrive at covariant results, use a covariant formulation, and maintain covariance throughout.'

After the war, quantum electrodynamics was the outstanding challenge: 'Quantum field theory, or more precisely, quantum electrodynamics, was forced from childhood into adolescence by the experimental results announced at

Shelter Island early in June 1947. The relativistic theory of the electron created by Dirac in 1928 was wrong. Not very wrong, but measurably so.

'A few days later, I left on a honeymoon tour across the United States. Not until September did I begin to work on the obvious hypothesis that electrodynamic effects were responsible for the experimental deviations, one on the magnetic moment of the electron, the other on the energy spectrum of the hydrogen atom.

'Although a covariant method was in order, I felt I could make up time with the then more familiar non-covariant methods of the day. By the end of November I had the results. The predicted shift in magnetic moment agreed with experiment. As for the energy shift in hydrogen, one ran into an expected problem.

'Consider the electromagnetic momentum associated with a charge moving at constant speed. The ratio of that momentum to the speed is a mass—an electromagnetic mass. It differs from the electromagnetic mass inferred from the electromagnetic energy. Analogously, the magnetic dipole moment inferred for an electron moving in an electric field is wrong. Replacing it by the correct dipole moment leads to an energy level displacement that was correct in 1947, and remains correct today at that level of accuracy as governed by the fine structure constant.

'I described all this at the January 1948 meeting of the American Physical Society, after which Richard Feynman stood up and announced that he had a relativistic method. Well, so did I, but I also had the numbers. Indeed, several months later, at the opening of the Pocono Conference, he ran over to me, shook my hand, and said "Congratulations, Professor! You got it right," which left me somewhat bewildered. It turned out he had completed his own calculation of the additional magnetic moment. Later we compared notes and found much in common.

'Unfortunately, one of the things we shared was an incorrect treatment of low energy photons. Nothing fundamental was involved; it was a matter of technique in making a transition between two different gauges. [We discussed this problem of Schwinger's and Feynman's mistake in their relativistic Lamb shift calculations in Chapters 7 and 8.] But, as in American politics these days, the less important the subject, the louder the noise. When that lapse was set right, the result of 1947 was regained. Incidentally, even Lord Rayleigh once made a mistake. That's one reason for its being called the Rayleigh–Jeans law.'

Now we come to the subject of this chapter. 'To keep to the main thrust of the talk—the evolution of Green's function in the quantum-mechanical realm—I move on to 1950, and a paper entitled *On Gauge Invariance and Vacuum Polarization* [64]. This paper makes extensive use of Green's functions, in a proper-time context, to deal with a variety of problems: non-linearities of the electromagnetic field, the photon decay of a neutral meson, and a short, but not the shortest derivation of the additional electron magnetic moment. The latter ends with the remark that "The concepts employed here will be discussed at length in later publications." I cannot believe I wrote that.' This paper will be discussed in pp. 307–315.

'The first, rather brief, discussion of those concepts appeared in a pair of 1951 papers, entitled *On the Green's Functions of Quantized Fields* [66]. One would not be wrong to trace the origin of today's lecture back 42 years to these brief notes. This is how paper I begins: "The temporal development of quantized fields, in its particle aspect, is described by propagation functions, or Green's functions. The construction of these functions for coupled fields is usually considered from the viewpoint of perturbation theory. Although the latter may be resorted to for detailed calculations, it is desirable to avoid founding the formal theory of the Green's functions on the restricted basis provided by the assumption of expandability in powers of the coupling constants. These notes are a preliminary account of a general theory of Green's functions, in which the defining property is taken to be the representation of the fields of prescribed sources."

'"We employ a quantum dynamical principle for fields which has been described in the 1951 paper entitled *The Theory of Quantized Fields* [65]. This (action) principle is a differential characterization of the function that produces a transformation from eigenvalues of a complete set of commuting operators on one space-like surface to eigenvalues of another set on a different surface."

'"In one example of a rigorous formulation, Green's function, for an electron–positron, obeys an inhomogeneous Dirac differential equation with an electromagnetic vector potential that is supplemented by a functional derivative with respect to the photon source; and, the vector potential obeys a differential equation in which the photon source is supplemented by a vectorial part of the electron–positron Green's function." (It looks better than it sounds.) "It is remarked that, in addition to such one-particle Green's functions, one can also have multiparticle Green's functions."

'The second note begins with: "In all the work of the preceding note there has been no explicit reference to the particular states on (the space-like surfaces) that enter the definitions of the Green's functions. This information must be contained in boundary conditions that supplement the differential equations. We shall determine these boundary conditions for the Green's functions associated with vacuum states on both (surfaces)."

'And then: "We thus encounter Green's functions that obey the temporal analog of the boundary condition characteristic of a source radiating into space. In keeping with this analogy, such Green's functions can be derived from a retarded proper time Green's function by a Fourier decomposition with respect to the mass."

'The text continues with the introduction of auxiliary quantities: the mass operator $M$ that gives a non-local extension to the electron mass; a somewhat analogous photon polarization operator $P$; and $\Gamma$, the non-local extension of the coupling between the electromagnetic field and the fields of the charged particles. Then, in the context of two-particle Green's functions, there is the interaction operator $I$. "The various operators that enter in the Green's function equations $M$, $P$, $\Gamma$, $I$, can be constructed by successive approximation. Perturbation theory, as applied in this manner, must not be confused with the expansion of the Green's functions in powers of the charge. The latter procedure is restricted to the treatment of scattering problems."

'Then one reads: "It is necessary to recognize, however, that the mass operator, for example, can be largely represented in its effect by an alteration in the mass constant and by a scale change of the Green's function. Similarly, the major effect of the polarization operator is to multiply the photon Green's function by a factor, which everywhere appears associated with the charge. It is only after these renormalizations have been performed that we deal with wave equations that involve the empirical mass and charge, and are thus of immediate physical applicability." (These papers will be discussed on pp. 323–325.)

'In the period 1951–1952, two colleagues of mine at Harvard [Robert Karplus and Abraham Klein], and I, wrote a series of papers under the title *Electrodynamic Displacements of Atomic Energy Levels* [68, 70]. The third paper, which does not carry my name, is subtitled *The Hyperfine Structure of Positronium.*[1] I quote a few lines: "The discussion of the bound states of the electron–positron system is based upon a rigorous functional differential equation for the Green's function of that system." And, "Theory and experiment are in agreement."' (These papers will be briefly discussed on pp. 328–329.)

'As for the rest of the 50's, I focus on two highlights. First: although it could have appeared any time after 1951, it was 1958 when I published *The Euclidean Structure of Relativistic Field Theory* [86]. Here is how it begins: "The nature of physical experience is largely conditioned by the topology of space–time, with its indefinite Lorentz metric. It is somewhat remarkable, then, to find that a detailed correspondence can be established between relativistic quantum field theory and a mathematical image based on a four-dimensional Euclidean manifold. The objects that convey this correspondence are the Green's functions of quantum field theory, which contain all possible physical information. The Green's functions can be defined as vacuum-state expectation values of time-ordered field products." I well recall the reception this received, running the gamut from "It's wrong" to "It's trivial." It is neither.' We will discuss Euclidean field theory, and the short-lived controversy surrounding its introduction, in Chapter 11.

'Second (highlight): Another Harvard colleague [Paul Martin] and I had spent quite some time evolving the techniques before we published a 1959 paper entitled *Theory of Many-Particle Systems* [89]. It was intended to bring the full power of quantum field theory to bear on the problems encountered in solid state physics, for example. That required the extension of vacuum Green's functions, which refer to absolute zero temperature, into those for finite temperature. This is accomplished by a change of boundary conditions, which become statements of periodicity, or anti-periodicity, for the respective BE or FD statistics, in response to an imaginary time displacement.' This important development will be discussed in pp. 329–334.

'As an off shoot of this paper, I published in 1960, *Field Theory of Unstable Particles* [94]. Here is how it begins: "Some attention has been directed recently to the field-theoretic description of unstable particles. Since this question is conceived as a basic problem for field theory, the responses have been some special device or definition, which need not do justice to the physical situation. If, however, one regards the description of unstable particles to be fully contained in the framework of the general theory of Green's function, it is only necessary to emphasize the relevant structure of these functions. That is the purpose of this note. What is essentially the same question, the propagation of excitations in many-particle systems where stable or long-lived "particles" can occur under exceptional circumstances, has already been discussed along these lines."' We discuss this paper in Chapter 11.

'One might be forgiven for assuming that this saga of George and me effectively ended with this paper. But that was 1/3 century ago!'

Schwinger then went on to discuss his development of source theory in the late 1960s, the Casimir effect, which he treated in the mid-1970s, and sonoluminescence with which he was very actively involved in the last few years of his life. We will come to these topics in later chapters. Schwinger's point again was that Green's function techniques played a crucial role in all his research.

Schwinger concluded: 'So ends our rapid journey through 200 years. What, finally, shall we say about George Green? Why, that he is, in a manner of speaking, alive, well, and living among us.'

Although this essay tells us rather little about George Green, it tell us a great deal about Schwinger's work over a fifty-year span. The purpose of the present chapter is to flesh out much of this story in the 1950s. But first, we need a recreational break.

## The first trip to Europe

Schwinger first traveled abroad in 1949. Earlier opportunities had presented themselves: we recall that he had been offered a fellowship to go to Holland, to Leiden, to work with Hendrik Kramers in 1938. Later, as he was becoming famous, Schwinger had been invited to attend the eighth Solvay conference in Brussels in 1948, but apparently the invitation did not arrive in time. He recalled, 'But obviously I was eager to go because when an invitation came in 1949 to go to the joint meeting of the Italian and Swiss Physical Societies at Basle and Como I accepted like a shot.'[2]

In May 1949 Schwinger received the Charles L. Mayer Nature of Light Award, awarded by the National Academy of Sciences. Feynman was on the committee, so it was clear he was ineligible, and Schwinger figured correctly that he was a shoo-in. He wrote an unpublished paper on quantum electrodynamics,* 'mostly concerned with scattering and Brownian states. In fact that paper contains an introduction to what later got called the Furry representation,[4] which is just simply an interaction representation in which the binding energy of a Coulomb potential is already included. It is the natural way of treating bound state problems.'[2] Weisskopf told the Schwingers that the award money ($2000) would more than pay their way in Europe, because it was so cheap to live there.[5] However, he did not appreciate the style in which they would travel. (In fact, Schwinger's view of the prize was that 'It ain't the money, it's the principle of the thing,' the title of a song composed by Arthur Roberts to celebrate Rabi's Nobel Prize in 1944. [197])

The first part of the summer they spent at Brookhaven, on Long Island, where Schwinger started working on his third version of quantum electrodynamics. Then they headed abroad for the meeting, which took place in September. They travelled to France on the *Coronia*, disembarking at Le Havre on lighters, because the harbor was still bombed out, followed by the boat train to Paris. When they arrived in Paris, they were met by their friend Steve White. 'He was the science reporter for the *Herald Tribune*. He had moved to Hearst and he was awaiting us and so when we got off the boat train there he was to take us by hand to the nearest bar. This must have been at noon. We drank champagne.'[2] In Paris they stayed at the Hôtel de la Californie, on Rue de Berre, opposite the offices of the *Herald Tribune*. They had a room next to the patio, and were awakened by a waiter setting the table on the patio singing 'La Vie en Rose.'[5]

The Schwingers first went to Zurich. They met Rabi in Paris, 'and Rabi said, "Pauli wants to see you," or "Pauli is angry at you. We must go to Pauli to set things right. We must go to Zurich." We went—Clarice and I, separately—I remember an occasion on which I found out where Pauli was. I picked up a phone, which I rarely do, called the ETH, asked for Pauli in German, which I don't really speak, or certainly didn't then. Anyway, we went to Pauli and

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* In fact, Schwinger's submission consisted of three papers, 'Quantum electrodynamics. I' [50], 'II' [52], and 'III,' the first two being completed by the deadline of 1 October 1948, and the third an uncompleted manuscript, rather different, apparently, from the final paper [57], which was only completed in May 1949. The incomplete manuscript indeed contained the 'Furry representation' [197]. See also Schweber.[3]

everything was sweetness and light. Pauli set me down and said, so why did you do this, why did you do that, and so forth. He went through the papers and said, "But this is wrong," or "I don't believe this." It's all in the past, I don't do it that way anymore. I was no way interested in defending earlier versions of quantum electrodynamics. For me, it had gone through at least three levels of development. I was only interested in one at that point.'[2] 'This refusal to be a stationary target left Pauli exasperated' [197]. At the time Pauli was working on the famous Pauli–Villars regularization.[6] 'They must have confronted me with it. I have no memory, but I must have said I didn't like it. But it was very amicable.'[2]

Clarice recalled meeting Franca, Pauli's wife. Even though they were very different, they became fast friends. They took the Schwingers to a restaurant in the mountains where Julian had his first *Pêche Melba*. He found it the most perfect dessert; Clarice recalled sending Oppenheimer a postcard which said, "*Pêche Melba* plus Julian equals bliss." Clarice also got along very well with Pauli and became very fond of him.[5]

The first part of the joint Swiss–Italian meeting was at Basle, near Zurich. Schwinger recalled a picnic at the time. Steve White 'had a car and was driving us. Everybody went out to a picnic at Ausflug. Only we wanted to be independent. It was very amusing. We were going up in the hills, the mountains somewhere, but instead of following, we followed in reverse. We were ahead of the bus, so we were always trying to anticipate which way the bus would turn. But it turned out that the Swiss Army was practicing artillery and we could hear the guns and so forth, and some of the areas we were heading toward or trying to go through were barred off. So we ended up wandering all over the Swiss mountains. It was great fun.'[2]

Schwinger had a second acoustical memory in Basle. 'They had to give us some sort of party and so we were all in a little underground cellar and a number of Swiss came in, a Swiss drum corps came in, and this tiny place with low ceilings was absolutely reverberating with noise.'[2]

The second part of the meeting was at Como. 'At Lake Como is the Hotel Villa d'Esta, which is luxury personified. So we stayed there.'[2]

'I paid no attention to the meetings in Como except I had to give a lecture, which of course was on quantum electrodynamics, and Dyson was there. One scene I remember about this lecture is while I gave this lecture, somebody asked me a question, maybe it had something to do with Feynman, so I turned to Dyson and said, "Dyson, do you know?" He wasn't paying any attention to me, and I had to yell, "Dyson!" And he said, "What? What?" I don't know what he was thinking about.' The lecture was the only time Schwinger attended the meeting. 'The rest was just spent wandering and looking at all the sights. Incidentally, at the time we were at the Villa d'Esta there was a *Concours d'élégance* of automobiles which undoubtedly had a lot to do with my later fascination with Italian cars.'[2]

After the meeting, they traveled on their own to Florence. There, they ran out of money. They stayed at the Hotel Excelsior where they had an enormous room, which had been a ballroom with a salon.[5] This used up the last of their prize money. They had to use their last money to telephone Clarice's mother back home to telegraph funds.[2] After that they never took Weisskopf's advice because they simply did not do things the same way, although their style of traveling did become more modest over the years.[5]

After Florence, the Schwingers visited Nice, and then had to return to Harvard, where classes had already started. They returned on the *Queen Mary*.

On this trip, or perhaps the following summer, the Schwingers encountered Werner Heisenberg at dinner in Pisa. Harold Levine and the Schwingers were sitting at the table facing the doorway; the room was filled with physicists having dinner. A man appeared at the door and nobody looked at him; he seemed to stand there an eternity. Clarice asked who he was—it was Heisenberg. Then, without thinking, Clarice went up to him and asked him to join them for dinner. Clarice could understand why Heisenberg, who had stayed and worked in Germany through the war, was ostracized, but felt a human bond toward the man.[5]

Returning home to Cambridge, they settled into a comfortable routine. However, their life was not without its tragic elements. They had, of course, planned to raise a family; both Clarice and Julian would undoubtedly have been exceptional parents. However, Clarice had several miscarriages, and eventually they had to give up their dream of children. It was the great disappointment of their lives.[7,*]

## Gauge invariance and vacuum polarization

The paper 'On gauge invariance and vacuum polarization' [64], submitted by Schwinger to the *Physical Review* near the end of December 1950, is nearly universally acclaimed as his greatest publication. As his lectures have rightfully been compared to the works of Mozart, so this might be compared to a mighty construction of Beethoven, the 3rd Symphony, the *Eroica*, perhaps. It is most remarkable because it stands in splendid isolation. It was written over a year after the last of his series of papers on his second, covariant, formulation of

* Some years later, in 1957, one of Schwinger's students, David Lynch, was expecting his first child; he saw an ad for a baby carriage, and when his wife called, she identified herself as the wife of a physicist. The woman who answered said, that's interesting, my husband is in physics, too. It was Clarice Schwinger. Of course, in the students' eyes, 'Schwinger wasn't in physics, he was physics.'[8]

quantum electrodynamics was completed: 'Quantum electrodynamics III. The electromagnetic properties of the electron—radiative corrections to scattering' [57] was submitted in May 1949. And barely two months later, in March 1951, Schwinger would submit the first of the series on his third reformulation of quantum field theory, that was based on the quantum action principle, namely, 'The theory of quantized fields I' [65]. But 'Gauge invariance and vacuum polarization' stands on its own, and has endued the rapid changes in tastes and developments in quantum field theory, while the papers in the other series are mostly of historical interest now. As Lowell Brown[9] pointed out, 'Gauge invariance and vacuum polarization' still has over 100 citations per year, and is far and away Schwinger's most cited paper.*

Yet even such a masterpiece was not without its critics. Abraham Klein, who was finishing his thesis at the time under Schwinger's direction, and would go on to become one of Schwinger's second set of 'assistants' (with Robert Karplus), as, first, an instructor, and then a Junior Fellow, recalled that Schwinger (and, independently, he and Karplus) ran afoul of a temporary editor at the *Physical Review.* That editor thought Schwinger's original paper repeated too many complicated expressions and that symbols should be introduced to represent expressions that appeared more than once. Schwinger complied, but had his assistants do the dirty work. Harold Levine, who was still sharing Schwinger's office, working on the waveguide book, typed the revised manuscript, while Klein wrote in the many equations. Klein recalled that he took much more care in writing those equations than he did in his own papers.[11]

Schwinger recalled later that he viewed this paper, in part, as a reaction to the 'invariant regularization' of Pauli and Villars.[6] 'It was this paper, with its mathematical manipulation, without physical insight particularly about questions such as photon mass and so forth, which was the direct inspiration for "Gauge invariance and vacuum polarization." The whole point is that if you have a propagation function, it has a certain singularity when the two points coincide. Suppose you pretend that there are several particles of the same type with different masses and with coupling constants which can suddenly become negative instead of positive. Then, of course, you can cancel them. It's cancellation again, subtraction physics, done in a more sophisticated way, but still, things must be made to add up to zero. Who needs it?'[2]

An extended synopsis of this work, of course, cannot do justice to its beauty, elegance, and power. Yet just as books are written about Beethoven's symphonies, we will attempt such a survey. The title of the paper is apt, as the first two sentences of the abstract indicate: 'This paper is based on the elementary

* In the 1997 *Science Citation Index*, it had 105 citations out of a total of 500 citations of all of Schwinger's work.[10]

remark that the extraction of gauge invariant results from a formally gauge invariant theory is ensured if one employs methods of solution that involve only gauge covariant quantities. We illustrate this statement in connection with the problem of vacuum polarization by a prescribed electromagnetic field' [64]. The primary methodology is the use of a proper-time formalism, which on the one hand is nothing other than the exploitation of the Euler representation of the gamma function, but which then allows one to commute dynamical variables by solving proper-time equations of motion, and remains one of the most powerful techniques in quantum field theory.

After adopting the gauge-invariant philosophy, Schwinger regarded 'the rest as just technique. I go through the solution of certain problems, some of which turned out to be of some importance in later developments. It is a *tour de force*, let's face it, because it is not easy to find a technique to deal with the electromagnetic field in which the vector potential never enters. The vector potential never appears, there's no gauge ambiguity. I got a great deal of fun out of this.'[2]

Schwinger started by describing the motion of an electron, of charge $e$ and mass $m$, in a prescribed electromagnetic field, $A_\mu$, which motion, of course, is given by the 'second-quantized' Dirac equation,

$$\gamma_\mu(-\mathrm{i}\partial_\mu - eA_\mu(x))\psi(x) + m\psi(x) = 0, \tag{9.1}$$

where the electron field $\psi$ and its adjoint satisfy the equal-time anticommutation relation

$$\{\psi(\mathrm{x}, x_0), \bar{\psi}(\mathrm{x}', x_0)\} = \gamma_0\delta(\mathrm{x} - \mathrm{x}'), \tag{9.2}$$

and $\gamma^\mu$ are the Dirac matrices which satisfy

$$\{\gamma_\mu, \gamma_\nu\} = -2\delta_{\mu\nu}, \tag{9.3}$$

in terms of the metric tensor $\delta_{\mu\nu}$, whose non-zero, diagonal matrix elements are all unity. The electron Green's function is defined in terms of the vacuum expectation value of the time-ordered product of Dirac fields,

$$G(x, x') = \mathrm{i}\langle(\psi(x)\bar{\psi}(x'))_+\rangle\epsilon(x - x'), \tag{9.4}$$

where for arbitrary fields the time ordering is defined by

$$(A(x_0)B(x_0'))_+ = \begin{cases} A(x_0)B(x_0'), & x_0 > x_0', \\ B(x_0')A(x_0), & x_0 < x_0', \end{cases} \tag{9.5}$$

and the $\epsilon$ symbol changes the sign depending upon the ordering:

$$\epsilon(x - x') = \begin{cases} 1, & x_0 > x_0', \\ -1, & x_0 < x_0'. \end{cases} \tag{9.6}$$

Now Schwinger wrote, 'It is useful to regard $G(x, x')$ as the matrix element of an operator $G$, in which states are labeled by space–time coordinates as well as by the suppressed spinor indices:

$$G(x, x') = (x|G|x'). \tag{9.7}$$

The defining differential equation for the Green's function is then considered to be a matrix element of the operator equation

$$(\gamma\Pi + m)G = 1, \tag{9.8}$$

where [the gauge-covariant momentum operator]

$$\Pi_\mu = p_\mu - eA_\mu \tag{9.9}$$

is characterized by the operator properties

$$[x_\mu, \Pi_\nu] = \mathrm{i}\delta_{\mu\nu}, \quad [\Pi_\mu, \Pi_\nu] = \mathrm{i}eF_{\mu\nu}, \tag{9.10}$$

and

$$F_{\mu\nu} = \partial_\mu A_\nu - \partial_\nu A_\mu \tag{9.11}$$

is the antisymmetrical field strength tensor.'

The proper-time integral appeared on the third page, when Schwinger writes the Green's operator as

$$G = \frac{1}{\gamma\Pi + m} = \mathrm{i}\int_0^\infty \mathrm{d}s \, \exp\{-\mathrm{i}(\gamma\Pi + m)s\}. \tag{9.12}$$

The vacuum expectation value of the current vector, which he expressed in terms of the Dirac matrix trace of the diagonal elements of the Green's function,

$$\langle j_\mu(x)\rangle = \mathrm{i}e \,\mathrm{tr}\, \gamma_\mu (x|G|x), \tag{9.13}$$

could then be obtained by variation of a certain action integral with respect to $A_\mu$. That action corresponds to the Lagrange function

$$\mathcal{L}^{(1)}(x) = \mathrm{i}\int_0^\infty \frac{\mathrm{d}s}{s} \mathrm{e}^{\mathrm{i}ms} \mathrm{tr}(x|\exp\{-\mathrm{i}\gamma\Pi s\}|x). \tag{9.14}$$

So both the Green's function $G(x, x')$ and the effective Lagrange function $\mathcal{L}^{(1)}$ reduce to the evaluation of the matrix element of a proper-time evolution operator,

$$(x'|U(s)|x'') = (x(s)'|x(0)''), \tag{9.15}$$

where

$$U(s) = \mathrm{e}^{-\mathrm{i}\mathcal{H}s}, \quad \mathcal{H} = -(\gamma\Pi)^2 = \Pi^2 - \frac{1}{2}e\sigma_{\mu\nu}F_{\mu\nu}, \tag{9.16}$$

and $\sigma_{\mu\nu} = (\mathrm{i}/2)[\gamma_\mu, \gamma_\nu]$ are the generalization of the Pauli spin matrices. The interpretation of $\mathcal{H}$ is that it is a 'Hamiltonian' that evolves the system in proper time, so that the matrix element of $U(s)$ is the transformation function from a state in which $x_\mu(s=0)$ has the value $x''_\mu$ to a state in which $x_\mu(s)$ has the value $x'_\mu$. This gives an immediate particle interpretation, with equations of motion

$$\frac{dx_\mu}{ds} = -i[x_\mu, \mathcal{H}] = 2\Pi_\mu, \tag{9.17}$$

$$\frac{d\Pi_\mu}{ds} = -i[\Pi_\mu, \mathcal{H}] = e(F_{\mu\nu}\Pi_\nu + \Pi_\nu F_{\mu\nu}) + \frac{1}{2}e\sigma_{\lambda\nu}\frac{\partial F_{\lambda\nu}}{\partial x_\mu}. \tag{9.18}$$

Schwinger then proceeded to solve these equations in three cases. First, he considered the elementary case in which $F_{\mu\nu} = 0$. Of course, this did not mean that the vector potential $A_\mu$ vanishes, but only that it be a pure gauge. In that case he found, for example, the following representation for the Green's function:

$$G(x', x'') = \frac{\Phi(x', x'')}{(4\pi)^2}\int_0^\infty ds\, s^{-2}e^{-im^2 s}\left(-\gamma\frac{(x'-x'')}{2s} + m\right) \times e^{i(x'-x'')^2/4s}. \tag{9.19}$$

Here $\Phi$ involves a line integral of the vector potential,

$$\Phi(x', x'') = \exp\left[ie\int_{x''}^{x'} dx_\mu\, A_\mu(x)\right]. \tag{9.20}$$

This then was independent of the path, because the potential has zero curl.

The third section of the paper is its heart. There Schwinger considered the case of constant field strengths. This is an exactly solvable problem because it is equivalent to a harmonic oscillator system. The equations of motion, in matrix form, are simply

$$\frac{dx}{ds} = 2\Pi, \quad \frac{d\Pi}{ds} = 2eF\Pi, \tag{9.21}$$

so because $F$ was constant, they could be immediately integrated:

$$\Pi(s) = e^{2eFs}\Pi(0), \tag{9.22}$$

$$x(s) - x(0) = \left[\frac{e^{2eFs} - 1}{eF}\right]\Pi(0). \tag{9.23}$$

Schwinger now proceeded inexorably, and in a page and a half of calculation obtained a proper-time representation for the Lagrange function $\mathcal{L}^{(1)}$:

$$\mathcal{L}^{(1)} = -\frac{1}{8\pi^2}\int_0^\infty ds\, s^{-3}e^{-m^2 s}\left[(es)^2\mathcal{G}\frac{\text{Re}\cosh esX}{\text{Im}\cosh esX} - 1\right], \tag{9.24}$$

where

$$\mathcal{G} = \frac{1}{4} F_{\mu\nu} F^*_{\mu\nu} = \mathbf{E} \cdot \mathbf{H}, \tag{9.25}$$

where $F^*$ represents the dual field strength,

$$F^*_{\mu\nu} = \frac{1}{2} \epsilon_{\mu\nu\lambda\kappa} F_{\lambda\kappa}. \tag{9.26}$$

(A duality transformation interchanges electric and magnetic quantities: $F \to F^*$, or $\mathbf{E} \to \mathbf{B}$, $\mathbf{B} \to -\mathbf{E}$.) Similarly,

$$\mathcal{F} = \frac{1}{4} F^2_{\mu\nu} = \frac{1}{2}(\mathrm{H}^2 - \mathrm{E}^2), \tag{9.27}$$

and then

$$X = \sqrt{2(\mathcal{F} + \mathrm{i}\mathcal{G})}. \tag{9.28}$$

Equation (9.24) is ultraviolet divergent, because the integrand is singular at $s = 0$. Renormalization is required. If we expand the integrand in powers of the field strength, we see that the first, divergent term is proportional to $F^2$, and thus amounts to a rescaling of the fields, and a corresponding renormalizing of the charge. Therefore that term should be simply omitted, and Schwinger was left with a finite, gauge-invariant Lagrangian function, exact in the field strength, and of second order in the fine structure constant:

$$\mathcal{L} = -\mathcal{F} - \frac{1}{8\pi^2} \int_0^\infty \mathrm{d}s\, s^{-3} \mathrm{e}^{-m^2 s} \times \left[ (es)^2 \mathcal{G} \frac{\mathrm{Re} \cosh esX}{\mathrm{Im} \cosh esX} - 1 - \frac{2}{3}(es)^2 \mathcal{F} \right] \tag{9.29}$$

$$= \frac{1}{2}(\mathrm{E}^2 - \mathrm{H}^2) + \frac{2\alpha^2}{45} \frac{(\hbar/mc)^3}{mc^2} \times [(\mathrm{E}^2 - \mathrm{H}^2)^2 + 7(\mathbf{E} \cdot \mathbf{H})^2] + O(F^6). \tag{9.30}$$

Here the coupling is written in terms of the fine structure constant, $\alpha = e^2/4\pi$. This is the famous Euler–Heisenberg Lagrangian.[12] This represents, for example, the scattering of light by light, which has never been observed directly (although experiments involving intense laser beams have been proposed), but indirectly it has been seen through its contribution to the anomalous magnetic moment of the electron.[13]

Schwinger concluded this section of the paper with a derivation of the effective Lagrangian for a spin-0 charged particle, which differs from the expansion given above in Eqn (9.30) by different numerical coefficients in front of the two Lorentz invariant structures, $\mathcal{F}^2$ and $\mathcal{G}^2$.

The Euler–Heisenberg Lagrangian represents the interaction of an arbitrary number of photons with a single electron 'loop.' From it can be deduced the cross section for the process $\gamma\gamma \to \gamma\gamma$; the total cross section for that process is (for example see Ref. 14)

$$\sigma = \frac{973}{10125\pi}\alpha^4\frac{\omega^6}{m^6}, \tag{9.31}$$

where $\omega$ is the photon frequency in the center-of-mass frame, which, since it was derived under the assumption that the fields are constant, is valid only for $\omega \ll m$. But in addition, it describes processes such as $\gamma\gamma \to 4\gamma$, as well as processes such as photon scattering in the presence of Coulomb fields, and the astrophysically important process of photon splitting in strong magnetic fields.

In the fourth section of the paper, Schwinger repeated the calculation of a third exactly solvable situation, that of a plane electromagnetic wave. However, in that case the invariants vanish,

$$\mathcal{F} = \mathcal{G} = 0, \tag{9.32}$$

so Schwinger concluded 'there are no nonlinear vacuum phenomena for a single plane wave, of arbitrary strength and spectral decomposition.'

The fifth section is quite remarkable. Here Schwinger considered the decay of scalar and pseudoscalar mesons into photons, a subject which had yielded some difficulty.[15] The problem was that the coupling of the pseudoscalar to a fermionic current* could be either through a pseudoscalar interaction,

$$g\phi(x)\tfrac{1}{2}[\bar{\psi}(x), \gamma_5\psi(x)], \tag{9.33}$$

or through an axial-vector interaction,

$$\frac{g}{2M}\partial_\mu\phi(x)\frac{1}{2i}[\bar{\psi}(x), \gamma_5\gamma_\mu\psi(x)], \tag{9.34}$$

where $M$ denoted the mass of the fermion and $g$ is the strength of the coupling. Formally, by use of the Dirac equation, these two interactions might be shown to be equivalent. But discrepancies occurred when it was attempted to compute the two photon decay of the pion (here we call the pseudoscalar by its modern name),

$$\pi \to \gamma\gamma, \tag{9.35}$$

where the two photons come from coupling to the fermionic vacuum expectation value or loop. In fact, however, Schwinger showed that if proper care was

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* At the time those fermions were thought to be protons, but the modern view is that they are quarks.

taken, the axial-vector interaction gave a result that agreed with the pseudoscalar interaction, given by the effective Lagrange function

$$\mathcal{L}' = \frac{\alpha}{\pi}\frac{g}{M}\phi \mathrm{E} \cdot \mathrm{H}. \tag{9.36}$$

This was an extremely important result, but unappreciated at the time. It was independently rediscovered, and dubbed the axial-vector anomaly, twenty years later by Bell, Jackiw, Adler, Johnson, and others.[16–18,*] Not only is it relevant for important physical processes such as pion decay, but similar anomalies occur in gauge theories, where, if they are not suitably cancelled, they will destroy the renormalizability, and hence the consistency of those theories.

The final section of the contained details another remarkable discovery, namely that a constant electric field can produce electron–positron pairs; hence, for example, the Coulomb field is unstable. This has an insignificant probability of occurring unless the electric fields are very strong, but such fields might occur in very heavy transuranic elements, and will be sought in heavy-ion accelerators. The probability, per unit time and unit volume, that a pair be created by a constant electric field $E$ is approximately given by

$$\frac{\alpha^2}{\pi}E^2 \sum_{n=1}^{\infty} n^2 \exp\left(-\frac{n\pi m^2}{e|\mathrm{E}|}\right). \tag{9.37}$$

One last *tour de force* concludes the paper. In a one-page Appendix, using these proper-time methods, Schwinger gives what for the time was the shortest known derivation of the anomalous magnetic moment of the electron,

$$\mu' = \frac{\alpha}{2\pi}\frac{e\hbar}{2mc}. \tag{9.38}$$

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* A brief history was given by one of the authors of the present volume in the pages of *Physics Today* a few years ago. A portion of that letter to the editor reads: Schwinger was 'the true discoverer of the axial-vector anomaly in its original context, the decay of the neutral pion into two photons. Julian Schwinger very explicitly in his classic paper "On Gauge Invariance and Vacuum Polarization" derived the anomaly by showing that pseudoscalar and pseudovector couplings are equivalent. Of course, the language was somewhat different in those days. This result had been apparently completely forgotten by the time of the work of Adler[17] and Bell and Jackiw,[16] but very shortly thereafter Jackiw and Johnson[18] recognized that "the first derivation of [the anomaly equation] for external electromagnetic fields was given by Schwinger." [Indeed, Adler in a Note Added in Proof to his paper[17] acknowledged Jackiw and Johnson's rediscovery of Schwinger's work.] These remarks are not at all meant to disparage in any way the significant contributions made by many people in 1968 and subsequently, but merely to remind us all in physics what a great debt we owe to Julian Schwinger.'[19]

As Schwinger later remarked, 'this is a very important paper, not for what it discusses, but for what it alludes to.'[2] Indeed, it continues to be the source of much new research, ranging from applications in astrophysics to searches for magnetic monopoles.

## The quantum action principle

Schwinger's wartime work on microwave radiation was largely based on the development of variational methods for computing the modes of microwave cavities and transmission lines. After the war, he immediately applied such techniques to nuclear physics [58, 60]. (Although these papers are dated 1950, they grew out of 1947 lectures of Schwinger.) It took somewhat longer for variational methods to take center stage in his work on quantum electrodynamics.

Recall that by 1950 he had successfully scaled the peak of quantum electrodynamics by two different routes (Feynman, on the other hand, had but one ascent.) The first approach, which was largely unpublished, led to his first calculation of the electron's anomalous magnetic moment (9.38) in 1947, reported in [43]. (Schweber published large extracts of his unpublished calculations, which were based on an ingenious method of successive canonical transformations.[3]) Schwinger abandoned this approach quickly, because it was not covariant, and therefore susceptible to serious errors. At the Washington APS meeting in April 1948 he announced the covariant approach [47], which was fleshed out in Quantum electrodynamics I, II, and III [50, 52, 57], submitted between the end of July 1948 and May 1949. It was these papers that sealed Schwinger's fame, and largely conclude Schweber's account of Schwinger's work.

But the best was yet to come. The monumental 'Gauge invariance and vacuum polarization' [64], described in the preceding section, was to be completed a year and a half later. And about the same time Schwinger saw how to obtain a quantum action principle, extending the stationary principles of mechanics of Lagrange and Hamilton, to the quantum domain. In this, as with Feynman, his point of departure was the famous paper of Dirac, 'On the Lagrangian in Quantum Mechanics,'[20] but the response was completely different: Feynman was to give a global 'solution' to the problem of determining the transformation function, the probability amplitude connecting the state of the system at one time to that of the system at a later time, in terms of a sum over classical trajectories, the famous path integral. Schwinger, instead, derived (initially postulated) a differential equation for that transformation function in terms of a quantum action functional. This differential equation possessed Feynman's path integral as a formal solution, which remained poorly defined; but Schwinger believed throughout his life that his approach was 'more general, more elegant, more useful, and more tied to the historical line of development as the quantum transcription of Hamilton's action principle.' [160]

As Schwinger stated later, 'The idea from the beginning was not, as Feynman would do, to write down the answer, but to continue in the grand tradition of classical mechanics, but only as a historical model, to find a differential, an action principle formulation. What is Hamilton's principle or its generalization in quantum physics? If you want the time transformation function, do not ask what it is but how it infinitesimally changes. The distinction [with the path integral approach] comes [because] this deals with all kinds of quantum variables, on exactly the same footing, which means from a field point of view not only do Bose–Einstein fields appear naturally but Fermi–Dirac fields. Whereas with the path integral approach with its clear connection to the correspondence principle, the anticommuting Fermi system appears out of nowhere, there is no logical reason to have it except that one knows one has to. It does not appear as a logical possibility as it does with the differential.'[2]

At the meeting on the history of particle physics held at Fermilab in May 1980, Schwinger elaborated on this point. 'This development must have begun in late 1949 or early 1950, to judge by a set of notes entitled "Quantum Theory of Fields, A New Formulation." They were taken by the now President of the California Institute of Technology, then known as Marvin Goldberger. Dated July 1950, they refer to a field theory course that was given in the semester between January and June. First for particles, and then for fields, the notes trace how the single quantum action principle leads to operator commutation relations, equations of motion, or field equations, and conservation laws. In the relativistic field context, the postulate of invariance under time reflection (remember, this is 1950) leads to two kinds of fields—two statistics—as a consequence of the more elementary analysis into two kinds of spin, integral and half-integral. This occurs because time reflection is not a canonical, a unitary, transformation, but also requires an inversion in the order of all products. That discloses the fundamental operator nature of the field, distinguishing essential commutativity from essential anticommutativity, as demanded by the spin character of the field. In a subsequent version [73] the existence of two kinds of fields with their characteristic operator properties is recognized at an earlier stage. Here also the non-Hermitian fields of charged particles are replaced by Hermitian fields of several components, facilitating the description of the internal degrees of freedom that would later proliferate. In this version, time reflection implies a transformation to the complex conjugate algebra, and the postulate of invariance predicts the type of spin to be associated with each statistic. An inspection of the proof shows that what is really used is the hypothesis of invariance under time and space reflection. That invariance and the spin-statistics connection are equivalent. But, with the later discovery of parity non-conservation, the common emphasis as embodied in the so-called *TCP* (or is it *PTC*) theorem, is to regard the spin-statistics relation as primary and the invariance under space-time reflection as a consequence.' [197]

To put these developments in context, we might quote from Schwinger's extended preface to his collection of the most fundamental papers on *Quantum electrodynamics* [83]: 'The evolutionary process by which relativistic field theory was escaping from the confines of its non-relativistic heritage culminated in a complete reconstruction of the foundations of quantum dynamics. The quantum mechanics of particles had been expressed as a set of operator prescriptions superimposed upon the structure of classical mechanics in Hamiltonian form. When extended to relativistic fields, this approach had the disadvantage of producing an unnecessarily great asymmetry between time and space, and of placing the existence of Fermi–Dirac fields on a purely empirical basis. But the Hamiltonian form is not the natural starting point of classical dynamics. Rather, this is supplied by Hamilton's action principle, and action is a relativistic invariant. Could quantum dynamics be developed independently from an action principle, which, being freed from the limitations of the correspondence principle, might automatically produce two distinct types of dynamical variables? The correspondence relation between classical action, and the quantum-mechanical description of time development by a transformation function, had long been known.[20] It had also been observed that, for infinitesimal time intervals and sufficiently simple systems, this asymptotic connection becomes sharpened into an identity of the phase of the transformation function with the classically evaluated action.[21] The general quantum dynamical principle was found in a differential characterization of transformation functions, involving the variation of an action operator [65]. When the action operator is chosen to produce first-order differential equations of motion, or field equations, it indeed predicts the existence of two types of dynamical variables, with operator properties described by commutators and anti-commutators, respectively [73]. Furthermore, the connection between the statistics and the spin of the particles is inferred from invariance requirements, which strengthens the previous arguments based upon properties of non-interacting particles.[22] The practical utility of this quantum dynamical principle stems from its very nature; it supplies differential equations for the construction of the transformation functions that contain all the dynamical properties of the system. It leads in particular to a concise expression of quantum electrodynamics in the form of coupled differential equations for electron and photon propagation functions [66]. Such functions enjoy the advantages of space–time pictorializability, combined with general applicability to bound systems or scattering situations. Among these applications has been a treatment of that most electrodynamic of systems—positronium, the metastable atom formed by a positron and an electron. The agreement between theory and experiment on the finer details of this system is another quantitative triumph of quantum electrodynamics.'[1]

It is revealing of Schwinger's view of the development of the subject that in his collection [83] he indeed puts these three papers in the indicated order: Dirac,[20] written in 1932, Feynman,[21] written in 1948, and Schwinger [65], written in 1951. Actually, as Schwinger notes at the beginning of his paper, his program was initiated in Summer 1949 at Brookhaven National Laboratory, and the paper was largely written there the following summer. Again to quote from the 1980 Fermilab lecture: 'My retreat began at Brookhaven National Laboratory in the summer of 1949. It is only human that my first action was one of reaction. Like the silicon chip of more recent years, the Feynman diagram was bringing computation to the masses. Yes, one can analyze experience into individual pieces of topology. But eventually one has to put it all together again. And then the piecemeal approach loses some of its attraction. Speaking technically, the summation of some infinite set of diagrams is better and more generally accomplished by solving an integral equation, and those integral equations usually have their origin in a differential equation. And so, the copious notes and scratches labeled "New Opus," that survive from the summer of 1949, are concerned with the compact, operator expression of classes of processes. And slowly, in these pages, the integral equations and the differential equations emerge. There is another collection of scraps which, at sometime in the past, I put into a folder labeled "New Theory—Old Version (1949–1950)," although I now believe that the reference to 1950 is erroneous—by then the New Theory in its later manifestation had arrived. There is a way to tell the difference. With the emphasis on the operator field description of realistic, interacting systems, the interaction representation had begun to lose its utility, and fields incorporating the full effects of interaction enter. The unpublished essay of the National Academy of Sciences competition had already taken a step in that direction. If fields of both types, with and without reference to interaction, appear in an equation, the historical period is that of the Old Version. The later version has no sign at all of the interaction representation. On one of these pages there is an Old Version, 1949, equation giving the first steps toward the relativistic equation for two interacting particles now known as the Bethe–Salpeter equation.[23] Accordingly, it is not surprising to read in a footnote of a 1951 paper,[24] presenting an operator derivation of the two-particle equation, that I had already discussed it in my Harvard lectures' [197]. (The file entitled 'New Opus 1949' and 'New Theory—Old Version (1949–50)', as well as 'Quantum Theory of Fields, A New Formulation,' class lecture notes transcribed by Marvin L. Goldberger, MIT, July 1950, containing very recent, unpublished work, may be found in the Schwinger archive at UCLA.[25])

Let us now sketch a description of that paper. As noted, the essential idea was to break away from correspondence-principle arguments and 'develop a self-contained quantum dynamical principle from which the equations of motion and the commutation relations could be deduced.' The introduction includes the words, 'Quantitative success has been achieved thus far only in the restricted domain of quantum electrodynamics. Furthermore, the existence of divergences, whether cancelled or explicit, serves to emphasize that the present quantum theory of fields must in some respects be incomplete. It is not our purpose to propose a solution of this basic problem, but rather to present a general theory of quantum field dynamics which unifies several independently developed procedures and which may provide a framework capable of admitting fundamentally new physical ideas.' As Schwinger remarked later, 'I was simply saying this was a synthesis of me, Feynman, Dyson, and so forth. It was going to be a unification in one systematic, self-contained framework, freed from the correspondence principle.'[2]

He began by introducing a complete set of eigenvectors 'specified by a space-like surface $\sigma$ and the eigenvalues $\zeta'$ of a complete set of commuting operators constructed from field quantities attached to that surface.' The question is how to compute the transformation function from one space-like surface to another, that is, $(\zeta_1', \sigma_1|\zeta_2'', \sigma_2)$. After remarking that this development, time-evolution, must be described by a unitary transformation, he *assumed* that any infinitesimal change in the transformation function must be given in terms of the infinitesimal change in a quantum action operator, $W_{12}$, or of a quantum Lagrange function $\mathcal{L}$. This is the quantum dynamical principle:

$$\begin{aligned}\delta(\zeta_1', \sigma_1|\zeta_2'', \sigma_2) &= \frac{i}{\hbar}(\zeta_1', \sigma_1|\delta W_{12}|\zeta_2'', \sigma_2) \\ &= \frac{i}{\hbar}(\zeta_1', \sigma_1|\delta \int_{\sigma_2}^{\sigma_1} (dx)\mathcal{L}|\zeta_2'', \sigma_2). \end{aligned} \tag{9.39}$$

Here, $\mathcal{L}$ is a relativistically invariant Hermitian function of the fields and their derivatives,

$$\mathcal{L} = \mathcal{L}(\phi^a(x), \partial_\mu\phi^a(x)), \tag{9.40}$$

where $a$ labels the different field operators of the system. If the parameters of the system are not altered, the only changes arise from those of the initial and final states, which changes are effected by infinitesimal generating operators $F(\sigma_1)$, $F(\sigma_2)$, expressed in terms of operators associated with the surfaces $\sigma_1$ and $\sigma_2$. In this way, Schwinger deduced the *Principle of Stationary Action*,

$$\delta W_{12} = F(\sigma_1) - F(\sigma_2), \tag{9.41}$$

from which the field equations follow.

Schwinger went on to deduce the stress tensor operator, and to exploit the relation between symmetries, which leave the action invariant, $\delta W_{12} = 0$, and conservation laws. Thus, translation invariance is associated with the conservation of energy–momentum, or in terms of the stress (or energy–momentum) tensor, $T_{\mu\nu}$,

$$\partial_\mu T_{\mu\nu} = 0, \tag{9.42}$$

and rotational invariance leads to the conservation of the angular momentum tensor $M_{\lambda\mu\nu} = x_\mu T_{\lambda\nu} - x_\nu T_{\lambda\mu}$,

$$\partial_\lambda M_{\lambda\mu\nu} = 0. \tag{9.43}$$

The latter requires the symmetry of the stress tensor, $T_{\mu\nu} = T_{\nu\mu}$. The conservation of charge is discussed similarly.

Schwinger was also able to derive the commutation relations for the fields from his dynamical principle. In terms of the unit time-like normal to the surface $\sigma$, $n_\mu$, the canonical momentum variable conjugate to the field $\phi^a$ is $\Pi^a = n_\mu \partial\mathcal{L}/\partial_\mu\phi^a$. Schwinger was able to demonstrate, by considering changes in the field operators induced by the generators due to field variations, that

$$\begin{aligned}
[\phi^a(x), \Pi^b(x')]_\pm &= i\hbar\delta_{ab}\delta_\sigma(x - x'), \\
[\phi^a(x), \phi^b(x')]_\pm &= [\Pi^a(x), \Pi^b(x')]_\pm = 0,
\end{aligned} \tag{9.44}$$

for fermions and bosons respectively. Here it is assumed that the two field operators considered lie on the same space-like surface, and the delta function is a three-dimensional delta function on that surface. The $\pm$ subscripts denote anticommutators or commutators, respectively. As we will discuss in more detail in Chapter 11, this was the first proof of the spin-statistics theorem for an interacting system, established by Pauli[22] only for non-interacting fields. 'In the requirement that commutators be employed, for components of an integral spin field, and anti-commutators for components of a half-integral spin field, we have the connection between the spin and statistics of particles.'

Schwinger concluded the second section of this paper with a connection to Hamilton–Jacobi theory, and to the work of Feynman. For a Bose–Einstein system he could define an ordered non-Hermitian operator $\mathcal{W} \neq W_{12}$ so that the differential equation for the transformation function relating eigenstates of the field operator can be integrated:*

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* Schwinger later remarked, 'It was my great mistake never to have solved more elementary problems, so that people could see [the action principle] in actual operation.'[2] He went on to refer to footnote 6 of this paper, in which $\mathcal{W}$ and $W_{12}$ are given explicitly for a one-dimensional free particle.

$$(\phi', \sigma_1|\phi'', \sigma_2) = \exp\left[\frac{\mathrm{i}}{\hbar}\mathcal{W}(\phi', \sigma_1; \phi'', \sigma_2)\right]. \tag{9.45}$$

In a footnote, Schwinger noted 'The exponential form of Eqn (9.45) is familiar as a basis for establishing a correspondence with classical Hamiltonian–Jacobi particle mechanics. Dirac employed this form in a discussion of unitary transformations and recognized, in part, that the Hamiltonian–Jacobi equations are rigorous as relations among ordered operators.[26] In Feynman's version of quantum mechanics,[21] the exponential form is employed for infinitesimal time intervals, with the real part of $\mathcal{W}$ defined as the classical action integral.'

The final section of this first paper was devoted to more details of the spin-statistics connection. Invariance under time reflection was the key assumption. At that time, time reflection and space reflection invariance were unquestioned. Only some five years later was it suggested theoretically, and experimentally confirmed, that parity, space-reflection invariance, was violated in weak interactions. Then, it was recognized by Schwinger that what he had established in 1951 was a proof of what is now called the *TCP* theorem. We will trace this later perspective in Chapter 11.

After a lapse of nearly two years, in February 1953 Schwinger submitted 'The theory of quantized fields. II' [73]. The paper begins with a reformulation of his dynamical principle. In particular, he considered invariance under the proper orthochronous Lorentz group, which preserves the temporal order of the space-like surfaces which define the action operator

$$W_{12} = \int_{\sigma_2}^{\sigma_1} (\mathrm{d}x)\mathcal{L}(x). \tag{9.46}$$

The general field is decomposed into two sets, Bose–Einstein and Fermi–Dirac, which satisfy commutation and anticommutation relations respectively. Because Schwinger was able to establish the connection between time reflection (with the fall of parity, generalized to the *TCP* symmetry) and the spin-statistics relation (the revised argument is given in Chapter 11) that relation is established.

Schwinger then turned to the electromagnetic field, coupled to charged fields. This was done by noting that the Lagrange function describing the charged particle represented by a Hermitian field $\chi$ is invariant under a constant phase transformation

$$\chi \to \chi\mathrm{e}^{\mathrm{i}\lambda\mathcal{E}}, \tag{9.47}$$

where $\lambda$ is a constant, and '$\mathcal{E}$ is an imaginary matrix which can be viewed as a rotation matrix referring to a space other than the four-dimensional world.' (In later years, Schwinger would denote $\mathcal{E}$ by $q$, a $2 \times 2$ matrix identical to the second Pauli matrix.) However, if $\lambda$ is not constant, $\lambda \rightarrow \lambda(x)$, the Lagrangian is not invariant,

$$\mathcal{L} \rightarrow \mathcal{L} + j_\mu \partial_\mu \lambda. \tag{9.48}$$

If the kinetic part of the charged particle Lagrange function is written in terms of a matrix $\mathcal{A}_\mu$ as

$$\mathcal{L}_k = \frac{1}{2}(\chi \mathcal{A}_\mu \partial_\mu \chi - \partial_\mu \chi \mathcal{A}_\mu \chi), \tag{9.49}$$

which is a generalization of the Lagrange function for a Dirac field, the current density appearing in the non-invariance statement (9.48) is

$$j_\mu = -\mathrm{i}\chi \mathcal{A}_\mu \mathcal{E} \chi. \tag{9.50}$$

But invariance of the action under such a local gauge transformation can be achieved by 'the addition of the electromagnetic field Lagrange function,'

$$\mathcal{L}_{\text{emf}} = \frac{1}{2}\{j_\mu, A_\mu\} - \frac{1}{4}\{F_{\mu\nu}, \partial_\mu A_\nu - \partial_\nu A_\mu\} + \frac{1}{4}F_{\mu\nu}^2 + J_\mu A_\mu, \tag{9.51}$$

(here the braces denote an anticommutator, i.e., symmetric ordering) provides a compensating quantity through the associated gauge transformation,

$$A_\mu \rightarrow A_\mu - \partial_\mu \lambda. \tag{9.52}$$

This is now the usual route for introducing gauge coupling into a theory. The requirement of local gauge symmetry determines the interaction between the gauge field and the particle field, here given by the first term in Eqn (9.51).

Schwinger then showed that the electromagnetic and charged fields are not kinematically independent. This is done by establishing a relation between the commutators of the field strengths with each other, and with the charged-particle currents. Only in the approximation of neglecting the interaction between currents and fields in timelike relation may one derive a simple relation for the field strength commutator:

$$\begin{aligned}\mathrm{i}[F_{\mu\nu}, F_{\lambda\kappa}] = (&\delta_{\nu\lambda}\partial_\mu\partial_\kappa - \delta_{\nu\kappa}\partial_\mu\partial_\lambda - \delta_{\mu\lambda}\partial_\nu\partial_\kappa \\ &+ \delta_{\mu\kappa}\partial_\nu\partial_\lambda)D(x - x'),\end{aligned} \tag{9.53}$$

(the four terms here just reflect the symmetry of the field strength tensor, $F_{\mu\nu} = -F_{\nu\mu}$) where $D$ is the difference between the retarded and advanced photon Green's functions, or propagation functions.

Schwinger concluded the paper by considering a special gauge, the radiation gauge, $\nabla \cdot \mathbf{A} = 0$. In this gauge, the independent dynamical variables were the vector potential, $A_k$ and the transverse electric field, $F_{0k}^{(\mathrm{T})} = -\partial_0 A_k$, for the following commutation relations held:

$$\mathrm{i}[A_k(x), F_{0l}^{(\mathrm{T})}(x')] = \left(\delta_{kl}\delta_\sigma(x - x')\right)^{(\mathrm{T})} \tag{9.54}$$

$$[A_k(x), A_l(x')] = [F_{0k}^{(\mathrm{T})}(x), F_{0l}^{(\mathrm{T})}(x')] = 0, \tag{9.55}$$

where the (T) superscript denoted transverse, that is, orthogonal to $\nabla$. Thus, a canonical formulation of quantum electrodynamics had been given.

But nearly two years earlier, on 22 May 1951, shortly after the first 'quantized field' paper was submitted, Schwinger had communicated two relatively brief, but extremely important papers to the National Academy, 'On the Green's functions of quantized fields. I' and 'II' [66]. These papers are just what they say. In the first he obtained a functional differential equation for the electron Green's function, defined by Eqn (9.4),

$$\left[\gamma_\mu\left(-\mathrm{i}\partial_\mu - e\langle A_\mu(x)\rangle + \mathrm{i}e\frac{\delta}{\delta J_\mu(x)}\right) + m\right] G(x, x') = \delta(x - x'), \tag{9.56}$$

where $J_\mu(x)$ is an external photon source. Using Eqn (9.13) he derived an equation for $\langle A_\mu\rangle$ in the Lorentz gauge $\partial_\nu\langle A_\nu(x)\rangle = 0$, which is

$$-\partial^2\langle A_\mu(x)\rangle = J_\mu(x) + \mathrm{i}e\,\mathrm{tr}\,\gamma_\mu G(x, x). \tag{9.57}$$

'The simultaneous equations (9.56) and (9.57) provide a rigorous description of $G(x, x')$ and $\langle A_\mu(x)\rangle$.' Schwinger later characterized this paper as having 'the first exact formulation of electrodynamics in terms of coupled differential equations.'[2]

Schwinger similarly obtained a differential equation for the photon Green's function. Most interesting, however is the two-particle Green's function, defined by

$$G(x_1, x_2; x_1', x_2') = \langle(\psi(x_1)\psi(x_2)\bar{\psi}(x_1')\bar{\psi}(x_2'))_+\rangle\epsilon, \tag{9.58}$$

where

$$\epsilon = \epsilon(x_1, x_2)\epsilon(x_1', x_2')\epsilon(x_1, x_1')\epsilon(x_1, x_2')\epsilon(x_2, x_1')\epsilon(x_2, x_2'), \tag{9.59}$$

where $\epsilon(x, y)$ is given by Eqn (9.6), which is antisymmetrical with respect to the interchange of $x_1$ and $x_2$ and of $x_1'$ and $x_2'$. If $\mathcal{F}$ denotes the Dirac operator containing functional derivatives appearing in Eqn (9.56), the two-particle Green's function satisfies the equation

$$\mathcal{F}_1\mathcal{F}_2 G(x_1, x_2; x_1', x_2') = \delta(x_1 - x_1')\delta(x_2 - x_2') - \delta(x_1 - x_2')\delta(x_2 - x_1'). \tag{9.60}$$

This is the famous Bethe–Salpeter equation, which was actually first published by Nambu.[27] In fact, Gell-Mann and Low's[24] and Salpeter and Bethe's[23] contributions appeared after this paper of Schwinger's. Only his students recognized his priority: for example, Abraham Klein wrote, 'I believe I saw a derivation of the so-called Bethe–Salpeter equation in a lecture by Julian before I ever read the famous paper.'[28] Richard Arnowitt, who was Schwinger's student from 1949–52, concurred. 'Julian did many things he was not given credit for, for example the Bethe–Salpeter equation. He worked it out through variational derivative techniques, which is the only way to derive it.' He went on to say that the Gell-Mann–Low derivation[24] is not convincing because you cannot organize the diagrams without knowing the answer.[29,*]

The second paper of the same title, which was received simultaneously by the *Proceedings of the National Academy of Sciences*, and followed immediately upon the first, elaborated the first by considering boundary conditions. That it was really a continuation is evidenced by the fact that the equations were numbered sequentially throughout the two papers. Schwinger considered outgoing wave boundary conditions, denoted by a + subscript, so that the one-particle Green's function was symbolically written as

$$[\gamma(p - eA_+) + M]G_+ = 1, \tag{9.61}$$

where space–time coordinates were regarded as matrix indices. The mass operator appearing here was defined by (cf. Eqn (9.56))

$$MG_+ = mG_+ + ie\gamma\frac{\partial}{\partial J}G_+, \tag{9.62}$$

The two-particle Green's function satisfied the integro-differential equation

$$[(\gamma\pi + M)_1(\gamma\pi + M)_2 - I_{12}]G_{12} = 1_{12}, \tag{9.63}$$

where the gauge-covariant momentum operator appeared as $\pi = p - eA$, and where the matrix element of $1_{12}$ was given by the right-hand side of Eqn (9.60). The interaction operator $I_{12}$ was given as an integral equation involving

* In fact, Gell-Mann and Low acknowledged Schwinger's priority: 'We are indebted to Drs. Bethe and Salpeter for communicating their results to us prior to publication. We understand that this equation has been treated by Schwinger in his lectures at Harvard.'[24]

photon and electron Green's functions. It was most transparent in the first approximation, where it corresponded to single photon exchange between the two electrons:

$$I(x_1, x_2; x_1', x_2') = -ie^2\gamma_{1\mu}\gamma_{2\mu}D_+(x_1, x_2)(x_1, x_2|1_{12}|x_1', x_2'). \qquad (9.64)$$

Schwinger concluded the set of two papers* with the words: 'It is necessary to recognize, however, that the mass operator, for example, can be largely represented in its effect by an alteration in the mass constant and by a scale change of the Green's function. Similarly, the major effect of the polarization operator is to multiply the photon Green's function by a factor, which everywhere appears associated with the charge. It is only after these renormalizations have been performed that we deal with equations that involve the empirical mass and charge, and are thus of immediate physical applicability.'

## The Einstein Prize

In 1951 Schwinger was awarded the first Einstein Prize. 'I shared the prize with the well-known mathematician [Kurt] Gödel. As I have the story, Einstein would have been perfectly happy to have all the prize given to Gödel, who was his friend, but I think Oppenheimer persuaded him to share it.'[2] This monetary award was about $5000. 'I have a memory of Oppenheimer telling me about it, perhaps before, and saying—I'm not sure what the basis was—but he said something to the effect that this should settle all of your monetary cares from now on. If so, he lived in a very different world.'[2]

At the award ceremony in Princeton, Schwinger 'met Einstein for the very first time. This was the first time I'd ever met Einstein in the sense of shaking his hand, but I think I reported that it was pretty clear to me that he really had no idea what I was doing there and had no interest in what I had done and, I suspect, I said something about it being clear that our meeting was too late, that we might have interacted usefully, but by this time his interests were too polarized, or something of that sort.'[2] Unfortunately, this 'got reported that Julian said that he was too old. It got reported badly. Unkind and unfair.'[5]

## Quantized fields continue

The third and fourth papers in the series 'The theory of quantized fields' were included in Schwinger's *Selected papers*.[30] As we recall, that collection was personally selected by Schwinger himself, and he prefaced the volume with a pithy comment as to the reason for each choice of inclusion. In the case of 'The theory

* A third paper in the 'Green's functions of quantized fields' series was started, but never brought to completion.[25]

of quantized fields. III' [74] the reason was for its 'systematic use for Bose systems of states based on non-Hermitian operators (later popularized as coherent states).' Many years later, Schwinger remained upset that his contribution had not been recognized: 'My mistake was not to have given the simple mechanical example of oscillators, [instead of going] directly to the electromagnetic field. This way looks very obscure, but it's identical to the oscillator technique.'[2] We will describe Schwinger's seminal work on coherent states, and Roy Glauber's later reincarnation of the idea,[31] in more detail in Chapter 10, in connection with Schwinger's reformulation of quantum kinematics.

Here we will simply note that this third paper, submitted just a month after the second one in the series, dealt with the electromagnetic field under the influence of a prescribed current. He introduced creation and annihilation operators for photons associated with a given space-like surface $\sigma$, which he called $a_{\lambda k}^{(\pm)}(\sigma)$, respectively. The state with photon occupation numbers $n_{\lambda k}$, where $\lambda$ specified the polarization and $k$ the momentum of the photon, was created from the vacuum state $\Psi_0$ by application of the creation operators,

$$\Psi(n\sigma) = \left(\prod_{\lambda k} \frac{(a_{\lambda k}^{(+)}(\sigma))^{n_{\lambda k}}}{(n_{\lambda k}!)^{1/2}}\right)\Psi_0. \tag{9.65}$$

But more useful for the theoretical development than the eigenstates of the number operator were eigenstates of the creation and annihilation operators, or, equivalently, the positive and negative frequency parts of the transverse electric field, denoted by Schwinger as $F_{0k}^{(\pm)}(x)$. Denoting the eigenvalue for the former by $F_{0k}^{(+)\prime}$, the corresponding non-Hermitian eigenvalue equation became

$$F_{0k}^{(+)}(x)\Psi(F^{(+)\prime}\sigma) = F_{0k}^{(+)\prime}(x)\Psi(F^{(+)\prime}\sigma). \tag{9.66}$$

These eigenstates were the so-called coherent states, which were related to the number eigenstates by the transformation function

$$(n|F^{(+)\prime}) = \prod_{\lambda k} \frac{(a_{\lambda k}^{(+)\prime})^{n_{\lambda k}}}{(n_{\lambda k}!)^{1/2}}, \tag{9.67}$$

where $a^{(+)\prime}$ is the eigenvalue of the creation operator $a^{(+)}$ in Eqn (9.65). Schwinger went on to treat the perturbed electromagnetic problem masterfully using this new non-Hermitian basis.

Paper IV [76] in the series was received by the *Physical Review* on 6 August 1953.* Here, Schwinger's characterization was 'Systematic use for Fermi systems of totally anticommutative number system (Grassmann algebra); Green's

* It was preceded by a short note in the *Philosophical Magazine* [75] pointing out that the quantum action principle applies equally well to systems described by first-order equations, contrary to an earlier claim in that same journal.[25]

functions for multiparticle states.'[30] The abstract begins: 'The principal development in this paper is the extension of the eigenvalue–eigenvector concept to complete sets of anticommuting operators. With the aid of this formalism we construct a transformation function for the Dirac field, as perturbed by an external source.' From the results in the non-Hermitian basis, again expressions for the occupation number representation of matrix elements may be extracted, which is accomplished for both Dirac and Maxwell matrices. Years later, Schwinger recalled the reaction to the introduction of these ideas. 'The eigenvalues of anticommuting fields are totally anticommuting numbers. I remember the reaction to this when I first used it somewhere: "unbelievable" or "grotesque." When you do the natural thing and it's unfamiliar, people don't want to get it.'[2]

The following paper, V [77], received toward the end of October 1953, continued this development of the Dirac field, but now not perturbed by a second prescribed Dirac field, but by an external Maxwell field. Provided that field vanished on the boundary surfaces, 'apart from the modification of the Green's function, the transformation function differs in form from that of the field-free case only by the occurrence of a field dependent numerical factor, which is expressed as an infinite determinant.' Perhaps the most interesting development in this paper was the expression of the scattering matrix in terms of forward time development for positive frequency modes, but backwards time development for negative frequency modes. Hence the scattering matrix was expressed in terms of an operator that 'connects an "initial" state specified by incoming fields with a "final" state specified by outgoing field.' This appeared to be a presentiment of the 'time-cycle' formalism that Schwinger would present in detail several years later in 'Brownian motion of a quantum oscillator' [101], which we will discuss in the following chapter.

The final 'quantized field' paper, number VI, [80] was submitted in January 1954. The emphasis was on determinantal methods, again for a Dirac field perturbed by an external electromagnetic field. This paper explicitly dealt with scattering, using Green's functions in the presence of a time-independent electromagnetic field. This was where the Furry representation,[4] which Schwinger had first introduced in his essay for the Nature of Light Award in 1949, was first published by Schwinger.[2] The paper concluded with a discussion of scattering, in the high energy limit, by an isotopic scalar potential. Characteristically, Schwinger began a seventh member of the sequence of papers, but abandoned the project.[25]

Paul Martin has provided some perspective on the comparative merits of Schwinger's second and third approaches to quantum electrodynamics. In his view, the 'quantum electrodynamics' series I–III did not give a general picture—and thus was less general than Feynman's approach. But the 'quantized field'

Green's function approach was far more general than Feynman's. 'Julian's way of treating field theory in general, in appreciating its generality, its nonperturbative character, were really developed by him in the 1950s, and are only now beginning to be appreciated by other people.'[32]

## Electrodynamic displacements of energy levels

As we recall, there were two critical tests of quantum electrodynamics. The first was Schwinger's successful calculation of the anomalous magnetic moment of the electron [43]. The second was the Lamb shift, the displacement of the $2S_{1/2}$ level of hydrogen from the $2P_{1/2}$, which in the Dirac theory are degenerate. We have recounted the story of the experimental discovery of this splitting of levels, and the theoretical resolution, in earlier chapters. The nonrelativistic theory was immediately successful, but more difficulty occurred in the relativistic calculation, with both Feynman and Schwinger making an error in matching low-energy and high-energy photons.

By the early 1950s, experimental technique had improved, and it was necessary for the calculations to be carried out to higher order. As Klein noted, in the period 1950–55, Harvard 'was the center of application of QED to bound-state problems.'[11] Robert Karplus and Abraham Klein, as Schwinger's assistants, collaborated with Schwinger on two papers on the subject, entitled 'Electrodynamic displacements of atomic energy levels' [68, 70]; the first was a Letter to the Editor; the second, subtitled 'Lamb shift' was received by *Physical Review* on 26 December 1951. As Schwinger remarked, Karplus and Klein went on to write a third paper on their own,[1] which was included with appreciation in Schwinger's collection [83]. Schwinger could certainly have legitimately put his name on this paper as well, but 'Julian was extremely generous in not taking credit.'[11,*]

Let us describe 'Electrodynamic displacement of atomic energy levels. II. Lamb shift' [70]. (In fact, the precursor to this paper is not the brief letter [68], but a paper of Karplus and Klein alone.[33]) Using a mass operator technique, based in part on 'Gauge invariance and vacuum polarization' [64], Karplus,

* There seem to have been some anomalous exceptions. Before Karplus first came to work as Schwinger's assistant, he brought a calculation to Schwinger. 'Julian made the calculation much more beautiful, but then tried to freeze Karplus out of the paper.' Only the intervention of Walter Kohn made Karplus stand up for his rights.[11] Also, at a later period, Bruno Zumino frequently joined Schwinger for lunch, and at some point it was claimed that Schwinger took one of Zumino's ideas and published it without suggesting to Zumino that he be a co-author. But ideas are slippery things, and Julian undoubtedly had a different perspective on these incidents. In any case, in view of the enormous size of the crowd of Schwinger's students and collaborators, these few exceptions prove the rule.

Klein, and Schwinger first rederived the first-order Lamb shift, which apart from the 'Bethe logarithm' was of order $Z^4\alpha^3$.[34] Here $Z$ is the atomic number, so $Ze$ is the charge of the nucleus. They then went on to calculate the correction, a displacement of the $nS$ level of

$$\frac{Z^5\alpha^4}{n^3}\left(1+\frac{11}{28}-\frac{1}{2}\ln 2+\frac{5}{192}\right)\text{Ry}, \tag{9.68}$$

where the last term corresponds to vacuum polarization. Altogether, for the hydrogen 2$S$ level this amounts to a 7.14 MHz displacement. (This formula included a small numerical correction to the result reported in [68], and coincided, apart from the vacuum polarization correction, with the result published earlier by Feynman's assistant Michel Baranger.[35]) Including the fourth-order magnetic moment correction[36] they were led to a prediction for the Lamb shift for an infinitely heavy nucleus,

$$\Delta E_\infty = 1058.42\text{MHz}, \tag{9.69}$$

which was completely consistent with the experimental value at the time, 1062 $\pm$ 5 MHz. Unfortunately, the value of the fourth-order anomalous magnetic moment of the electron used at the time was incorrect. A few years later, Schwinger's student, Charles Sommerfield, and A. Petermann independently recalculated that correction.[37,*] This error, however, did not have a very major effect on the result: A recent theoretical value for the Lamb shift is 1057.853 $\pm$ 0.013 MHz (assuming an r.m.s. proton radius of 0.805 $\pm$ 0.011 fm), compared to the experimental value of 1057.851 $\pm$ 0.002 MHz. (These numbers are taken from the review by Kinoshita and Yennie.[38])

## Quantum field theory and condensed matter physics

Raphael Aronson was the youngest of Schwinger's students, having been something of a prodigy himself.[11] Perhaps because of his youth, he felt that he needed more guidance than Schwinger would give, so his experience at Harvard was not totally happy. It was a struggle for him to find a suitable thesis problem. After abandoning a variational approach to scattering (which would later be taken up by Walter Kohn) he recalled that his second thesis problem, in about 1950, was

* Schwinger commented later: 'I cannot refrain from remarking that this same year [1950] saw the first application of the Feynman–Dyson methods to a problem that had not already been solved by other procedures. This was the calculation by Karplus and Kroll[36] of the $\alpha^2$ modification of the electron magnetic moment. They got it wrong. That error remained unnoticed until 1957, when Sommerfield, as his doctoral thesis, used the mass operator technique to produce the right answer.'[37] [197]

to apply the methods of quantum field theory to the many-body problem. But it was too early, and the attempt was unsuccessful.[39] As Schwinger noted later, it was probably only possible after completion of the work on Euclidean field theory [86, 88] for the many-body development to proceed.[2] This is because finite-temperature Green's functions correspond to imaginary frequencies.

Paul Martin presented an entertaining account of the prehistory of their work together.[40] 'During the late 1940s and early 1950s Harvard was the home of a school of physics with a special outlook and a distinctive set of rituals. Somewhat before noon three times each week, the master would arrive in his blue chariot and, in forceful and beautiful lectures, reveal profound truths to his Cantabridgian followers, Harvard and MIT students and faculty.* Cast in a language more powerful and general than any of his listeners had ever encountered, these ceremonial gatherings had some sacrificial overtones—interruptions were discouraged and since the sermons usually lasted past the lunch hour, fasting was often required. Following a mid-afternoon break, private audiences with the master were permitted and, in uncertain anticipation, students would gather in long lines to seek counsel.

'During this period the religion had its own golden rule—the action principle—and its own cryptic testament—"On the Green's Functions of Quantized Fields" [66]. Mastery of this paper conferred on followers a high priest status.† The testament was couched in terms that could not be questioned, in a language whose elements were the values of real physical observables and their correlations. The language was enlightening, but the lectures were exciting because they were more than metaphysical. Along with structural insights, succinct and implicit self-consistent methods for generating true statements were revealed. To be sure, the techniques were perturbative, but they were sufficiently potent to work when power series in the coupling constant failed because, for example, the coupling was strong enough to produce bound states.

'In the dark recesses of the sub-basement of Lyman Laboratory, where theoretical students retired to decipher their tablets, and where the ritual taboo

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* In a later recollection,[41] Martin elaborated: 'Speaking eloquently, without notes, and writing with both hands, he expressed what was already known in new, unified ways, incorporating original examples and results almost every day. Interrupting the flow with questions was like interrupting a theatrical performance. The lectures continued through Harvard's reading period and then the examination period. In one course we attended, he presented the last lecture—a novel calculation of the Lamb shift—during Commencement Week. The audience continued coming and he continued lecturing.'

† Schwinger evidently was aware of the mystique. In a later letter recommending Martin for a permanent appointment at Harvard he stated that Martin was 'superior in intrinsic ability and performance. Quantum field theory is the new religion of physics, and Paul C. Martin is one of its high priests.'[25]

on pagan pictures could be safely ignored, students scribbled drawings that disclosed profound identities between diagrams and sums of diagrams.* Few papers have had so large an influence as these papers and the subsequent, less cryptic version of part of their content in the series "Theory of quantized fields, I–VI" [65, 73, 74, 76, 77, 80]. Clarifying, justifying, and rephrasing the ideas and the techniques that they contain has occupied many physicists and the results of these activities have often been valuable.'[40]

Martin received his PhD in 1954, and went to Copenhagen, where he worked on nuclear many-body problems. This was also the time that great progress was being made in superconductivity.[42] That these 'problems had many common features and that a language and techniques akin to those that Schwinger had introduced for relativistic fields should also be developed for equilibrium systems gradually became apparent' to Martin and his collaborators. 'Upon my return [to Harvard] in 1957, I was fortunate enough to enlist Julian's collaboration in the pursuit of this goal.

'The paper [89] Julian and I wrote in 1958 seems to be the only paper of the nearly 200 his bibliography contains that falls in the area of statistical and solid state physics. But it is far from his only contribution to the field. A number of the seventy students whose doctoral research was directed by Julian worked on theses in solid state and plasma physics and several more have gone on to apply tools and modes of thinking he developed in these fields. Thus, although Julian may not realize the degree to which his techniques and their extensions have pervaded the field, I am revealing nothing new to him when I report that field-theoretic methods are extremely valuable for studying nonrelativistic many-body systems. He and some others among you are likely to be more surprised by the fact that there has also been "spin-off" in the opposite direction, that is, that information about bizarre and unsuspected field-theoretic phenomena have emerged from theoretical studies of superfluid helium films, superconductors, and magnetic materials such as $RbMnF_3$, $K_2NiF_4$, and $LiTbF_4$.'[40]

Later Martin recalled the remarkable summer they spent together in Madison in 1958. 'Julian and I and Clarice and my wife Ann spent a great deal of time together in Madison, Wisconsin, during the summer of 1958. Earlier in 1958,

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* Martin expounded on this forbidden knowledge after Schwinger's death:[41] 'As to conversations we held with him as graduate students, he might frown when one of us drew a Feynman diagram, but we knew all about those diagrams, including how to generate them quickly and concisely from functional equations that bypassed Wick theorems and the like. Sensitive souls, recognizing that a frown was the most overt sign of displeasure Julian would ever display, might have refrained. But I and many others were not sensitive—and none of us were treated less warmly or generously as a result of such transgressions.'

upon my return from Copenhagen and Birmingham, where I had carried out various calculations on interacting fermions and bosons, I turned to Julian with a number of questions and suggestions. He arranged with Bob Sachs for us to join Clarice and him at the University of Wisconsin where he was also lecturing and working on problems in particle physics. It was an eventful summer. I would carry out field-theoretic calculations using our temperature- dependent Green's function approach, and he would generalize them, or he would generalize and carry out his own calculations to see how they went. We would talk about problems together, in his backyard or ours or on an excursion to Taliesin East or on the lake, and then go off to work and write separately. Neither of us would have guessed that this paper would capture the attention of mathematicians who would speak of KMS analyticity.

'Many visitors came through Madison that summer, and Clarice and Julian entertained most of them. Our own home was a dormitory for graduate students visiting Julian. One, who defended his PhD thesis there, was Shelly Glashow. His committee consisted of Bob Sachs, Frank Yang, Julian, and me. A high point of the examination, oft recalled by Shelly and me, was a debate between Frank Yang and Julian. Frank was most unsympathetic to Julian's theory, which Shelly was investigating. Why, Frank asked, should anyone want a theory with more than one two-component neutrino—one in which the muon and electron had different lepton numbers and were associated with different neutrinos? Shelly and I expect that Frank may have forgotten that discussion. [We will discuss Schwinger's two neutrinos in Chapter 12.]

'Although statistical mechanics and condensed matter physics are not fields with which many people associate Julian's name, they should be. In 1961, Julian developed systematic techniques for treating quantum systems away from equilibrium [101]—techniques that require additional Green's functions because there is no fluctuation-dissipation theorem. This theory, also developed by Keldysh, is now being used to describe and analyze the behavior of microelectronic devices. More recently, he and Berthold Englert have studied atomic physics using the Fermi–Thomas approximation and the Casimir effect at finite temperature.'[41] (The latter developments will be discussed in Chapter 15.)

The 'Theory of many-particle systems. I' [89] (II never occurred as a joint publication—it appeared as a joint paper of Martin and Kadanoff[43]) was received by the *Physical Review* on 20 March 1959. Its stated purpose was 'to develop general methods for treating multiparticle systems from the quantum field theoretical viewpoint.' A number of references are then given to the literature, starting with Matsubara,[44] with particular note of the Russian work.[45]

The starting point is the function

$$e^{W(i\lambda,i\tau)} \equiv \mathrm{Tr}\, e^{-iN\lambda-iH\tau}, \tag{9.70}$$

where $H$ is the Hamiltonian, and $N$ the number operator for the system. In terms of the eigenvalues for these operators, $E$ and $N$, this function may be resolved in terms of the spectral density,

$$e^{W(i\lambda,i\tau)} = \sum_N e^{-iN\lambda} \int dE\, e^{-iE\tau} \rho(NE). \tag{9.71}$$

The spectral density may then be formally given by Fourier transformation,

$$\rho(NE) = \int_{-\pi}^{\pi} \frac{d\lambda}{2\pi} \int_{-\infty}^{\infty} \frac{d\tau}{2\pi} e^{iN\lambda + iE\tau + W(i\lambda,i\tau)}. \tag{9.72}$$

The precise meaning of this integral is as the boundary value of an analytic function in the lower half $\lambda$, $\tau$ complex planes.

The above integral may be evaluated asymptotically. To do this, Martin and Schwinger first projected out the trace with a given number of particles,

$$e^{W_N(i\tau)} \equiv \mathrm{Tr}_N\, e^{-iH\tau}, \tag{9.73}$$

which led to

$$\rho(NE) = \int_{-\infty}^{\infty} \frac{d\tau}{2\pi} e^{iE\tau + W_N(i\tau)}. \tag{9.74}$$

Then the integral over $\tau$ could be evaluated by the method of steepest descents, deforming the contour so it passed through the extremum $\beta = i\tau_0 > 0$ defined by the equation

$$E = -\frac{\partial}{\partial\beta} W_n(\beta). \tag{9.75}$$

The result was

$$\rho(NE) = \frac{e^{\beta E + W_N(\beta)}}{\sqrt{2\pi\, \partial^2 W_N(\beta)/\partial\beta^2}}. \tag{9.76}$$

A similar saddle point evaluation of $e^{iW_N(\beta)}$ could be done, with the result that the spectral density had a form given by two parameters $\alpha$ and $\beta$ (ignoring non-exponential factors):

$$\rho \sim e^{\alpha N + \beta E + W(\alpha,\beta)}. \tag{9.77}$$

To introduce the concept of pressure, Martin and Schwinger described the particle by a non-relativistic field $\psi$. Then, they were able to derive the equation

$$\frac{1}{\beta} \ln \rho = \frac{\alpha}{\beta} N + E + pV, \tag{9.78}$$

which had an immediate thermodynamic interpretation if we call $1/\beta = kT$, $k$ being Boltzmann's constant, and $T$ the absolute temperature, $k \ln \rho = S$, the entropy, and $-\alpha/\beta = \mu$, the chemical potential. However, this result applies beyond the realm of thermodynamic equilibrium.

In the third section of the paper, Martin and Schwinger turned to Green's functions, defined here by

$$G_n^{NE}(\mathrm{r}_1 t_1 \ldots \mathrm{r}_n t_n; \mathrm{r}'_n t'_n \ldots \mathrm{r}'_1 t'_1)$$
$$= (-i)^n \epsilon \Big[NE\Big|\Big(\psi(\mathrm{r}_1 t_1)\cdots\psi(\mathrm{r}_n t_n)\psi^\dagger(\mathrm{r}'_n t'_n)\cdots\psi^\dagger(\mathrm{r}'_1 t'_1)\Big)_+\Big|NE\Big], \tag{9.79}$$

with the notation

$$[NE|X|NE] \equiv \frac{\mathrm{Tr}_{NE}\, X}{\mathrm{Tr}_{NE}\, 1}. \tag{9.80}$$

Time ordering is denoted by the + subscript, and the $\epsilon$ symbol is 1 for bosons, and $\pm 1$, depending on the time-ordering, for fermions.

Two-particle Green's functions were discussed in the next section, and, for example, the fluctuation-dissipation theorem was derived as a relation between the current–current correlation function and the conductivity.[46]

Section V was devoted to determining the Green's functions that determine physical properties. These were determined by an infinite sequence of coupled equations; numerous properties could be obtained from approximate solutions of the first few of these. More powerful field-theoretic techniques were introduced in Section VI, namely functional differential equations. This provided the starting point for general investigations of multiparticle systems.

Paul Martin recently recalled that he primarily wrote this paper, but that 'Julian did much of the calculation. He had the big picture into which we tried to put things. He was committed to the idea that things must be understandable and beautiful and fit into a framework. The 1950s were so good for Julian because he had the right framework for a large body of knowledge.'[32]

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# 10

# The world according to Stern and Gerlach

Schwinger was a master of quantum mechanics from his earliest days. His first, unpublished, paper [0], written at age 16, already showed his control of the entire machinery of relativistic quantum mechanics. Yet it was undoubtedly his contact with I. I. Rabi at Columbia that led to his deeper understanding and reformulation of quantum mechanics. Rabi was doing experiments with atomic beams at Columbia in the mid-1930s, and the question was how atomic and nuclear spins interacted with magnetic fields. We recall that Rabi[1] and Schwinger [4] independently contributed to the development of this theory. The importance of the latter paper, in Schwinger's view, was that it led to new insights into quantum mechanics, only implicit then.

But this, at the time, was not a burning issue with Schwinger. In the late 1930s there was the puzzling problem of the mesotron to solve. Then the war came, with the challenges of radar and microwave cavities, and, ultimately, synchrotron radiation. After the war, quantum electrodynamics became the primary focus. But the settled job at Harvard brought teaching graduate classes as a major endeavor. Within a year or two, he started to teach quantum mechanics on a regular basis.* Of course, Schwinger never regarded there to be a barrier between teaching and research, and his course on field theory always chronicled his current investigations into the structure of matter. By about 1950 Schwinger had begun his third reformulation of quantum electrodynamics, or in general, quantum field theory, based on the quantum action principle (see

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* One of the two courses he taught in his first semester at Harvard was on nuclear physics. In fact, at least the first semester of that three-semester course was devoted to quantum mechanics rather in the style of Dirac.[2,3] John Blatt from MIT attended the lectures, and took excellent notes, which were then shipped off to Princeton where graduate students copied them onto ditto masters. Because these secondary students did not know what they were copying, curious transcription errors occurred, such as the 'military matrix' instead of the 'unitary matrix.' Still these notes 'became the most precious thing I owned,'[2] and the 'best book on quantum mechanics.'[3]

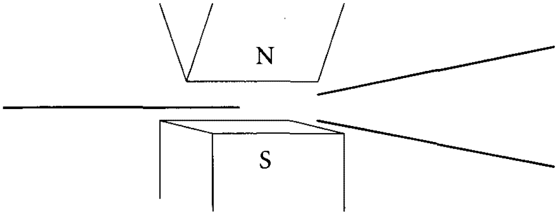


Fig. 10.1 Schematic diagram of a Stern–Gerlach apparatus, causing a beam of spin-$\frac{1}{2}$ atoms to be split into two sub-beams. The wedge-shaped north pole of the magnet produces an inhomogeneous field.

Chapter 9).* This is a general principle (the quantum generalization of the stationary principles of classical physics), and therefore it was applicable to any quantum system. So it was not surprising that about this time Schwinger began a truly novel way of presenting and formulating quantum mechanics. But the quantum action principle was a dynamical principle, and quantum kinematics must be described first; and for this he reached back to his experience with Rabi.

A beam made up of atoms with spin may be separated into components with different projections of the spin along a given axis, different 'magnetic quantum numbers,' by the application of an inhomogeneous magnetic field. This is the famous Stern–Gerlach apparatus, illustrated in Fig. 10.1. Particularly simple is a beam of spin-$\frac{1}{2}$ atoms, or electrons, which is split into exactly two beams by such a device. Schwinger realized that such a system, with only two, or a few, states, is far simpler to treat than the wavefunction describing the position of an electron, say, where there are an infinite number of states. The mathematics is not that of differential operators and integrations over square-integrable functions, but that of small matrices. So Schwinger made the analysis of successive Stern–Gerlach experiments on spin systems the basis of his introduction to quantum mechanics—all the properties of quantum mechanics could be inferred from a few simple experimental facts. A mathematics, 'measurement algebra,' was invented to describe successive measurements, which led inexorably to the transformation function theory of Dirac.

So Schwinger taught variations of this approach to quantum mechanics continuously, from 1950 until the 1990s. Yet, tragically, only fragments were published. The first attempt was in his Les Houches lectures of 1955. Starting in 1959, portions appeared in brief reports published in the *Proceedings of the National Academy of Sciences* [91, 93, 96–98, 102, 106]. With the assistance of Robert Kohler, of the State University College at Buffalo, a very incomplete

* For example, in his fall 1950 course on quantum mechanics,[4] already the quantum action principle made its appearance.

book, *Quantum kinematics and dynamics*, did appear in 1970 [152]. Kohler, who was not a student of Schwinger, had attended Schwinger's Les Houches lectures, and proposed a far more ambitious project, encompassing the field theory book Schwinger was trying to write at the same time. But Schwinger opted for a minimal volume, because he still hoped to write definitive books on quantum mechanics and field theory on his own.[4] Many years later, after Schwinger had resettled at UCLA, a former Harvard student, Robert Hilborn, proposed turning his lecture notes from Harvard into a more complete textbook. Schwinger wrote him a not unfavorable response: 'Dear Prof. Hilborn, Thank you for your letter of October 22 about your class notes of Physics 251 (1968–69). I had given that course a number of times in earlier years, always in different ways, and have copious notes of my own on the material. But I do not find any record of 1968–69 and therefore must have, for the first time, repeated an earlier version. (Horrors!) I mention this only to point out that I no longer have a memory of what was presented, and, correspondingly, find it difficult to judge the merits of giving it wider distribution. Could you send me a copy of your material, both to refresh my memory and to see how you have organized it.

'I have only one experience of having unpublished material put out for me, and that worked out well [152]. Perhaps this could serve as a model for another collaboration, if it should seem mutually agreeable.'

But this second attempt came to nothing. This was about the time that Schwinger and his postdocs, DeRaad, Milton, and Tsai, were embarking on the *Classical electrodynamics* book project,* and they all had vaguely in mind the idea of following it up with a similar quantum mechanics book, so Hilborn was put off with a suggestion that he might be involved at some unspecified later date.

Why do we term this failure to complete the book tragic? Because Schwinger apparently very much wanted this exposition, which had been so well received by his students (a bit of it appears in Kurt Gottfried's excellent book[5]), to reach a larger audience, and so he attempted to write his definitive quantum mechanics book throughout his life.† In particular, during his last decade, after abandoning the *Classical electrodynamics* project, he devoted an enormous amount of time to such a book, yet hardly a fragment seems to remain in the archives.‡ And

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* That book was to appear posthumously only twenty years later [231]. We will describe this project in Chapter 15. Earlier competition for Schwinger's attention was a book on quantum field theory he had promised to write for Addison-Wesley in the mid-1950s.

† For example, an earlier fragment of a manuscript, distinct from the Les Houches lectures, exists in the Schwinger archive at UCLA.[4]

‡ In a lecture he gave about 1990 at Ulm/Donau entitled 'Quantum mechanics: symbolism of atomic measurement,' he began by saying 'This is the title of a book I've been

tragic too, because as the lecture notes and their memory fades, an important contribution to the foundations and practice of quantum mechanics is being lost to future generations.

This chapter will relate the story of this obsession. Not only will we describe measurement algebra, but we will relate three other episodes that bear on this story. First, around 1950, again based on the Rabi experience, Schwinger presented a definitive basis for the theory of angular momentum, based on elementary harmonic oscillator variables (raising and lowering operators). This paper was widely circulated,* and greatly appreciated, but never published until it was included in the Biedenharn and Van Dam collection in the mid-1960s [69]. Then there were important contributions to the theory of potential problems [100, 116], and to the Brownian motion of an oscillator [101] (bearing on non-equilibrium statistical mechanics), all of which grew out of his course and summer school lectures. Toward the end of his life, due to his interaction with B.-G. Englert and Marlan Scully, he wrote three papers on 'Humpty Dumpty' [208–210], referring to the question of whether that which has been broken apart—in this case a spin system—can be put back together again, the answer being no. But some useful ideas emerged from this late work (such as a 'MAGIC' interferometer). Along the way, we will try to answer the question why one who had done so much to bring quantum mechanics to new levels was so incapable of presenting it to the world at large.

## The quantum theory of measurement

After a summer of traveling in Europe in 1953, Schwinger had a sabbatical in 1954. That was the year they were in 'Calcutta.'[6] 'I took a sabbatical from Harvard but I didn't go anywhere. I sat down to write the book on quantum mechanics. I didn't write it then, but I sure had lots of pads of paper. I remember only one unpleasant episode that year. I was working like mad trying to write it all down, utterly resenting any intrusions, which of course were many. If you stay at home, the phone is always ringing and so forth. And I think Clarice called and said Pauli is in town and would like to see you. I'm afraid I snarled and said "Sorry."'[7] Clarice recalled that it was David Frisch of MIT who called. She told

writing in my head for 40 years.' A lecture of the same name, delivered to the Society of Physics Students at the University of New Mexico in 1991, was recorded on videotape, a copy of which is in the archives.[4]

* It appeared as an Atomic Energy Commission report: USAEC report NYO-3071 [69]; as a mark of its impact, we note that many years later Schwinger received a letter from W. A. Nierenberg, Director Emeritus of the Scripps Institute, returning the report: 'Enclosed is a copy of your 1952 publication on angular momentum. It was great, but I no longer need it.'[4]

him, 'Oh, he went to Calcutta,' and he said, 'Clarice, why didn't you go with him?' Clarice replied, 'I did, I did.' So, they referred to that year as Calcutta.[6] 'That is indeed the origin of the Black Hole of Calcutta.'[7]

The following summer, 1955, the Schwingers went to Italy and France, where Julian lectured at Pisa and at the famous summer school in Les Houches.* His lectures 'on quantum mechanics and quantum field theory were never publicly distributed. But I do possess, as always, copies. They began with quantum mechanics as I then understood it and I assume I went through all the same measurement algebra, action principles, simple problems in quantum mechanics, quantum field theory, second quantization, and went on and on. It was only two weeks but I taught at a very fast pace.'[7]

'The notes were not printed at the time, but there was a book, which, by some miracle, got made from part of those notes and which got put out with the aid of some guy [Kohler] who appeared out of nowhere and said I have these notes of yours from Les Houches and I think they ought to be publicly distributed and I'm willing to help put them out. And that was the basis of the book that got published in 1970 called *Quantum kinematics and dynamics* [152].'[7] The foreword to that volume fills in a bit more of the history: 'Early in 1955 I began to write an article on the Quantum Theory of Fields. [Presumably this was an article for the *Handbuch der Physik*.] The introduction contained this description of the plan. "In part A of this article a general scheme of quantum kinematics and dynamics is developed within the nonrelativistic framework appropriate to systems with a finite number of dynamical variables. Apart from specific physical consequences of the relativistic invariance requirement, the extension to fields in part B introduces relatively little that is novel, which permits the major mathematical features of the theory of fields to be discussed in the context of more elementary systems."

'A preliminary and incomplete version of part A was used as the basis of lectures delivered in July 1955 at the Les Houches Summer School of Theoretical Physics. Work on part A ceased later that year and part B was never begun. Several years after, I used some of the material in a series of notes published in the *Proceedings of the National Academy of Sciences*. And there the matter rested until, quite recently, Robert Kohler (State University College at Buffalo) reminded me of the continuing utility of the Les Houches notes and suggested their publication. He also volunteered to assist in this process. Here is the result. The main text is the original and still incomplete 1955 manuscript, modified

* Cecille DeWitt had invited Schwinger to come to Les Houches for two months, which would have enabled them to provide support through a Fullbright grant. Schwinger agreed only to come for two weeks, which necessitated him getting an NSF grant to attend the conference in Pisa as well, which almost did not come through.[4]

only by the addition of subheadings. To it is appended excerpts from the *Proc. Nat. Acad. of Sciences* articles that supplement the text, together with two papers that illustrate and further develop its methods' [101, 116].

After the Les Houches summer school, the Schwingers drove 'straight north through Germany. On the way we stopped in Hamburg, which was again a contribution to gastronomy because we went into a restaurant and ordered lobster and the waiter looked at us and said "How many?"'[7] The waiter suggested tactfully that perhaps half a lobster each would be enough because they were so expensive. Indeed, Julian and Clarice each ordered half a lobster which certainly satisfied them financially.[6] 'I think we did not cut a distinguished figure as we staggered out of the car into this fancy restaurant.'[7] They were flabbergasted when the bill came,[6] 'but we were only half as flabbergasted as the waiter.'[7] This time they had brought plenty of money with them. From Germany, the Schwingers drove on to Copenhagen, where Stanley Deser had arranged a residence for them, complete with silver and Royal Copenhagen porcelain.[6] Walter Gilbert, who would become Julian's assistant a couple of years later, had persuaded them to go to Copenhagen, and Stanley Deser, who was a postdoc then at the Niels Bohr Institute, found a place for them to stay, a house in Rungsted, which is just outside Copenhagen. The Schwingers rented the lower floor of a marvelous old palatial house. Initially, the landlady wanted only to rent to embassy people; Deser persuaded her that Julian was infinitely more impressive than any diplomat. The previous tenants had young children so she had put away all her china and sterling, but Deser got her to bring it all out again. The Schwingers had a flat with a balcony and a rose garden, and a view of the sea. They stayed there for 10 days. Every day Deser would take them sightseeing and buying silverware. They enjoyed living near the sea; it was for them a magical time. Clarice recalled saying on one of their drives to the beach, 'I'm happy and I know I'm happy.'[6]

That summer 'we had a very good time. And if you ever look back in the meteorological records of European summers, '55 was unique. No rain, warm, a very good year for wine and travel.'[7]

Schwinger's first publication on his new formulation of quantum mechanics did not appear until he published 'The algebra of microscopic measurement' in the *Proceedings of the National Academy of Sciences* in 1959 [91]. But this was merely 'a long-delayed report of what had appeared in lectures published by the National Bureau of Standards.'[7] Schwinger visited Los Angeles in the summer of 1952, during the height of the McCarthy era anti-Communist hysteria, when the University of California had adopted a loyalty oath. 'Dave Saxon refused to sign and therefore lost his job. I don't know how this happened and I don't recall the details, but the National Bureau of Standards either had or set up an office at UCLA and gave Dave a job and then he invited me to give lectures there, and these are the lectures I gave at UCLA. As it says, performed by contract with the University of California, sponsored in part by the Office of Naval Research.'[7]

This new approach, in fact, naturally first appeared in Schwinger's Harvard lectures a year or two earlier.* 'From 1950 or maybe 1951 I would begin with a very definite approach in which quantum mechanics was the symbolism of atomic measurement. And I would begin then, as I would now, except it would be much smoother now, to tell them about the Stern–Gerlach experiments. And then I would introduce a symbolism of simple Stern–Gerlach experiments, composite Stern–Gerlach experiments, symbolize it by what I called a measurement symbol, and the measurement symbol algebra then evolved into quantum mechanics. My approach was self-contained, involving one raw bit of experimental data from which everything else could be deduced. And the raw data to me was the still mind-blowing outcome of the Stern–Gerlach experiments, in which instead of the beam just being spread, [it is split into discrete sub-beams]. That to me encompasses all of quantum mechanics. The spirit was just to evolve in a sort of natural way, not deduce but evolve, the whole machinery from the very beginning. Each time I did it, it became a little more sophisticated.'[7]

Richard Arnowitt recalled that in the courses he took from Schwinger during his period as a student at Harvard (1948–52) 'Everything was in the language of Green's functions. For example, in ordinary quantum mechanics the poles of the Green's functions are the energy eigenvalues.'[8] During his last year at Harvard, Arnowitt graded for Schwinger's quantum mechanics course. 'This was when measurement algebra was first introduced. In December [nearly the end of the first semester] Julian decided, "Gee, I need to introduce Schrödinger's equation so they can take other courses," '[8] for up to that point he had concentrated on quantum kinematics, somewhat as we will sketch in the next subsection. He did go on to introduce the quantum action principle, which, for example, was 'essential for Arnowitt, Deser and Misner[9] getting the canonical variables for general relativity.'[8]

Why did Schwinger publish these lectures only years later, in a journal of distinction, but with almost no readership among physicists? 'This is, after all, seven years later in which I was trying to put down in public literature but not run through the danger of having to confront a referee. I was sure any referee would say what are you doing this for, this is not publishable. I wanted it recorded somewhere and in those days anybody belonging to the National Academy could submit papers and they would be published. So I made extensive use for quite a while of that liberty to get across what I had [to say without

* Schwinger's recollection here may be a bit faulty. According to the notes of the courses he taught in 1950, 1951, and 1952, measurement algebra appeared only in his lectures at Harvard in the fall of 1952, after the summer he spent at UCLA.[4]

having] to argue [with] other people's ideas about what should or should not be published. It was, in the language of American football, an end run.'[7],*

This is how Schwinger described Measurement Algebra in 1988, shortly after he had given an overview of his approach in an homage to Hermann Weyl [208a]. 'The basic operation I considered was a measurement in which a collection, a beam of atoms, whatever, entered the apparatus; they are allowed to enter only if they were in a certain physical state having certain physical properties and they could leave only if they had either the same or different [specified] values. So this was a selection operator. And these are the fundamental operations out of which all physical properties ... emerge. This measurement apparatus selects only one kind of atom, meaning atoms with certain values of physical properties, and puts out another kind of atom. As far as anything observable is concerned it is as though this consisted of two stages; the first stage destroys the incoming atom, the second stage creates the outgoing atom. Nothing is different. The net result, only this comes in, only that goes out. But if one symbolizes these acts of annihilation and creation so that these measurement symbols are now written as products of each, then one is led to—I can't say more fundamental, but it's an algebraically simpler way of presenting the formalism in which the whole point is not really to say it in words but to represent this act of annihilation and creation by a symbol. Then the question is what are the algebraic properties of the symbols. When multiplied in this order, they produce a physical operator. When multiplied in that order, they produce a number. The initial experiment is to take away and then put in its place. Now we do it in the other order. First we create something and then we annihilate it. So we start from effectively nothing, we create something and then we annihilate. Well, unless what we're annihilating is the only thing available, you can't do it. So it's zero. If you can annihilate exactly what is produced, you do so with certainty, so it's one. I have just described to you the orthonormality properties of a system of vectors. So we are immediately at vector space, and we already have the construction of operators in terms of products of all this stuff. From this point to the probability interpretation is immediate. The whole structure of quantum mechanics emerges. It gives the natural foundation for the interpretation of quantum mechanics. [This approach attempts] to lay the foundations in a way that would be unarguable. I get so tired of reading endlessly about the interpretation or the problems in the theory of measurement, and I believe it's utter nonsense.'[7]

* An 'end run' is an American football play in which the ball-carrier attempts to run wide around the end of the line of players as an evasive trick.

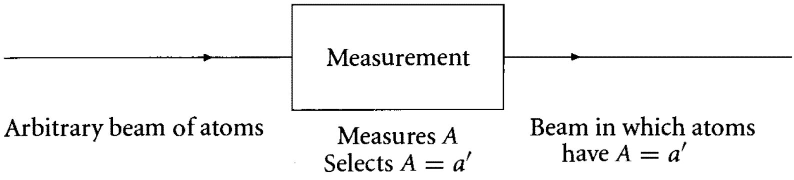


Fig. 10.2 Diagram of elementary measurement process; here for simplicity we regard atoms as possessing but one property, called $A$.

## Measurement algebra

Here we wish to sketch the mathematical basis of Schwinger's measurement algebra.* It is a symbolic representation of what was learned from an analysis of Stern–Gerlach measurements, where a particular value of a physical property, the component of the magnetic moment of the atom in the $z$-direction, or, equivalently, the $z$-component of angular momentum, is selected. In general, let us say that a physical property $A$ is measured, and the results of the measurements are the possible values of $A$, the set of real numbers

$$a_1, a_2, \ldots, a_n; \tag{10.1}$$

let a typical value be denoted by $a'$ or $a''$. A measurement is also a selection. In the Stern–Gerlach experiment, we select a particular value of $J_z$, stopping all other sub-beams with different $J_z$ values. In general the situation is as illustrated in Fig. 10.2. The beam coming out of the measuring apparatus is said to be in a definite state, in which $A = a'$. To represent this measurement, we introduce the symbol,† the *measurement symbol*, $|a'|$. It corresponds to a selective measurement in which the property $A$ is measured, and only those atoms which have $A = a'$ are selected.

If we follow one Stern–Gerlach measurement by an identical one, nothing further happens. The second measurement simply verifies the first. Symbolically, we express this as

$$|a'||a'| = |a'|. \tag{10.2}$$

On the other hand, if we measure, in the second experiment, a different state, we get nothing. That is, if the first Stern–Gerlach experiment selects $J_z = \frac{1}{2}\hbar$ and the second $J_z = -\frac{1}{2}\hbar$, no atoms emerge from the composite measurement

* The following is based on a course Schwinger started teaching at UCLA in 1978, aimed at undergraduates.

† Earlier, Schwinger had used the notation $M(a')$, but he came to realize that the vertical bar notation, although possibly admitting of confusion with the absolute value sign, was more convenient for later developments.

consisting of the first experiment followed by the second. The general symbolic statement of this is

$$|a'||a''| = 0, \quad \text{if } a' \neq a'', \tag{10.3}$$

where 0 is the symbol of a measurement that rejects everything. What the first selects, the second rejects. (It makes no difference if these equations are read from right to left, or from left to right.) The symbol 0 has the following properties:

$$\begin{aligned} |a'|\,0 &= 0, \\ 0\,|a'| &= 0, \\ 0\,0 &= 0. \end{aligned} \tag{10.4}$$

The first two equations state that if you attempt to measure a property, before or after rejecting everything, you get nothing. We are beginning to see an algebra, in which the multiplication of symbols represents performing one experiment after another.

Now, what do we mean by the addition of measurement symbols? $|a'| + |a''|$ represents a less selective measurement in which the selected atoms have *either* property $A = a'$ or $A = a''$ without discrimination. We do not mean that you measure $a'$, $a''$ separately and put these selected 'beams' back together. No separation of atoms with property $A = a'$ or $A = a''$ is made. Intervention by measurement is a dramatic event. Here, we do not distinguish $a'$ from $a''$ in any way. By the physical meaning of addition, the order does not matter:

$$|a'| + |a''| = |a''| + |a'|. \tag{10.5}$$

Similarly, we could perform an even less selective measurement, in which $A = a'$, $a''$, or $a'''$ is selected without discrimination. This is represented by the symbol $|a'| + |a''| + |a'''|$, where the terms can be written in any order. We can keep going in this manner until we select atoms which have any value of the property $A$ without discrimination. The symbolic transcription of this is

$$|a_1| + |a_2| + |a_3| + \cdots + |a_n| = \sum_{a'} |a'| = 1, \tag{10.6}$$

where 1 is a symbol for a measurement that selects all systems without discrimination—that is, no measurement at all, since nothing is done to the beam of atoms. The properties of 1 are evident, since it corresponds to letting everything through:

$$\begin{aligned} |a'|\,1 = 1\,|a'| &= |a'|, \\ 1\,1 &= 1, \\ 1\,0 = 0\,1 &= 0; \end{aligned} \tag{10.7}$$

1 has the algebraic properties of unity.

By physical arguments it is easy to show that the distributive property holds in this new algebra:

$$\left(\sum_{a'} |a'|\right) |a''| = \sum_{a'} \left(|a'||a''|\right), \tag{10.8}$$

since the two sides of this equation have the same physical meaning, represented by the symbol $|a''|$.

Now we want to introduce a symbol for the physical quantity $A$ itself. Since $|a'|$ represents a 'filtration' of the beam, filtering out only those atoms in which $A = a'$, we let the symbol for the property $A$, also called $A$, satisfy

$$A|a'| = a'|a'|. \tag{10.9}$$

This means that if we first select atoms with property $A = a'$ and then measure $A$ we will of course get the value $a'$. We can read these symbols either way, so we also have

$$|a'|A = |a'|a'. \tag{10.10}$$

From this, we can write $A$ explicitly in terms of the measurement symbols $|a_1|$, ..., $|a_n|$:

$$A = A1 = A\sum_{a'} |a'| = \sum_{a'} A|a'| = \sum_{a'} a'|a'|, \tag{10.11}$$

since multiplication is distributive. This exhibits $A$:

$$A = \sum_{a'} a'|a'|. \tag{10.12}$$

It is easy to see that this is consistent with the original definition of $A$, given by Eqn (10.9).

The algebra developed to this point is too special. Let us consider measurements in which atoms are selected in one state, and emitted in another state, that is, where there is a change of state. Fig. 10.3 illustrates this for the Stern–Gerlach experiment. It is easy to convince oneself that such a change of state is possible; for example, the magnetic dipole moment in the Stern–Gerlach example can be made to precess through 180° about a perpendicular constant field; or, more practically, a parallel magnetic field can cause the spin states to split in energy according to the dipole interaction energy $E = -\mu \cdot \mathrm{H}$. In this case a resonant electromagnetic wave can induce the transition from the $m = \frac{1}{2}$ state to the $m = -\frac{1}{2}$ state.

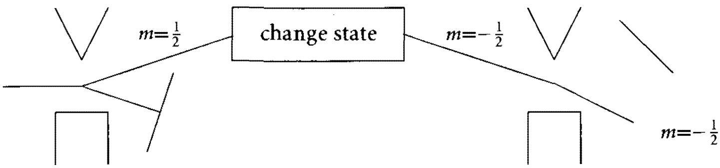


Fig. 10.3 Two identical Stern–Gerlach apparatuses, set up so that the first selects spin angular momentum along the $z$-axis of $J_z = m\hbar = \hbar/2$, and the second $J_z = m\hbar = -\hbar/2$. Between the two measurements is a device which changes the states of the atoms from $m = +\frac{1}{2}$ to $m = -\frac{1}{2}$.

We generalize from the picture and consider a measurement of a property $A$ in which atoms are selected with $A = a'$ but emitted with $A = a''$. The measurement symbol for this is $|a'a''|$, which again can be read in either order. This generalizes what we had before: when there is no change in state, $|a'a'| = |a'|$. Suppose we follow one such measurement by another such,

$$|a'a''||a''a'''| = |a'a'''|. \tag{10.13}$$

The first measurement (if we read form left to right) selects $a'$, emits $a''$, the second measurement selects $a''$ and emits $a'''$; the net effect is to select $a'$ and emit $a'''$. Everything emitted by the first measurement is selected by the second. On the other hand, if the intermediate states are different,

$$|a'a''||a'''a^{\text{iv}}| = 0, \quad a'' \neq a''', \tag{10.14}$$

because what is produced by the first stage cannot enter the second; the second rejects what the first emits. We can put these two statements together by defining a $\delta$-symbol,

$$\delta(a', a'') = \begin{cases} a' = a'' : & 1 \\ a' \neq a'' : & 0 \end{cases}. \tag{10.15}$$

Then we have

$$|a'a''||a'''a^{\text{iv}}| = \delta(a'', a''')|a'a^{\text{iv}}|. \tag{10.16}$$

Now we see that we have entered something new; by the physical meaning multiplication is *non-commutative*:

$$|a'a''||a'''a^{\text{iv}}| \neq |a'''a^{\text{iv}}||a'a''|. \tag{10.17}$$

Next we must learn how to extract probabilities from our measurement algebra. Thus, suppose we consider two successive Stern–Gerlach experiments, but

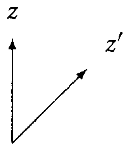


Fig. 10.4 Orientation of two successive Stern–Gerlach experiments.

which are now rotated relative to each other. That is, one of them measures a particular value of the angular momentum along the $z$-axis, the other along the $z'$-axis; the two directions are, in general, not the same, as Fig. 10.4 shows. We want to learn how to calculate $p(m, m')$, the probability of finding $J_z = m\hbar$, given that the first measurement obtained $J_{z'} = m'\hbar$.

In general, suppose we have two different properties $A$, $B$. We first select systems with $B = b''$. What is the probability $p(a', b'')$ of subsequently measuring $A = a'$? We have in mind here that a measurement of $A$ *or* a measurement of $B$ describes the system fully. If we know the value of either $A$ or $B$ we know all we can about the system. We cannot measure $A$ and $B$ simultaneously; measurement of $A$ destroys what is known about $B$. This is precisely the case with the $J_z$, $J_{z'}$ example. The sequence of selective measurements described above is represented by $|b''||a'|$. The second measurement destroys at least some of the information determined by the first measurement. To see what happens as a result of the $A$ measurement, we remeasure $B$: $|b''||a'||b''|$. This measures the effect of the disturbance produced by the intermediate $A$ measurement. The net effect is a selection of $B = b''$:

$$|b''||a'||b''| = \text{number}|b''|, \tag{10.18}$$

since the overall effect is a selection of $b''$ and an emission of $b''$. What is the number here? If $A = B$, $b'' = a''$.

$$|a''||a'||a''| = \delta(a', a'')|a''|; \tag{10.19}$$

that is, if $a' = a''$, we are just repeating the same selective measurement. Then, the number is 1, which expresses the *certainty* of the second measurement giving $A = a'$. If $a' \neq a''$, the second measurement rejects all that comes from the first; the second measurement will *certainly not* find any systems with the property $A = a'$. This example makes us suspect

$$|b''||a'||b''| = p(a', b'')|b''|, \tag{10.20}$$

with $p(a', b'')$ being the probability of finding $a'$ given $b''$.

Because of the property of the totally non-selective measurement, Eqn (10.6), it is easy to verify that this hypothesis is consistent with the probability requirement,

$$\sum_{a'} p(a', b'') = 1, \tag{10.21}$$

that is, the sum of the probabilities of all possible outcomes is unity. The other crucial property that must be verified is non-negativity. For the verification of this, a bit more machinery needs to be invented.

It is convenient to decompose the measurement symbol $|a'a''|$ into two acts, as long as we do not try to state how the transition occurs:

1. Atoms are removed from state $a'$.
2. Atoms are put into state $a''$.

The algebraic counterpart of this two-stage process is

$$|a'a''| = |a'\rangle\langle a''|, \tag{10.22}$$

where $|a'\rangle$ represents removal of the atom from $a'$, and $\langle a''|$ represents placing the atom in $a''$. Is this factorization consistent with what has gone before? The statement (10.16) is consistent, provided

$$\langle a''||a'''\rangle \equiv \langle a''|a'''\rangle = \delta(a'', a'''), \tag{10.23}$$

so we see $\langle a''|a'''\rangle$ as a number describing an internal rearrangement. We interpret this symbol as: start with no atoms; put out atoms in state $a''$, absorb atoms in state $a'''$; put out no atoms. If $a'' \neq a'''$ there is nothing to absorb, so we get zero.

We now see that every physical state has two 'vectors' associated with it, $|a'\rangle$ and $\langle a'|$. These are unit, orthogonal vectors, in the sense of Eqn (10.23). Because we have two kinds of vectors,* we have here a *complex* geometry; $\langle a'|$ is some sort of 'complex conjugate' to $|a'\rangle$. The geometry is $n$-dimensional if there are $n$ values of $a'$.

As with an ordinary vector space, one can represent the geometry by different coordinate systems. Here, changing the coordinate system corresponds to measuring different physical quantities—such as components of angular momentum in one, or another, direction. Suppose we have two physical quantities $A$ and $B$, which have as typical results of measurements $a'$ and $b'$, respectively. The corresponding states are, in the first case $|a'\rangle$, $\langle a'|$, and in the second $|b'\rangle$, $\langle b'|$.

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* Schwinger called these two types of vectors left vectors ($\langle a'|$) and right vectors ($|a'\rangle$). They are what Dirac called bra and ket vectors, from the division of the word *bracket*. 'I couldn't see myself using such words, so I didn't.'[7]

If we measure property $A$ and get all possible states without discrimination, we have the unit symbol, representing no measurement at all, and similarly for $B$:

$$1 = \sum_{a'} |a'\rangle\langle a'| = \sum_{b'} |b'\rangle\langle b'|. \tag{10.24}$$

The states are represented by unit orthogonal vectors:

$$\langle a'|a''\rangle = \delta(a', a''), \quad \langle b'|b''\rangle = \delta(b', b''). \tag{10.25}$$

Thus $\{|a'\rangle\}$, $\{|b'\rangle\}$ are two different orthonormal sets of vectors. We can now transform from one of these orthonormal sets to the other,

$$\langle a'| = \langle a'|1 = \langle a'| \sum_{b''} |b''\rangle\langle b''| = \sum_{b''} \langle a'|b''\rangle\langle b''|. \tag{10.26}$$

This expresses how a $\langle a'|$ vector is written in terms of $\langle b''|$ vectors. The expansion coefficients are scalar products of $a$ and $b$ vectors—these numbers express how the old coordinate system is related to the new coordinate system. In other words, $\langle a'|b''\rangle$ is a kind of direction cosine. The transformation works similarly with right vectors,

$$|a'\rangle = 1|a'\rangle = \sum_{b''} |b''\rangle\langle b''|a'\rangle. \tag{10.27}$$

This expresses right $|a'\rangle$ vectors in terms of right $|b''\rangle$ vectors, with 'direction cosines' $\langle b''|a'\rangle$. But, unlike in Euclidean geometry, the two kinds of direction cosines are not identical,

$$\langle b''|a'\rangle \neq \langle a'|b''\rangle. \tag{10.28}$$

The second kind also appears in

$$\langle b''| = \langle b''| \sum_{a'} |a'\rangle\langle a'| = \sum_{a'} \langle b''|a'\rangle\langle a'|, \tag{10.29}$$

the expression of left $b$ vectors in terms of left $a$ vectors.

Now we return to the probability hypothesis (10.20). If we analyze this in six stages,

$$|b''\rangle\langle b''|a'\rangle\langle a'|b''\rangle\langle b''| = p(a', b'')|b''\rangle\langle b''|, \tag{10.30}$$

we see

$$p(a', b'') = \langle b''|a'\rangle\langle a'|b''\rangle = \langle a'|b''\rangle\langle b''|a'\rangle = p(b'', a'); \tag{10.31}$$

the probabilities are constructed as the product of both kinds of direction cosines, and are therefore the same whichever the order of the two measurements.

Now impose the physical requirement that probabilities be *real, non-negative* numbers. This will automatically be true (and it is essentially impossible to think of any other possibility that is not reducible to this) if the two 'direction cosines' are complex conjugates of each other,

$$\langle a'|b''\rangle = \langle b''|a'\rangle^*, \tag{10.32}$$

for then

$$p(a', b'') = |\langle a'|b''\rangle|^2 \geq 0. \tag{10.33}$$

The unitarity statement (10.21), that the sum of the probabilities adds up to one, now has a geometrical interpretation, that the sum of the absolute squares of the 'direction cosines' is unity:

$$1 = \sum_{a'} |\langle a'|b''\rangle|^2. \tag{10.34}$$

We can also show that the number $n$ of $a$ states is equal to the number of $b$ states. We call the set of $n^2$ complex numbers, $\langle a'|b'\rangle$, following Dirac,* the *transformation function.*

In this straightforward, incisive way, Schwinger laid out the basis of quantum kinematics. As we see, state vectors emerge as a convenient mental decomposition from elementary measurement processes. The measurement algebra is, in fact, the algebra of quantum operators, acting on left and right vector spaces describing physical states. In his courses, Schwinger went on to build up the entire machinery of quantum mechanics on this foundation. When the time came to include dynamics, the quantum action principle, which we have discussed in the previous chapter, was naturally embedded in this framework.

## The National Academy papers

As noted, this development was only published starting in 1959 in a series of articles in the *Proceedings of the National Academy of Sciences.* What was the content of these relatively short communications? In Schwinger's words, it began with 'The algebra of microscopic measurement' [91], which 'was the first of a series devoted to the mathematics of quantum mechanics as a symbolic presentation of physics.'[11] That paper largely is a presentation of the development sketched in the previous subsection.

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* The definitive account of Dirac's formulation of quantum mechanics is given in his book *The principles of quantum mechanics.*[10]

In December 1959 Schwinger communicated 'The geometry of quantum states' [93], which continued this development. Here the factorization (10.22) of the measurement symbol into the product of right and left vectors, albeit with a more clumsy notation, was introduced. Schwinger discussed the change of bases, and introduced operators and matrix elements. The point of the following paper, 'Unitary operator bases' [96], was stated in the opening paragraph: 'To qualify as the fundamental quantum variables of a physical system, a set of operators must suffice to construct all possible quantities of that system. Such operators will therefore be identified as the generators of a complete operator basis. Unitary operator bases are the principal subject of this note.' In particular, he considered a system defined by a finite set of vectors $\langle a^k|$, $k = 1, \ldots, N$, and considered the properties of the unitary operators that translate between these states,

$$\langle a^k|V = \langle a^{k+1}|, \quad k = 1, \ldots, N, \tag{10.35}$$

where periodicity is imposed, $\langle a^{k+N}| = \langle a^k|$, so $V^N = 1$. The eigenvalues of $V$ are phases, $\mathrm{e}^{2\pi\mathrm{i}k/N}$, and one can transform to another basis, $\{\langle v^k|\}$, in which $V$ is diagonal. The $v$ states similarly have a translation operator, which is conveniently defined as

$$\langle v^k|U = \langle v^{k-1}|. \tag{10.36}$$

One may easily show that

$$V^l U^k = \mathrm{e}^{2\pi\mathrm{i}kl/N} U^k V^l. \tag{10.37}$$

For two states, the operators $U$ and $V$ may be identified with the spin-$\frac{1}{2}$ variables $\sigma_1$ and $\sigma_2$, while in the limit of an infinite number of states, these become the translation operators in momentum and coordinate space, respectively.

In the fourth paper, 'Unitary transformations and the action principle' [97], submitted in August 1960, the quantum action principle was *derived* for systems with continuous degrees of freedom. Although Schwinger had introduced the quantum action principle nearly a decade earlier, for example in [73], there it was postulated rather than derived. 'Actually I did not derive the action principle restricted to time evolution. It's generalized to evolution of all these parameters [characterizing changes in reference frames]. So it could be a transformation from one reference frame to another at a different time with a spatial origin displaced and the orientation of the reference frame rotated or anything else which might be of interest. It is quite a general description, emphasizing the quite general kinematical and dynamical priority of the action principle.'[7]

The fifth paper, 'The special canonical group' [98], was, in Schwinger's phrase, 'quantum phase space.'[11] It obtained continuous position and momentum variables as the limit of a system containing only a finite number of degrees of freedom as remarked above. The transition amplitudes of the theory were expressed as phase-space integrals over classical trajectories, similar to Feynman's earlier path-integral formulation of quantum mechanics. However, although 'this formulation is closely related to the algorithms of Feynman[12], it differs from the latter in the absence of the ambiguity associated with non-commutative factors, but primarily in the measure that is used' [98].

'Quantum variables and the action principle' [102] began the process of examining the validity of the action principle for other types of variables, in particular, to spin-$\frac{1}{2}$ variables. In a footnote, Schwinger thanked Volkov for suggesting this idea: 'The possibility of using the components of an angular momentum vector as variables in an action principle was pointed out to me by G. Volkov during the 1959 Conference on High Energy Physics held in Kiev, USSR.' But Schwinger recognized that this was not sufficient; in order to convert 'the action principle into an effective computation device, [it is necessary] to extend the number system by adjoining an exterior or Grassmann algebra.'

Thus, the seventh paper, 'Exterior algebra and the action principle. I' [106] was characterized by the 'systematic use of Grassmann algebra.'[11] This 1962 paper extended the quantum action principle to fermionic degrees of freedom by introducing an exterior algebra, which are $N$ elements, $\epsilon_k$, $k = 1, \ldots, N$, that obey

$$\{\epsilon_k, \epsilon_l\} = 0, \tag{10.38}$$

where the braces signify an anticommutator, which implies that a single element satisfies $\epsilon_k^2 = 0$. Although reference was given to a mathematics book by Chevalley,[13] Schwinger developed the mathematics in the context of the physical problem, 'which is my philosophy. That's important because you want to see how it fits into the machinery that you want to use it for, not as something off the shelf and adapted to a different purpose. You need certain things a mathematician would not think of, such as what is the meaning of taking the complex conjugate of the totally anticommuting number? Who would think that it means to reverse the order of multiplication as though it were the adjoint? It was not possible to do other than go through this myself to develop it in the framework' of quantum mechanics.[7] In spite of its title, there was no second paper in the sequence. This, in fact, was the final entry in this PNAS series.

A summary of Schwinger's ideas on quantum kinematics was given many years later in a tribute to Hermann Weyl [208a], written at the time Schwinger was reanalyzing the basis of his view of quantum mechanics, the Stern–Gerlach experiment, and writing the *Humpty Dumpty* papers that we will discuss below.

## Angular momentum

In 1965 an important collection of papers on the theory of angular momentum was edited by Lawrence Biedenharn and Hendrik Van Dam, *Quantum theory of angular momentum.*[14] It contained a great many valuable papers on the subject, but it was outstanding because of two fundamental papers which had theretofore remained unpublished. In the words of the editors, 'We would feel inadequate to this task of selection were it not for the singular circumstance that two of the most important and fundamental papers in the field have never been properly published. These are: the famous unpublished manuscript of Wigner,[15] and the monograph of Schwinger on angular momentum [69]—each of which inspired further work despite the handicap of limited circulation. These works of Wigner and Schwinger, combined with Racah's classic papers[16] and that of Bargmann,[17] form the core of the present selection and enable us to be fully confident of the value of our collection.'[14]

As with most of Schwinger's research papers, the ideas were first presented in, and developed for, a course. Richard Arnowitt, who came to Harvard in 1948 and approached Schwinger in the spring of 1949 to be taken on as a research student, recalled the quantum mechanics course he took from Schwinger that first year. 'Schwinger was one of the great teachers of our time, as well as a great researcher. He lectured without notes, and in the quantum mechanics course derived the properties of angular momentum from creation and annihilation operators. Everything was worked out. In the lectures he derived the states from the bottom up [that is, from the state of lowest value of $J_z$]. At the end of the lecture, he reached in his pocket, pulled out his notes, and checked his result—but it was different because there he'd proceeded from the top down.'[8]

Years later, Schwinger described the origin of his monumental work, with a rather unnecessary worry about its date of completion. 'This work was actually done in 1951 and I think it was published [as an AEC report] probably on the second day of 1952. It always annoys me that one gets the impression that it was 1952. You know when you move it from '51 to '52 you move it a whole year. But it was actually done at the end of '51.

'It was done under the aegis of some private firm, I've forgotten what. I think I was doing some consulting for them. Why they allowed me to write this elaborate paper? But that was in response to a need for me to understand something. During the war Racah was working out these elaborate coefficients [for coupling angular momenta] using the most incredible algebra, totally opaque, and I wanted simply to understand in my own way what that was. So I went back to some work I had done in 1937. Rabi had this paper on nonadiabatic transitions[1] and I have a parallel paper [4]. And that is a method of dealing with angular momentum which is very simple in which any angular momentum is treated as a Bose–Einstein ensemble of spin-$\frac{1}{2}$. And then I simply did this using second quantization techniques, hardly a surprise, and redeveloped the whole theory of angular momentum and was able with the greatest of ease to get these Racah coefficients and a lot more besides. That was the content of this paper. I was doing it for my own education. As always, once you start on something you can't stop and it became more and more elaborate. But it remained unpublished although it was known to people in the field doing similar things. So I have been copiously quoted by people who worried about Racah coefficients; then it finally got incorporated into that book [69].'[7]

Schwinger's 50-page paper illustrates his lifelong belief in the unity of physics and mathematics. More properly, he always felt that the mathematics should emerge from the physics, not the other way around, a reflection of his profound phenomenological bent. The paper begins with the following paragraph. 'One of the methods of treating a general angular momentum in quantum mechanics is to regard it as the superposition of a number of elementary "spins," or angular momenta with $j = \frac{1}{2}$. Such a spin assembly, considered as a Bose–Einstein system, can be usefully discussed by the method of second quantization. We shall see that this procedure unites the compact symbolism of the group theoretical approach with the explicit operator techniques of quantum mechanics.' [69]

He introduced creation and annihilation operators associated with these spin-$\frac{1}{2}$ states, $a_\zeta^\dagger = (a_+^\dagger, a_-^\dagger)$ and $a_\zeta = (a_+, a_-)$. These satisfy the usual commutation rules,

$$[a_\zeta, a_{\zeta'}] = 0, \quad [a_\zeta^\dagger, a_{\zeta'}^\dagger] = 0, \quad [a_\zeta, a_{\zeta'}^\dagger] = \delta_{\zeta,\zeta'}. \tag{10.39}$$

The angular momentum operator is then written in terms of these boson creation and annihilation operators as follows:

$$\mathrm{J} = \sum_{\zeta,\zeta'} a_\zeta^\dagger (\zeta \,|\, \tfrac{1}{2}\sigma \,|\, \zeta') a_{\zeta'}. \tag{10.40}$$

Of course, this construction guarantees that J satisfies the correct angular momentum commutation relations,

$$[J_i, J_j] = \mathrm{i}\epsilon_{ijk} J_k. \tag{10.41}$$

The angular momentum eigenstates, in which $\mathrm{J}^2$ and $J_z$ have the eigenvalues $j(j+1)$ and $m$, respectively, may be explicitly constructed from the creation operators,

$$\Psi(jm) = \frac{(a_+^\dagger)^{j+m}(a_-^\dagger)^{j-m}}{[(j+m)!(j-m)!]^{1/2}}\Psi_0, \tag{10.42}$$

where $\Psi$ is the no-boson state, that is, is annihilated by the operator $a_\zeta$, $a_\pm\Psi_0 = 0$.

Schwinger then considered rotations, and developed the properties of matrix elements of the rotation operator,

$$U = \mathrm{e}^{-\mathrm{i}\phi J_3}\mathrm{e}^{-\mathrm{i}\theta J_2}\mathrm{e}^{-\mathrm{i}\psi J_3}, \tag{10.43}$$

which is given in terms of the three Euler angles $\phi, \theta, \psi$. The coupling of angular momentum was considered, which led to a coefficient identical to Wigner's 3-$j$ symbol. The coupling of three and four angular momenta led, as Schwinger promised, to the famous Racah coefficients. The final section of the paper dealt with tensor operators, vital to the description of atomic and nuclear systems.

This paper was a remarkable virtuoso display of Schwinger's brilliance. Most of the results were known already by other means; but here they were derived in an elegant powerful formalism, which could readily be applied to other situations. It is no wonder that those who heard the lectures of Schwinger on this or other topics had an advantage over others, especially as the material was 'never properly published.'

In the decade and a half after the war Schwinger wrote two other shorter papers on angular momentum theory, neither of which was published at the time, or even circulated as a report as was [69]. They appeared only in 1977, when Schwinger put them together, with some introductory notes, as his contribution to the Rabi *Festschrift* [184]. The subject of both was the 'Majorana formula.'[18] Once again it was his desire to understand a result from a physical rather than a mathematical point of view. He first made such an attempt in this connection in 1937 in [4], but 'what was thus left implicit [the construction of angular momentum $j$ from $2j$ spin-$\frac{1}{2}$ systems] was actually the most important result in this work' [184]. In 1945 Schwinger had begun writing a paper, entitled 'A note on group theory and quantum mechanics,' the purpose of which was to argue his view that 'the mathematical methods of group theory are too general for the purposes of quantum mechanics, that quantum mechanics is adequate to describe, within its own framework, those symmetry operations that arise from physical considerations.' In a few pages, Schwinger derived Majorana's result, which described 'how the behavior of an arbitrary magnetic moment in a time varying magnetic field is related to that of a spin-$\frac{1}{2}$ system' [184]. Unfortunately, for some reason, this paper was never completed, and it appeared in [184] in its uncompleted state.

Five years later Schwinger did complete his monograph on angular momentum [69], but this was not the last word on the subject. 'A publication in 1958 by A. Meckler[19] made me aware, to my chagrin, that some aspects of the Majorana formula had not been brought out previously, although they were implicit in the 1951 monograph' [184]. These had to do with the relation between Legendre polynomial operators and Chebyshev polynomials. Schwinger wrote a short note on this, and submitted it to *Physical Review* in the fall of 1959. 'That paper was rejected by Editor S. Goudsmit for reasons that I then found so incomprehensible that I cannot now recall them' [184].

## Students' perspectives

At this period, around 1950, Schwinger had 10 to 15 research students. He invariably lectured on Monday, Wednesday, and Friday, nominally from 12:00 to 1:15, followed by lunch sometimes with Herman Feshbach. After lunch he was available to see students (usually just one day a week, typically Wednesday). Arnowitt recalled that he would 'see Julian once a month. I would do everything I possibly could until I was completely stuck. [When I'd see him] he'd think for a few seconds, then rattle off five things I'd never thought of. For me it was one of the great learning experiences of my life.

'Julian did many things he was not given credit for. For example, the [so-called] Bethe–Salpeter equation.[20] He worked it out by a variational derivative technique—that's the only way to derive it. The Gell-Mann–Low derivation[21]* is not convincing—you cannot organize the diagrams unless you know the answer. Further, you must assume the energy states are real.'[8]

Of course, once out of Schwinger's immediate orbit, it was not easy to get a response from the great man. 'The first paper Stanley Deser and I wrote I gave to Julian for comments.[22] He never returned it. Recently, Clarice returned the paper to Stanley' after a lapse of 45 years![†] It was even hard to get a response for a thesis. 'I gave Julian my thesis [on hyperfine structure of hydrogen, recoil effects]—no response. I wrote a letter, is it okay for me to publish it? Then I wrote another letter. By this time, Salpeter had a student who had published similar work. "If I don't hear from you in a month, I will submit it to *Physical Review*."' He didn't and he did.[8, 24]

Another of Schwinger's students at the time was Margaret Kivelson, who had been an undergraduate at Harvard, so when she became a graduate student in 1950 she never thought of working for anyone else. 'It never occurred to me that Julian wouldn't take me.'[25] She started work on the Bethe–Heitler problem, but

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* As we recall from the previous chapter, Gell-Mann and Low remarked upon and acknowledged the priority of Schwinger's lectures.

† Deser recalled finding it among Schwinger's papers after his death.[23]

had to give it up, nearly giving up physics in the process, because someone else was threatened. But she then found a good thesis problem. 'Julian realized that you could do the calculation of Bremsstrahlung in the extreme relativistic limit. Instead of doing a power series in the coupling constant, the fine structure constant, if you looked at the extreme relativistic limit and recognized that Bremsstrahlung became more and more focused at small angles you could solve the problem to all orders in the coupling constant if you expanded to lowest orders in the angle of scattering. And it was a very elegant idea and it was very much in keeping with a lot of things that were happening at that time, where people were finding new ways to do calculations, avoiding the expansion in the coupling constant by finding something else that was small. And I did use his functional derivatives in the analysis and so it was not only a new problem but also a new technique being used for the problem. I think it was a really important paper, the only problem with it was that Julian never noticed that I didn't publish it. And so I wrote my thesis, I took my oral, I wrote 60 pages of equations, and was very worried that I might have errors. But nobody found any, and after they passed me on my oral, I went up to the library and pulled out the latest issue of *Phys. Rev.* and found a paper by Bethe, Maximon, and Davies[26] in which they had done the same calculation in a totally different way and come up with the same answer. I was just crushed. I thought the result was out and that meant that I shouldn't publish my thesis. So I never published. I should have. Years later, decades later, I told Julian that I'd never published it because it had come out, the result was there, and even though it was a different method I didn't think—in those days I was young and innocent—I didn't understand that an independent calculation done by a different method, by an innovative method, had its interest, and I have no doubt it would have been published. But Julian, he was so busy doing his thing, he never paid attention to it—the fact that I hadn't published it—he was absolutely shocked. For years, people who were in graduate school with me and knew what I had done would refer to my thesis, but, of course, the rest of the world didn't know about it.'

Kivelson's contacts with Schwinger were even less frequent that Arnowitt's. 'I would see Julian, he would give me all the time in the world, I would go away, and whatever he had talked to me about would be enough to keep me busy for about three months. So that I didn't even try to see him for several months, because I'd be working through all the things that were inspired by our conversation. Then I would get in line behind the other 12 graduate students. It would take another three months to get back, so I have a feeling I saw him twice a year outside the classroom.... We always joked that Julian only existed on Monday, Wednesday, and Friday. Monday was a faculty meeting, and Friday he went home after lecture, so everybody saw him Wednesday afternoon, after he got back from a long late lunch.'[25]

The question arises: why did Schwinger have so many successful students, while Feynman had essentially none? Arnowitt recalled, 'Whenever I talked to Feynman, I got a headache. It was a total strain to keep up. [In contrast] Julian was always very modest, quiet, soft-spoken. There was no one-upmanship in conversation. Feynman was much more active in conversation. Feynman went through phases where he was not productive. Then he'd do something great, for example, $V - A$. He had a feeling of inferiority from time to time. Julian always was working, had problems for students, as part of a whole program of research. Julian was hard to work with if you wanted guidance, but easy to work with if you wanted inspiration.'[8] Evidently, for many at Harvard, the latter was the case.

## Potential problems and quantum oscillators

In 1960 Schwinger took up skiing. The Schwingers went on holiday to a third-rate hotel in New Hampshire. In this area people have houses in Tyrolean style, which are rented out furnished in the ski season. They went just to have a break. But on the afternoon they arrived they went to the ski school and the shop. For some reason Julian signed them up for ski lessons, at 10 o'clock in the morning, when they normally did not get up until 11. They both took lessons and Julian became enamored of skiing, a passion he retained all his life.[6]

Julian could not do anything unless he really understood what the purpose was. He found that the ski instructors often pushed him ahead much faster than he thought appropriate, so Julian became more and more miserable. He stopped taking lessons. He would go on the ski slope and practice by himself. Clarice described looking at the slope and seeing a solitary figure poised diagonally, who was Julian thinking about what he had done, what he should do, and then he would do it again. Through this self-analysis he became a reasonably good skier and he enjoyed it tremendously.[6]

That winter, on 29 November 1960, Schwinger communicated 'On the bound states of a given potential' to the *Proceedings of the National Academy of Sciences* [100]. The point was 'demystifying mathematics';[11] in this case the result of Bargmann,[27] that there are only a finite number of bound states belonging to a spherical potential $V(r)$ for which $\int_0^\infty dr\, r\,|V(r)|$ exists,* was the issue that sparked Schwinger's investigation. Schwinger used variational methods to obtain a more general bound. For example, for a non-spherical potential, the number of states that have energy less than or equal to a number $-\kappa^2$ is

* This integral does not exist for the Coulomb potential, $V = 1/r$, for which an infinite number of bound states occur.

bounded by

$$N(E \leq -\kappa^2) < \frac{1}{(4\pi)^2} \int (\mathrm{dr})(\mathrm{dr}') \frac{|V(\mathrm{r}) + \kappa^2||V(\mathrm{r}') + \kappa^2|}{|\mathrm{r} - \mathrm{r}'|^2} \bigg|_{V+\kappa^2<0}, \tag{10.44}$$

and, for states of given orbital angular momentum $l$ in a spherically symmetric potential, the number of states with energy no greater than $-\kappa^2$ is

$$n_l(E \leq -\kappa^2) < \frac{1}{2l+1} \int_0^\infty dr\, r\, |V(r) + \kappa^2| \bigg|_{V+\kappa^2<0}, \tag{10.45}$$

which reduces to Bargmann's result[27] for $\kappa = 0$.

Schwinger concluded this paper with an application to the tensor force, that spin-dependent nuclear potential which he had spent so much time studying in the 1930s, (cf. Eqns (3.3) and (3.4))

$$V(\mathrm{r}) = V_a(r) + V_b(r) S_{12}, \tag{10.46}$$

$$S_{12} = 3\frac{(\sigma_1 \cdot \mathrm{r})(\sigma_2 \cdot \mathrm{r})}{r^2} - \sigma_1 \cdot \sigma_2. \tag{10.47}$$

As Schwinger noted, 'in an application to a physical system, such as the deuteron, for which the distribution of energy values is known, these inequalities provide simple bounds on the potential used to represent the data.'

A much more elaborate and important paper, 'Brownian motion of a quantum oscillator' [101], had been received by the *Journal of Mathematical Physics* the day before. Undoubtedly, Schwinger was motivated to pursue this investigation by the excitement of the discovery of the maser (microwave amplification through stimulated emission of radiation), the radio-wave precursor of the laser (light amplification through stimulated emission of radiation). In fact, Schwinger held a contract with the Signal Corps for three years starting in 1954 to investigate the 'high frequency limit of millimeter microwave generation.' The third semi-annual report on this contract work in 1956 claims 240 hours spent on the project, which seems entirely on the subject treated in [101], dealing with the quantum limit through coherent states, and so on.[4]

As Paul Martin noted, this 'quantum oscillator paper was the basis for what Keldysh and others did later.'[28,*] Pradip Bakshi, a student at the time (1960),

* L. V. Keldysh, *Soviet Physics JETP* 20, 1018 (1965) [*Zh. Eksp. Teor. Fiz.* 47, 1515 (1964)] obtained a Feynman diagram technique for calculating Green's functions for nonequilibrium systems, driven by an external field, the same problem Schwinger considered. Interestingly, Keldysh in this paper referred to the Martin–Schwinger equilibrium paper [89], but not to Schwinger's nonequilibrium paper [101].

who, with Kalyana Mahanthappa, exploited the formalism of this paper concurred: 'Keldysh took only a part of this closed-time formalism. It contains unitarity and completeness, not just causality. This very vital and central paper contains angles still unexplored.'[29] Bakshi recalled an intimate contact with this paper. While he was still searching for a thesis problem, he attended the lectures Schwinger was giving at the 1960 Brandeis Summer Institute for Theoretical Physics. Schwinger suggested he drop by his house on the way back. There 'he handed me a yellow pad, saying bring it back, I'll probably need it [in the] next lecture.' It turned out to be the manuscript for [101]. Bakshi spent all night copying out this precious document.[29]

Schwinger eloquently expressed the philosophy of this paper. 'Underlying this is a fundamental development of the quantum action principle which has, up to this point, always referred to the evolution from one time to a later time. Of course, it could be done either way, but it was always moving unidirectionally in time. Now I have in mind an application to problems in which one is not so much interested in the specification of initial states and final states but in a situation complicated enough that you don't have a definite final state, but you are interested in just the average values of certain physical properties, which is a very common occurrence. You start the system off and then you make a measurement, not in detail, but just of one physical property which therefore has contributions to it from all possible states. The obvious way to do it is the straightforward one of a summation over all possible outcomes, multiplied by probabilities for those outcomes which the time evolution function would give you. And I could not help asking myself could I find a technique that would go directly to what was wanted and not do it piecemeal by using much more information than was necessary. And I realized that a way of doing it was to follow the evolution of the system from an initial state forward in time and then back to the initial state, thereby in a sense coming much closer to the classical idea.[7]

'This really comes to the heart of the difference between Feynman and myself at the very beginning. I had in mind, if you like, classical ideas or the questions to which I'm returning here. You start the system off initially and let it evolve, and the discussion of everything should be in terms of propagation functions that are causal in the sense that nothing happens until something has excited the system. In classical electrodynamics you talk about retarded Green's functions. Retarded Green's functions are so natural classically. You start the system off and then the wave propagates and it doesn't get beyond as far as it can go with the speed of light. Being so influenced by electrodynamics, that was what I adopted in the 1947–48 paper. Feynman's approach—very interesting development really—as Dyson recognized, was to use a mixed description, as he put it. You might say physically what I had done and what everybody had done up to this point was to describe things in terms of how you prepare the state, how you selected it, at a certain initial time. The propagator carried the system forward in time.

Feynman had looked at something different, a choice of initial state and a choice of final state. Not a question you would ever ask classically, but one that comes more and more to the fore quantum-mechanically where with small numbers of degrees of freedom you can indeed pick out a definite final state. And the action principle, as I evolved it, initially was always expressed in that language.[7]

'But now I'm coming to questions concerned with systems that are complicated enough that it is not easy to pick a final state. The measurements that you make are rather averages over all possible final states and so now I was asking the natural question. Very well, here's the action principle. Can it not be used for the direct evaluation of what is the quantity of physical interest without having to work out an infinity of probabilities when you want only one number? And now we are coming back to what in fact was, if you like, my classical starting point, that the input is the initial preparation of the state and you don't know anything about any definite final state. It does what it does. It goes into all possible final states and you don't make a selection. Rather, you measure one or many physical properties and the question was could one use the action principle to produce that single number. The answer was yes. And the technique of doing it was really rather nice. You consider a system that evolves forward in time under the action of one Hamiltonian and backwards under the action of a different Hamiltonian, because if the Hamiltonians were the same, it would come back to the same initial state. You get new information by changing the Hamiltonian in the course of time. And you change it, of course, by a multiple of the physical property you're interested in. You have an arbitrary parameter that multiplies the physical property you're interested in. You let that parameter change by a sort of step function in time so it's present only at the final time and the net evolution of the system tells you the expectation value of that single physical property of interest at this final time in terms of how the system was set on its path and what its dynamics are and so forth. The whole idea is that by working out a time transformation function of the time cycle you get what you want. Whereas, the other way of doing it involves using time transformation functions forward to definite states, squaring them to get probabilities, and adding them all up. The thing one thinks of immediately when somebody asks what's an expectation value. A lot of work. Now here is a simplification in which a linear answer is being given for what is ordinarily thought of as an absolute squared quantity. You'd have to take a probability amplitude, square it to get probabilities. Here, I'm getting the information I want from a probability amplitude. Nice trick.[7]

'There are many interesting ideas here and I'm sorry I didn't further develop this line of thought and left it for others to do. I would also like to point out, just for historical interest, that in the discussion of the oscillator extensive use is made throughout all this of the non-Hermitian operators and their eigenvectors and eigenvalues, which my good friend and colleague Roy Glauber[30] will claim as his own later.'[7] What Schwinger was referring to here was the notion of 'coherent states,' which are eigenvectors of the creation and annihilation operators. These states were in fact first introduced by Schwinger in 1951 in 'The theory of quantized fields. III' [74], as we mentioned in the previous chapter, and discussed in detail in the Les Houches lectures in 1955.

Consider the harmonic oscillator Hamiltonian

$$H = \frac{\omega}{2}(p^2 + q^2). \tag{10.48}$$

The creation and annihilation operators, which Schwinger usually called $y^\dagger$ and $y$, are constructed according to

$$y = \frac{q + \mathrm{i}p}{\sqrt{2}}, \quad y^\dagger = \frac{q - \mathrm{i}p}{\sqrt{2}}, \tag{10.49}$$

which satisfy

$$[y, y^\dagger] = 1. \tag{10.50}$$

Hence the equations of motion for these non-Hermitian variables are

$$\mathrm{i}\frac{\mathrm{d}y}{\mathrm{d}t} = \omega y, \tag{10.51}$$

$$-\mathrm{i}\frac{\mathrm{d}y^\dagger}{\mathrm{d}t} = \omega y^\dagger. \tag{10.52}$$

Even though $y$ and $y^\dagger$ are not Hermitian, they possess right and left eigenvectors, respectively,

$$y|y'\rangle = y'|y'\rangle, \quad \langle y''^*|y^\dagger = \langle y''^*|y''^*. \tag{10.53}$$

(As usual in Schwinger's notation, primes on quantities denote eigenvalues.) These eigenstates can be constructed from the ground state, or from the $n$th excited energy eigenstates,*

$$|y'\rangle = \sum_{n=0}^{\infty} \frac{y'^n (y^\dagger)^n}{n!}|0\rangle = \sum_{n=0}^{\infty} \frac{(y')^n}{\sqrt{n!}}|n\rangle. \tag{10.54}$$

Because it is then easily seen that

$$\langle y'^*|y''\rangle = \sum_{n=0}^{\infty} \frac{(y'^* y'')^n}{n!} = \mathrm{e}^{y'^* y''}, \tag{10.55}$$

in particular that

$$\langle y'^*|y'\rangle = \mathrm{e}^{|y'|^2}, \tag{10.56}$$

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* A similar construction was written down by Schrödinger in 1926, at the very beginning of quantum mechanics.[31]

we see that $|y'\rangle$ is not a normalized state. Let us normalize it, and define

$$|QP\rangle = |y'\rangle e^{-\frac{1}{2}|y'|^2} \tag{10.57}$$

where $y' = (Q + \mathrm{i}P)/\sqrt{2}$. These states are not orthogonal, but nearly so,

$$\langle QP \| Q'P' \rangle = e^{-\frac{1}{4}(Q-Q')^2} e^{-\frac{1}{4}(P-P')^2} e^{\frac{\mathrm{i}}{2}(PQ'-QP')}. \tag{10.58}$$

These are the *coherent states*. They have been given that name because they act most like the classical states. The expectation values of $q$ and $p$ are simply $Q$ and $P$:

$$\langle QP|q|QP\rangle = \langle QP|\frac{1}{\sqrt{2}}(y + y^\dagger)|QP\rangle = \frac{1}{\sqrt{2}}(y' + y'^*) = Q, \tag{10.59}$$

$$\begin{aligned}\langle QP|p|QP\rangle &= \langle QP|\left(-\frac{\mathrm{i}}{\sqrt{2}}(y - y^\dagger)\right)|QP\rangle \\ &= -\frac{\mathrm{i}}{\sqrt{2}}(y' - y'^*) = P.\end{aligned} \tag{10.60}$$

A coherent state is a minimum uncertainty state, with both $q$ and $p$ having equal mean square fluctuation:

$$\langle (p - P)^2\rangle = \langle (q - Q)^2\rangle = \frac{1}{2}, \tag{10.61}$$

or, in the language of the uncertainty principle,

$$\Delta q \Delta p = \frac{\hbar}{2}, \tag{10.62}$$

where we have restored physical units. These states continue to play crucial roles in many fields of physics, and their study is the subject of entire conferences.

A classic example of the continual interplay between research and teaching is the paper 'Coulomb Green's function' [116]. Although it was published in 1965, as he stated in his comments in the *Selected papers*,[11] it is 'too bad it wasn't published in the 40s.' In fact, the paper notes that 'It was worked out to present at a Harvard graduate course given in the late 1940s. I have been stimulated to rescue it from the quiet death of lecture notes by recent publications in this Journal [the *Journal of Mathematical Physics*], which give alternative forms of the Green's functions.'[32]

Schwinger's derivation exploited the four-dimensional Euclidean rotational symmetry of the non-relativistic Coulomb problem, which symmetry resulted from the conservation of angular momentum, and of the Runge–Lenz vector. Thus Green's function in momentum space could be expressed in terms of four-dimensional spherical harmonics. After obtaining an explicit formula for the Coulomb Green's function, he obtained from it the corresponding scattering amplitude, including the well-known Coulomb phase shift.

## 'Is spin coherence like Humpty Dumpty?'

In the early 1980s Schwinger had a German postdoc, Berthold-Georg Englert, who collaborated extensively with him on a major series of papers on the Thomas–Fermi statistical model of atoms. We will describe this collaboration in detail in Chapter 15. In 1985 Englert returned to Munich, but their friendship continued, and Schwinger visited Englert periodically. On one of these visits, the two of them were sitting in the office of Marlan Scully in the Max Planck-Institut für Quantenoptik in Garching, Munich.* Scully was describing to them how it might be possible to reunite two atomic beams which had been separated by a Stern–Gerlach apparatus. This was Schwinger's turf, and he objected strenuously to Scully's presentation, 'pointing out it's not so easy to reunite beams.'[33] Thus was born a three-way collaboration that led to four papers and numerous presentations. The primary papers were called 'Is spin coherence like Humpty Dumpty?', a reference to the nursery rhyme in which all the king's horses and all the king's men could not put Humpty together again.

'Scully came up with a calculation which looked as if he could [reunite the beams] and then finally we got together and figured out what were the actual limits. If you assume that you have four perfectly identical Stern–Gerlach magnets and you are allowed to linearize the problem, then, of course, you can reunite the beams perfectly. The question is, to what extent do they have to be identical? And it turns out, if you take into account that the magnetic field has to obey Maxwell's equations and all that, then you really cannot reunite them, and even to get some coherent effect in the spin degree of freedom you would have to control the macroscopic apparatus with microscopic precision, which is impossible.'[33]

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* Willis Lamb recalled a side trip during one of Schwinger's visits to Garching: 'During my stay at Ulm, Schwinger received the Humboldt Award, which brought him to the Institute for Optical Sciences at Garching, near Munich. [Actually, Schwinger received the Humboldt award in 1981; this was a subsequent trip on the same award, in the late 1980s.] During his stay there, he was invited to the University of Ulm to give a talk in the morning which, as apparent to all those who knew Schwinger, was not easy for him, but he did come. When he arrived in the Department of Physics, he found all offices closed, except one (which was mine); he entered and sat there alone, without trying to look for anyone. When I learned that Julian was in my office, I rushed to it, and we talked. He gave his talk [which was described on p. 339], and afterwards he was invited to lunch, but he plain forgot about it and left for his hotel to join Clarice.

They stayed at the Stern Hotel, the best hotel in Ulm, but they had some difficulty with the Registration Clerk, who insisted that they could have the room only for that night, but the next day they must leave, as the rooms were to be painted. Now, this could happen to anybody anywhere, but that it happened to Julian was noteworthy; he and Clarice left Ulm the next day rather than move to another hotel.' [Telephone interview of Willis E. Lamb, Jr. by Jagdish Mehra, 12 March 2000].

The first paper was 'a simplified calculation, where you treat the longitudinal motion through the magnetic field classically so that the spatial dependence of the field is translated into a time dependence. This makes it easy to solve Heisenberg's equations of motion in the linearized regime, but it's good enough to answer the question of how precisely you have to control the apparatus.'[33] It was contributed to the *Festschrift* for David Bohm [208]. The second paper [209] mainly reflected Schwinger's work, 'to refine the calculation, get rid of the approximations.' This was done by using his harmonic oscillator representation for spin (10.40). 'It was a very nice piece of work, because you could isolate the average motion and look at small oscillations around them. To incorporate the effects that come from Maxwell's equation when you go from one magnet to the other, you have to match the fields. And you could study the effects of the fringing fields. It became clear that even if you allow yourself perfect control of the magnets there are still Maxwell's equations which prevent you from perfectly reuniting the beams.'[33] It was submitted to *Zeitschrift für Physik, in memoriam* to Otto Stern on the 100th anniversary of his birth, and also reprinted in the *Proceedings* of the Eleventh International Conference on Atomic Physics held in Paris, a meeting in honor of Rabi. According to Englert, 'These papers were rather important, because they provided the first careful detailed calculation of atomic interferometers, and now we have all kinds of atomic interferometers all over the world.'[33] The third paper [210] showed that observation destroys the interference fringes. 'Part of the Humpty-Dumpty business was if you've managed to separate the beams and you now start to watch which way the atom takes through the interferometer, how will this acquisition of information destroy the fringes? It's the old story of the double slit, which way did the particle go? Now here we had the means, in principle, if you take all these things for granted and you think you've reunited the beams with at least some precision so you get some coherent effect, you have split the beam so much that you can look at one of them at a time. It was mainly Scully's invention to use microwave resonators, a field of radiation so that an atom going along one way could absorb a photon from one resonator and then emit it in the second and end up, more or less, in the same state, and at the same time leave a trace which way was taken. The analysis showed that it is not the perturbation of the center-of-mass motion that washes out the fringes but solely the entanglement of the spin degrees of freedom with the detector degrees of freedom. This already was a different story that no longer involved Julian very much.'[33]

After these papers appeared in 1988, a couple of conference presentations followed by Scully and Englert. They talked about 'Center of Mass Motion of Masing Atoms,' at the NATO Advanced Study Institute in Istanbul in 1989, which Asim Barut organized, with Schwinger, Englert, and Scully as co-authors. 'That was a paper about the center-of-mass motion of atoms where we showed that the atom going through the resonator, emitting a photon, absorbing a photon, is not significantly perturbed in its center-of-mass motion. Photons in a resonator do not have mode functions which are eigenfunctions of momentum. You can arrange things so that the atom does not really suffer any significant recoil. That had a continuation, because usually if you send atoms through a resonator the kinetic energy is very large compared to the interaction energy so you don't have to worry about back scattering, for example. However, if you would use very slow atoms, then you could think of reflecting half the atoms when they come to the resonator because one component of the wavefunction would experience a repulsive interaction, and the other component an attractive interaction. Half the intensity would go through and half would be reflected. That was written up in *Europhysics Letters* [218] by me, Julian, Scully, and also Barut, for reasons I no longer recall. I don't remember that Barut contributed anything. It must have been an outgrowth of the fact that Scully was too lazy to do the calculation and he asked Barut and Barut scribbled something on a piece of paper. Julian was a little upset about that because he really didn't want to have Barut's name on one of his papers.'[33,*]

The papers were submitted from Garching because that was Scully's home base, and Englert had an office there as well. 'The Max Planck Institute kind of invited Julian on these occasions. There also were a few years when Scully was running the Center for Advanced Studies at the University of New Mexico in Albuquerque, and he used to invite me and Julian regularly.'[33]

At the same time Schwinger gave his own lectures on atomic interferometry; we will discuss this idea, which Schwinger called MAGIC, for Magnetic Atoms for a Gyroscopic Interferometric Counter, briefly in Chapter 15. Schwinger began one of these lectures with the plea, 'Incidentally, I know that I ask the impossible, but please try to divest your mind of all accumulated lore and return to a state of virginal innocence.'[4]

Another lecture, entitled 'Snowbird,' exists in part in the UCLA archive.[4] It provides an overview of this program. Perhaps it is appropriate to conclude this chapter by quoting the opening paragraphs, which explain Schwinger's attitude toward quantum mechanics throughout his life. 'To me, the formalism of quantum mechanics is not just mathematics; rather it is a symbolic account of the realities of atomic measurement. That being so, no independent quantum theory of measurement is required—it is part and parcel of the formalism.

'This is not a universally held opinion, however. I quote from one recent paper: "Ordinary quantum mechanics is based on two distinct principles of evolution of the wavefunction. The first principle, to be applied in ordinary

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* Another paper by Scully, H. Walther, Englert and Schwinger, 'Observation and Complementarity in Quantum Mechanics—New Tests and Insights' was prepared, but apparently never published, at least with Schwinger's name.[4]

situations, is expressed by the Schrödinger equation, which provides a deterministic evolution of the wavefunction. The second principle, to be applied when a measurement takes place, is the reduction postulate, according to which the wavefunction undergoes a sudden stochastic evolution."

'Stepping aside for a moment, let me remark that, apart from a reversal of the numbering, and the replacement of "causal" by "deterministic," this is just the dichotomy introduced, long ago, into quantum mechanics by von Neumann.[34]'

'The cited paper continues with: "Problems arise when one tries to describe the measurement process, i.e., when one considers the measuring apparatus not as a separate object but together with the measured system, as part of a larger physical system."

'The authors then point to a recent attempt to avoid the alleged problems: "This theory (quantum mechanics with spontaneous localization) accepts a stochastic modification of the Schrödinger equation consisting in the assumption that each constituent particle of any physical system is subject at random times to a random localization process. ... Quantum mechanics with spontaneous localization leads to a natural solution of the problem of quantum measurement."

'In my opinion this is a desperate attempt to solve a non-existent problem, one that flows from a false premise, namely the von Neumann dichotomization of quantum mechanics. Surely physicists can agree that a microscopic measurement is a physical process, to be described as would any physical process, that is distinguished only by the effective irreversibility produced by amplification to the macroscopic level. Perhaps what has been lacking is a detailed analysis of the dynamics involved in some realistic measurements.' Schwinger went on to describe his work with Scully and Englert on the analysis of the Stern–Gerlach magnetic moment experiment.[4]

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# Custodian of quantum field theory

Schwinger had now scaled the peak of quantum electrodynamics, not once, but three times, the last time by inventing a new approach to any quantum-mechanical system, the quantum dynamical principle. Feynman's route up the same mountain, using what has become the enormously fruitful path-integral technique, was, as Schwinger noted much later, what amounted to a solution of the functional differential equations of Schwinger's action principle.*

Now the task of the field theorist, as was already apparent in the 1930s, was to build upon this success of QED and apply the powerful machinery invented to understand the strong and weak nuclear interactions. But this was to prove nearly impossible. The theory of neutrons, protons, and pions, which at first glance did not appear so very different from that of electrons, positrons, and

* 'During the 25 year period of quantum electrodynamical development, there was great formal progress in the manner of presenting the laws of quantum mechanics, all of which had its inspiration in a paper of Dirac. This paper[1] discussed for the first time the significance of the Lagrangian in quantum mechanics. I have always been puzzled that it took so long to do this, but a faint glimmering of the reason appeared when I reread this paper recently and noticed that even Dirac himself thought that the action principle required the use of coordinates and velocities rather than coordinates and momenta, despite the existence of the classical action expression

$$W = \int_{t_2}^{t_1} \mathrm{d}t \left[ \sum_k p_k \frac{\mathrm{d}q_k}{\mathrm{d}t} - H(p, q) \right].$$

(Incidentally, this same hang-up seems to persist in recent articles claiming that the quantum action principle is inapplicable to curved spaces.) Eventually, these ideas led to Lagrangian or action formulations of quantum mechanics, appearing in two distinct but related forms, which I distinguish as differential and integral. The latter, spearheaded by Feynman, has had all the press coverage, but I continue to believe that the differential viewpoint is more general, more elegant, more useful, and more tied to the historical line of development as the quantum transcription of Hamilton's action principle.... It continues to surprise me that so many people seem to accept [the path-integral solution to the differential equation for the vacuum persistence amplitude] as a satisfactory *starting* point of a theory.' [160]

photons, did not seem to yield a self-consistent theory in which one could calculate meaningful numbers that could be compared to experiment. And it soon became much worse. First in cosmic rays, and then in the new accelerators which were built in the late 1940s and early 1950s (the synchrocyclotrons at Berkeley and Chicago, then the Cosmotron at Brookhaven, and the Bevatron at Berkeley) discovered a plethora of new particles. Some were interpretable as some sort of excited state of the neutron and proton, like the $\Delta^{++}$ resonance (a 'resonance' because it had a short lifetime ($\sim 10^{-23}$ s), and therefore had a large uncertainty or spread in its mass ($\sim$ 100 MeV)), while others carried completely new properties or quantum numbers, like the V particles, with a quantum number that would ultimately be called strangeness. By the end of the 1960s, hundreds of these 'resonances' were discovered, and the catalog of their properties grew into the several hundred page bi-ennial compendium called the Particle data book.[2]

Surely all these states could not be fundamental. And yet there was no theory that treated them as excitations in the same way that the states of an atom were described as different configurations of electrons. In the eyes of many, if not most, theorists by 1960, field theory was impotent in dealing with strongly interacting particles. And the weak interactions were not much better off. Although, as we will discuss in the next chapter, the glimmering of a theory was coming into being, it was not possible to do serious calculations, because infinities kept appearing which could not be removed by the renormalization prescriptions that worked so well in QED. Reaction to this crisis was twofold: either one developed effective techniques that tried to connect basic physical principles, such as causality, to the experimental situation, or one retreated to a democratic, rather anarchic, point of view, in which all particles were equally fundamental, and the whole was more than the sum of its parts. The first, pragmatic, approach was that which underlay dispersion relations[3] and current algebra,[4] while the second was that of the bootstrap hypothesis.[5] Actually, these two approaches were closely allied, for the notion of the analytic scattering matrix was common. But the utility of the local field was to be jettisoned. As Kenneth Johnson summarized the situation, in those days the $S$-matrix was king, and field theory was thought to apply only to the leptons.[6]

Although it would be too strong to say that field theory was forgotten by the early 1960s, since, after all, the famous textbook by Bjorken and Drell appeared in 1965,[7] most practitioners were not employing it seriously by that point, and most felt that totally new fundamental concepts would be necessary to understand the phenomena discovered by the new accelerators. The chief exception was Julian Schwinger and his school.* 'I think I was the only person

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* In Europe the principal holdouts were Werner Heisenberg and his followers.

through the fifties and sixties who kept up the belief in field theory.'[8] But he was somewhat frustrated by the reaction. 'I think what bothered me mostly throughout all this was the intolerance with which my own attempt to use field theory were regarded. I was tolerant of them, but it was not reciprocated.'[8] Unlike Feynman, who left QED as a completed subject in 1950, and turned to problems such as superfluidity, Schwinger continued to perfect quantum field theory throughout the 1950s and 1960s. His many students also kept up the good fight. This dogged determination to play out the hand, what we might call Schwinger's conservatism, was to be vindicated in the end, when in the early 1970s it became apparent after all that local quantum field theory was the appropriate language to describe not only electrodynamics, but weak and strong forces, and the particles that interact via those forces, as well. It is the purpose of this chapter to describe the story of Julian Schwinger's work in the central period between the field theory revolutions of the late 1940s and the early 1970s, roughly during the period 1957 through 1965.

## Phenomenological field theory

It was sometimes said of Schwinger, in the 1970s and 1980s, that he was a field theorist and should stay away from phenomenology.* This was intended as a disparagement of the profoundly phenomenologically based source theory he developed from 1965 on, and the multitude of phenomena to which he brought it to bear, from high-energy scattering of electrons off nuclei to the structure of weak interactions. (We will explore the source theory experience in Chapters 13 and 14.) But this comment reflects ignorance of Schwinger's history and development. His earliest papers on nuclear physics, which we discussed in Chapter 2, demonstrate that he was a masterful phenomenologist. He was opposed to field theory as a mathematical subject, cut off from the real world of electrons and photons, of nucleons and pions, and hence he had no truck with the axiomaticists, nor with those later who were to claim that reality lay with strings in 10 dimensions at an energy scale of $10^{19}$ GeV. Although nearly always formal in his style of doing physics, he was all his days a phenomenologist, seeing the role of the physicist as to understand the world of experience.

This was certainly true in his late field theory days, from the late 1950s until 1965. He had already laid, in 'A theory of fundamental interactions' [82], published in 1957, the groundwork for the unification of the weak and electromagnetic interactions, and thus for the standard model of particle physics that we take for granted at the end of the millennium. We will describe this awesome

* This reminds us of the wonderfully mistaken opinion of Robert Sachs in the 1940s. He believed that Schwinger was too good a nuclear physicist to get involved in field theory.[9]

paper in the following chapter. But he published many other papers confronting the poorly understood experimental situation in high-energy physics of the day.

Schwinger presented the results in 'Field theory of unstable particles' [94] at the International Conference on High Energy Physics, the so-called Rochester meeting, which was held in July 1959 in Kiev, then in the Soviet Union. (He received an NSF travel grant to attend this meeting.[10]) This was the Schwingers' first trip to the USSR. Schwinger had various memories of that trip. 'I took a walk with [Lev] Landau and he asked me about something that he was doing that I didn't know anything about [superfluidity], and that's not a good basis for conversation. I hadn't been paying any attention to those things. He should have asked Feynman, not me. . . . I was certainly impressed with him. And we were walking along, he was quite tall; he had gray hair at the time, wore a dark suit, I think. Well, I'm always a little abashed when confronted with, you know, a great name. The trouble is I don't think he really knew what I was doing and I certainly didn't know what he was doing. So we exchanged generalities. Perhaps the nearest thing we might have talked about is Landau's work on the asymptotic propagation function; he found a complex singularity that led him to suggest that maybe there was a cutoff at the gravitational scale.'[11,8]

The Schwingers were taken around to a number of places. 'There was a boat ride up or down, probably both, the Volga, and I remember being pursued by all kinds of Russian physicists who wanted to ask me about everything, to the point where I was really getting exhausted. So I secreted myself.

'My memories are of the strange things, such as getting in an airplane in Moscow and landing in an unplowed field, which must have been a former air force base, and essentially there was no formal reception. [I remember] jumping off the steps, then looking around. Nobody was there to greet us. Where did we go? What is this place? And then somebody comes over and I had no Russian. He had no English and he just sort of poked at me in the chest. He was an enormous creature and said something about Congress . . . . Somebody who had been detailed, I think, to pick us up. I think we said yes and finally we got dumped into a car and ended up in the right place. It was a very disconcerting meeting to be landing at an old bomber field with nobody around.'[8]

Schwinger was impressed with the medieval gates of Kiev, which reminded him of the magnificent final section of one of his favorite pieces of music, Mussorgsky's *Pictures at an exhibition.**

After the meeting, the Schwingers went to Moscow and Leningrad, and then to Helsinki. They were enchanted with Helsinki. In part, this was a reaction

* In fact, this music was a tribute to Victor Hartmann's drawing for a great gate to the city, which he designed, but which was never erected; Hartmann regarded these plans 'in the massive old Russian style' to be his masterpiece.[12]

to the bleakness of the Soviet Union, although there they had been perfectly content. From Helsinki they went to Stockholm.[13]

The paper based on the talk he gave at Kiev, 'Field theory of unstable particles' [94], was submitted a month later, in August 1959, to *Annals of Physics.* There he showed that the description of stable and unstable particles can be given a uniform treatment in the framework of Green's functions. This is an elegant paper, making extensive use of spectral representations for Green's functions. The question he raised at the outset was whether the familiar exponential decay law of an unstable particle, that is, that the probability of finding the particle a time $t$ after it is produced is $e^{-t/\tau}$, where $\tau$ is the mean lifetime, should break down after a sufficiently long time. After a consideration of the nature of measurement, he concluded that 'with the failure of the simple exponential decay law we have reached, not merely the point at which some approximation ceases to be valid, but rather the limit of physical meaningfulness of the very concept of unstable particle.

'The whole point was to develop the space–time structure of a Green's function in general so it will be applicable both to stable particles and unstable particles. The nearest precursor of this is the work of Weisskopf and Wigner; there it was all done as an approximation and a certain *Ansatz* was accepted and the derivation of the exponential law of decay is clearly approximate and it's not clear what the situation is. People just invent arbitrary definitions. When you have an unstable particle, what do you call its mass? Is it where the pole, the complex pole is? Is it the center of the maximum? You really need a universal basis to infer all these things. The thing was to start with physical ideas. Then I am willing to let the mathematics take me where it wants to.'[8]

He concluded the paper with a brief discussion of the kaon situation. There are two neutral $K$ mesons, $K^0$ and $\bar{K}^0$, having hypercharge equal to plus and minus one, respectively. The weak interactions, however, do not respect hypercharge.* Therefore, the Green's functions exhibit exponential decay of two different types, corresponding to the 'mass eigenstates' $K_1$ and $K_2$. The resulting 'kind of mass interferometer' was soon to become familiar to all with the discovery of $CP$ violation in $K$ decays.[14]

Some years later, Schwinger presented the 'Ninth baryon' first at the Coral Gables Conference in January 1964 [119], and then as a *Physical Review Letter* [120], submitted that same month. These represented calculations based on his new 'Field theory of matter,' which had not yet been presented. He was considering the eight baryons in the family with the nucleons, the neutron and proton; these are the two nucleons (denoted $N$), the three sigma baryons

* Hypercharge, which in this case equals strangeness, was a concept introduced by Schwinger in [81]—see p. 415.

(denoted $\Sigma^+$, $\Sigma^-$, and $\Sigma^0$), the two xi baryons (denoted $\Xi^-$ and $\Xi^0$), and the neutral $\Lambda$ singlet. (These are now described as belonging to an octet representation of 'flavor' $SU(3)$, and are now understood in the quark model as being built up of up, down, and strange quarks.) At that time, of course, the approximate symmetry group was not yet established, and instead of $SU(3)$ he thought it might be a group he called $W_3$. Schwinger predicted a ninth singlet member of the representation, by analogy with the partner to the octet of spin one mesons, the ($\rho$, $K^*$, $\bar{K}^*$, $\omega$) system, completed by the $\phi$. (Actually, there is mixing between the singlet states, $\omega$ and $\phi$. The $\omega$, which decays predominantly into three pions, was predicted by Nambu, Chew, Sakurai, Schwinger, and others before its discovery in 1961—see p. 395). He thought this baryon might be the particle now called the $\Lambda(1405)$, at a mass of about 1.4 GeV. As we will see, this interpretation was not to stand, and no such ninth singlet partner to the baryon octet is believed to exist, yet he did extend the analogy to the spin-0 octet, and suggested that that system of three pions, two $K$ mesons, two $\bar{K}$ mesons, and one $\eta$ meson include a ninth member, at a mass of about 1.5 GeV. Indeed, the $\eta'$ meson was discovered within a month or so of this prediction,[15] but at a considerably lower mass.

In spring 1964 Schwinger began a series of four papers entitled 'Field theory of matter' [118, 124, 127, 128]. The first paper he later referred to as 'speculative (two intermediate vector bosons, with incomplete lepton polarization) and practical (mass formulas, selection rules).'[16] His attitude toward the relation between field and particle was stated in a footnote: 'I contend that the fundamental dynamical variables are field operators, while particles are identified as stable or quasistable excitations of the coupled field system. There is no *a priori* relation between the primary dynamical fields and the secondary phenomenological fields that can be associated with the observed particles.' [118] In this paper, Schwinger built upon the ideas suggested in 'A theory of fundamental interactions' [82] (which we will analyze in the following chapter) and imagined a non-Abelian interaction between Fermi fields, carrying nucleonic charge, and neutral and (nucleonic) charge-carrying vector fields. He assumed that independent three-dimensional unitary groups $U_3$ act on each of the fields separately. (This is his $W_3$ symmetry group.) The multiplicity of any charge, electrical or nucleonic, was three: 'This is a threefold way.' 'The threefold symmetry is introduced here at a deeper dynamical level than the observed particles. An independent attempt in this direction has been made by Gell-Mann.[17] He introduced particles of fractional charge which can be detected, presumably, only by their "palpable piping, chirrup, croak, and quark." '[18] [118]

In this scheme, baryons were formed by combining fermion and vector fields, that is, created by the operator $\psi_a V_b$, where the indices refer to the two $U_3$ groups, $a, b = 1, 2, 3$. Thus, automatically, there were not eight, but nine baryons in the multiplet. Mesons were constructed from $\bar{\psi}_a\psi_b$, and, indeed, by this time (March 1964) it was known there were nine $1^-$ mesons as well as nine $0^-$ mesons. For the singlet member of the former he made a width estimate, $\Gamma(\phi) = 3.4\,\text{MeV}$, which then 'agrees well with the observed value.' (The current accepted value is $4.43 \pm 0.05$ MeV.[2])

Schwinger closed this paper with a discussion of the symmetry groups, $W_3$ versus $SU_3$. He noted that 'the crucial experimental spin-parity determination for $Y_0^*(1405\,\text{MeV})$ [now called $\Lambda(1405)$] will test whether $W_3$ or $SU_3$ is the more realistic symmetry group.' Unfortunately for Schwinger, $\Lambda(1405)$ turns out to have spin-parity $\frac{1}{2}^-$, and therefore cannot be a member of the same multiplet as the nucleons, which have spin-parity $\frac{1}{2}^+$.

A month later, in April 1964, Schwinger submitted '$\Delta T = \frac{3}{2}$ non-leptonic decay' to *Physical Review Letters* [121]. The title referred to the remarkable suppression of weak decay processes in which the isospin $T$ changes by $\frac{3}{2}$, as compared to those in which it changes by $\frac{1}{2}$. Thus the amplitude for the $\Delta T = \frac{1}{2}$ process $K_1^0 \to \pi^+\pi^-$ is 23 times greater than that for the $\Delta T = \frac{3}{2}$ decay $K^+ \to \pi^+\pi^0$. In this note, Schwinger pointed out that the latter rate can be inferred from the leptonic decay rates for $\pi^+ \to \mu^+ + \bar{\nu}$ and $K^+ \to \pi^0 + e^+ + \nu, \pi^0 + \mu^+ + \bar{\nu}$. There was a twofold ambiguity in extracting a relative coupling constant in these decays. One of the choices, with the hypothesis that the weak interactions were universally mediated through a charged intermediate vector boson (which Schwinger called $Z$, but which is now called the $W$), gave complete consistency with the observed non-leptonic kaon decay ratio.

Two further *Physical Review Letters* followed, submitted in August and September of that year. These were 'Broken symmetries and weak interactions' I and II [122] [123]. These were concerned with the breaking of $SU(3)$ symmetry by parity-preserving non-leptonic decays. (Schwinger was still working within his $W_3$ scheme, which contained $SU(3)$ as a subgroup.) He considered the $\Delta T = \frac{1}{2}$ parity-conserving decays to proceed because the decay constants for the pion and kaon are different, $f_K \neq f_\pi$. He also considered mixing with his 'ninth baryon,' and, in the second very brief note, a breakdown of the Gell-Mann–Okubo octuplet mass formula.[19]

The second 'Field theory of matter' paper [124] was submitted in July 1964. The one-sentence abstract stated that 'A qualitative dynamical description is given for observed regularities of non-leptonic phenomena in strong, electromagnetic, and weak interactions.' The paper was based on broken $W_3$ symmetry, and the construction of observed states by products of fermionic and vectorial field products of opposite signs of nucleonic charge, $\bar{\psi}_a\psi_b$, $\bar{\psi}_a V_b$, and $\bar{\psi}_a\bar{\psi}_b\psi_c V_d$. The importance of the paper, in Schwinger's later view, was that it gave 'the dominance of dynamics by short-ranged operator products,'[16] the precursor to a chief tool of modern field theory, the operator-product expansion.[20]

Schwinger attempted to describe the phenomenology in one paragraph: what would be called vector dominance was the key. Electromagnetic interactions were seen as proceeding through the $1^-$ mesons such as the $\omega$ and $\rho^0$. 'Similarly, the vector and pseudoscalar currents that are coupled to the $Z$ [now $W$] field can be represented approximately by the phenomenological fields of known $1^-$ and $0^-$ mesons.' This led immediately to the Goldberger–Treiman relation,[21] which relates the pion decay rate to the axial vector coupling constant.

The paper itself was only two and a half pages long, but was followed by another page of *Notes added in proof.* There, Schwinger clarified and corrected certain errors and confusions concerning $SU(3)$ and vacuons (vacuum expectation values of field products).

Schwinger returned to Russia in 1962, as an exchange professor in Leningrad, and in 1964. In the latter year he again attended the 'Rochester' meeting, this time in Dubna, and presented a paper on the 'Field theory of matter' [126]. Clarice did not accompany him on this trip, one of their rare separations. It was preceded by a visit to Copenhagen, where Schwinger somehow took a picture of Marlene Dietrich.[13] That summer he also lectured at the Brandeis summer school, on the same subject [125].

The final 'Field theory of matter' paper [128] was received by the *Physical Review* in April 1965. Again, Schwinger's words from his introduction tell us what he was after. 'The preceding paper of this series [127] describes a program for establishing contact between the fundamental fields $\psi_{\zeta a}(x)$ and $V_a^\mu(x)$ $V_a^{\mu\nu}(x)$, and the phenomenological fields that represent observed particles. This is accomplished by a technique of comparative kinematical transformation. Compact groups of kinematical transformations on the fundamental fields are exploited as a device for conveying dynamical information concerning the highly localized structure of the phenomenological fields. The hypothesis of completeness for stable and unstable particles permits a linear representation of the essentially localized transformations on the phenomenological fields. This implies a correspondence between group generators at the fundamental and the phenomenological levels. Each kind of generator is a quadratic function of the appropriate kind of field. The quadratic functions of the fundamental fields, as objects with various tensor transformation properties, are also represented linearly by the phenomenological fields of bosons with suitable spins and parities. Through the machinery of relativistic field theory, particularly the distinction and the relation between independent and dependent field components, these alternative phenomenological identifications of group generators serve to determine phenomenological field dynamics. In this paper the program will be illustrated by the dynamics of $0^-$ and $1^-$ bosons, in the idealization of $U_3$ symmetry. We shall also consider briefly the dynamics of spin-$\frac{1}{2}$ particle triplets. The extension to baryon interactions and the inclusion of unitary symmetry-breaking effects will be dealt with separately.' That extension was never to be, for Schwinger's source theory revolution was to bring a completely phenomenological orientation to the fore.

In this paper, Schwinger abandoned his old $W_3$ symmetry, and instead used unitary symmetry $U_3$. In particular, the 12-component fundamental field $\psi_{\zeta a}$ transformed according to $U_6 \times U_6$. On the other hand, the $0^-$ and $1^-$ nonets of mesons were contained in 36 non-Hermitian phenomenological fields, which transform as second-rank tensors under $U_3$. Meson dynamics were obtained by writing down a Lagrangian invariant under the symmetry group $U_3$, which implied an interaction term $\mathcal{L}_{\text{int}}$ invariant under $U_6 \times U_6$. 'Let it be emphasized that we have *derived* this property of the interaction term from our fundamental dynamical assumptions concerning localizability and completeness. It is entirely comprehensible that $\mathcal{L}_{\text{int}}$ should possess this invariance as a kinematical expression of the highly localized dynamical relation between phenomenological and fundamental fields, without such transformations having the slightest relevance to the remainder of the phenomenological Lagrange function, which characterizes the propagation of the physical excitations.' [128] Here Schwinger was referring to the difficulty of having a symmetry that mixes internal and space–time properties.[22]

Schwinger next showed that this theory was consistent with the phenomenological analysis of $\rho\pi\pi$ and $\omega\rho\pi$ couplings given in [118]. And he considered the dynamics of fermion triplets, from which followed the observed mass degeneracy of the singlet and octuplet of $1^-$ mesons (the mass of the $\rho$ is 769 MeV, while that of the $\omega$ is 782 MeV). The fermion triplet also allowed the derivation of electromagnetic properties, through a form of 'vector meson dominance' that was explicitly gauge-invariant.

This last attempt at phenomenology in the context of field theory contained two interesting *Notes added in proof*. The first thanked Sidney Coleman (whom Schwinger was responsible for Harvard hiring) for raising the issue of the Schwinger terms (pp. 389–393) in this context. The inclusion of these additional terms in the equal-time commutators required an additional term in the meson interaction Lagrangian, which necessitated the equality of the masses for all nine $1^-$ mesons when the $U_3$ symmetry was valid. The second note referred to a nonet of $2^+$ mesons, the $f_2(1270)$, $a_2(1320)$, $K_2^*(1430)$, and the $f_2'(1525)$ (using modern notation, where the number in parentheses is the mass in MeV), which have isospin 0, 1, $\frac{1}{2}$ (twice), and 0, respectively. These could be easily accommodated within Schwinger's dynamical scheme. He also noted that 'parity violating transformations can be represented if non-linear transformations are admitted.'

Clearly Schwinger was chafing at the necessity of passing from the fundamental field-theoretic world to the world of phenomena. Within a matter of months he would see a way of freeing himself from the inaccessible fundamental level. That was source theory, to which we will turn in Chapter 13. But now we must recount his other major contributions to field theory during this same critical period.

## An excursion into dispersion relations

In 1957 Schwinger became interested in the analytic structure of the Green's functions of quantum field theory, in particular in obtaining spectral forms or dispersion relations for two- and three-point functions, that is, for propagation functions and for vertex amplitudes. He presented his results at a Rochester conference, in April 1957, in a session chaired by Marvin Goldberger.[10] His presentation, was followed by a response by Gunnar Källén who 'most violently disagree[d] that the formula written down is the most general representation of the three-point function.' After the meeting Schwinger submitted a supplement to his presentation for the conference proceedings to one of the editors, his former student Roger Newton. The issue is clarified by a letter from Stanley Deser sent to Schwinger from Copenhagen, dated 26 [April] 1957: 'The form you wrote down ... is *not* the most general under the usual assumptions, but it is in fact equivalent to the form Källén had, and to which the Lehmann–Jost counter example applies.' The Green's function is given by a time-ordered product of fields, or, in terms of momenta $p_i$ and spectral masses $\kappa_{ij}$,

$$
\begin{aligned}
\mathcal{G} &= ((\phi_1\phi_2\phi_3)_+) \\
&\sim \int \frac{e^{i\sum p_i x_i}\delta\left(\sum p_i\right)\delta\left(\sum z_i - 1\right) f(\kappa\ldots)\mathrm{d}p\,\mathrm{d}z\,\mathrm{d}\kappa}{\left[p_1^2 z_2 z_3 + p_2^2 z_1 z_3 + p_3^2 z_1 z_2 + \kappa_{12}^2 z_3 + \kappa_{13}^2 z_2 + \kappa_{23}^2 z_1 - i\epsilon\right]^3} \qquad (11.1)\\
&= \int \Delta_+\left(x_{12}^2, \kappa_{12}^2\right)\Delta_+\left(x_{13}^2, \kappa_{13}^2\right)\Delta_+\left(x_{23}^2, \kappa_{23}^2\right) f(\kappa\text{'s})(\mathrm{d}\kappa\text{'s}), \qquad (11.2)
\end{aligned}
$$

$\Delta_+$ being the scalar propagation function,

$$
\Delta_+(x^2, \kappa^2) = \int \frac{(\mathrm{d}p)}{(2\pi)^4} \frac{e^{ipx}}{p^2 + \kappa^2 - i\epsilon}. \qquad (11.3)
$$

'So, the consensus is that while assumption of the form (11.1) or (11.2) for $\mathcal{G}$ does lead to the dispersion relation, it is as yet insufficient, since the theory may admit of more singularities in $\mathcal{G}$, etc, though of course perturbation theory always satisfies (11.1) or (11.2).'[10]

After receiving this letter, Schwinger sent a telegram to Roger Newton dated 10 May 1957: 'Promised supplementary remarks are being sent. JS.'[10] The Proceedings of the Conference contain both the original manuscript of Schwinger's lecture [83a], Källén's toned-down response, and a five page supplement by Schwinger, followed by another Källén rebuttal.[23] Evidently, in view of the hostile response, Schwinger never wrote a journal article on this subject.

After the Rochester meeting the Schwingers went home with Bob and Jane Wilson and stayed with them overnight in Ithaca. The next day was Easter, and their boys began to look for Easter eggs about two hours after the Schwingers had gone to sleep. At a more reasonable time in the morning, the adults had champange and a good breakfast.[13]

In the summer of 1957 Schwinger attended a mathematics conference in Lille. 'Les Problèmes Mathématique de la Théorie Quantique des Champs.'[10] 'I gave a lecture on whatever I was thinking about the formulations of field theory at the time. I don't think it was the action principle, but I think I wrote down some symbolic solutions of the field equations involving exponentials of a product of a couple of functional operators and the mathematicians in the audience burst into laughter. That was outrageous, disgraceful. I was a little stunned, so that was not very successful. But the audience was wrong. I also have memories of wandering around Lille and running into some of the local people who were pleased to amuse themselves at my expense. It was really funny, you know, to meet people I met before and have them exchange remarks to each other in French under the impression that I understood not a word, but I did. A little shocking.'[8] He also had 'distinct gastronomic memories of a little restaurant, a tiny restaurant, on a street just off Grand Place, that was mind-blowing,'[8] a restaurant he visited again when he went to Belgium in 1961.

## Spin, statistics, and the TCP theorem

The reader may recall that when Schwinger began to read the physics literature, he took extensive notes and worked out the details of some of the more striking papers which appeared in the early 1930s. Among these was the paper of Pauli and Weisskopf[24] 'On the quantization of scalar relativistic wave equations,' which was concerned, in part, with spin and statistics. This is the familiar result, overwhelmingly in accord with observation, that particles with integer spin (in units of Planck's constant $\hbar$) obey Bose–Einstein statistics, so that any number of such particles, called bosons, can be in the same quantum state (now observed in the Nobel Prize-winning experiments on cold atoms[25]) while particles that carry integer plus $\frac{1}{2}$ units of angular momentum, fermions, obey Fermi–Dirac statistics. Thus fermions can only be one to a state, which is responsible for the building up of atomic and nuclear structure. This spin and statistics connection remains merely an empirical fact in nonrelativistic quantum mechanics, but it has been promoted to a theorem in relativistic quantum field theory.[26]

As we have seen many times, years passed before Schwinger made his own contribution to this important subject.

Although Pauli established the spin-statistics connection in 1940,[27] he did so only for non-interacting fields. In fact, Schwinger gave the first proof for the case of interacting fields in his papers 'The theory of quantized fields. I' [65] and 'II' [73]. In these early papers, published in 1951 and 1953, Schwinger assumed parity invariance: 'This was the statement: if it was fully Lorentz invariant under all possible transformations, proper, improper, isochronous or anti-isochronous, then spin and statistics would follow. The later line of development which came with the destruction of parity was to turn it around, and say, if you accept the spin-statistics connection, then you can run it backwards and prove time reflection invariance, which is time reflection invariance including charge and parity. What became *TCP* was simply my theorem inverted in the light of the new experimental situation.'[8] Here *TCP* (or any other permutation of the same letters) refers to invariance under the combined transformations of time reflection *T*, charge conjugation (replacing particles by their antiparticles) *C*, and parity (space reflection) *P*.

But Schwinger was never given credit for this early work; the *TCP* theorem is invariably attributed to Lüders and Pauli,[28] who published three years later. Some years afterward Schwinger protested to Pauli, but Pauli refused to accept Schwinger's priority: 'I couldn't persuade him. I remember though pointing to a simple example of it in one of the papers, 'Theory of quantized fields. IV' [76], indicating that in fact I had already made use of the combinations of those transformations [*T*, *P*, and *C*] and that was also a natural thing to do and this was certainly pre-Lüders, and so forth. Then I also felt very much that I'd been gypped on that. That *TCP* was my old theorem, stood on its head.'[8]

To make this point, Schwinger submitted 'Spin, statistics, and the *TCP* theorem' [84] to the *Proceedings of the National Academy of Sciences* in December 1957. He was motivated to write this paper because of the recent discovery of the breakdown of parity and of charge conjugation in weak interactions.[29] Once again, it helps to set the stage by quoting the introductory paragraph: 'The recent experimental work relating to space parity and charge symmetry in the so-called weak interactions have emphasized the need for a clearer recognition of the role played by the continuous Lorentz subgroup of proper, orthochronous Lorentz transformations. This refers, in particular, to some work of the author [65, 73], in which the general requirement of Lorentz invariance for the quantum theory of fields is used to deduce the connection between the spin and the statistics of particles, and to a converse statement (now known as the "*TCP* theorem") in which the acceptance of the spin-statistics connection implies invariance under a combined space, time, and charge-reflection operation. It is the intention of this note to make explicit the fact that the general dynamical structure of the quantum theory of fields, together with the specific assumption of invariance under the proper orthochronous Lorentz subgroup, and the existence of a lowest-energy state (the vacuum) for any physically realizable system, implies both the connection between the spin and statistics of particles and the *TCP* theorem.'

Schwinger then employed his quantum dynamical principle, and its invariance under Lorentz transformations (and hence under Euclidean rotations) and under complex conjugation to show that the following two Lagrange functions were equivalent:

$$\mathcal{L}(\phi_{\text{int}}, \phi_{1/2\,\text{int}}, \psi_{\text{int}}, \psi_{1/2\,\text{int}}), \quad \mathcal{L}(\phi_{\text{int}}, \mathrm{i}\phi_{1/2\,\text{int}}, \mathrm{i}\psi_{\text{int}}, \psi_{1/2\,\text{int}}), \tag{11.4}$$

where $\phi_{\text{int}}$ and $\psi_{1/2\,\text{int}}$ are boson and fermion fields carrying integer and half-integer spin, respectively, while $\phi_{1/2\,\text{int}}$ and $\psi_{\text{int}}$ have the wrong connection between spin and statistics. The dominance of the kinetic energy part of the Lagrangian at high energy was then used by Schwinger to argue that if the spectrum of the wrong-statistics fields extended to $+\infty$ in the first Lagrangian, it extended to $-\infty$ in the second, meaning that the spectrum was unbounded below, there was no lowest energy state, no vacuum. Thus these fields were incompatible with the physical assumptions, and could be ruled out. The remaining fields had a Lagrangian which was invariant under reflections in space and time together with complex conjugation, which was the statement of the *TCP* symmetry.

The final point of the paper was to make the connection between complex conjugation and charge symmetry. The latter symmetry was attributable to Schwinger's use then, and later, of Hermitian fields 'which, individually, or in the multiplicity necessary to incorporate spin, do not describe internal characteristics but rather accomplish this through an additional degeneracy.' Charge, of whatever variety, was represented by an imaginary, antisymmetrical matrix. The connection between complex conjugation and charge conjugation was then immediate.

This short paper concluded with a speculation concerning the use of complex numbers in quantum mechanics. He referred to an extensive textbook on the subject which was never to see the light of day. 'The mathematical machinery of quantum mechanics is a symbolic expression of the laws of atomic measurement, abstracted from the specific properties of individual techniques of measurement.' This was a reference to his measurement algebra, which we have described in detail in Chapter 10. He carried on this remark in a footnote. 'The possibility thus suggested of a direct physical motivation of the otherwise remote mathematical entities of quantum mechanics has been under development since 1951 and at several stages has been published in various (inaccessible) places. Perhaps the most complete account appeared in *Lectures at the Summer School* *of Theoretical Physics, Les Houches, France, 1955.*' Although he continued working on his quantum mechanics book throughout his life, he was never to bring it to a satisfactory form in his eyes. (Of course, it appeared in brilliant lectures at Harvard and elsewhere, and a fragment of the Les Houches lectures was published in [152].)

Some four years later, in 1961, Schwinger was to return to this subject, in 'Spin and statistics' [103], co-authored with his student Lowell Brown. Brown remembered the genesis of this paper well. 'The spin-statistics theorem paper started out as an Appendix in my thesis. The formalism in the proof was, of course, Schwinger's. The basic idea of the proof is essentially that of the earlier Burgoyne work.[30] The details, however, are different. I think that the idea of using a theorem of Laplace transforms for the proof was mine. But the general methods for treating general spin were Schwinger's. By the time we got around to write the paper, in the summer of 1961, Schwinger was at UCLA and I was working at IBM in San Jose to make enough money to buy an Alfa Romeo Giulietta Spider in Italy in the Fall. I sent Schwinger the Appendix, and although he completely rewrote it in detail, the basic outline was not changed. A secretary at UCLA typed the manuscript—of which I still have a copy—and it was sent to me to proofread and then send on to the *Progress of Theoretical Physics*. I found only one error: the secretary had written "four-dimensional spherical harmonies" instead of "harmonics." '[31]

The paper was published in the Japanese journal *Progress in Theoretical Physics*, 'because that is where Schwinger told me to send it. I do not know the basic reason for this. I think that Schwinger subscribed to this journal at that time, and perhaps he wanted to submit a paper to it as a symbol of goodwill to the Japanese physicists—didn't he give a lecture on Tomonaga [200]—and didn't he have some respect for their work on QED during the war? But also maybe he wanted to publish it there because although the details were original, the fundamental idea was not. I stated in a footnote in my Appendix: "This proof of the spin-statistics connection is in many respects similar to that of Burgoyne.[30] Burgoyne's proof, however, makes extensive use of analytic properties and group theory in a somewhat abbreviated fashion. It is therefore the opinion of the author that an elementary and self-contained proof is worthwhile presenting." '[31]

In this paper, Brown and Schwinger indeed improved on Schwinger's earlier discussion of the spin and statistics theorem. This was necessary because his previous papers made problematic assumptions concerning the high energy behavior of the fields, and involved superfluous field components. 'It suffices to require of the fields that, characteristic of the statistics, they commute (Bose–Einstein) or anticommute (Fermi–Dirac) for any two points in space-like relation at spatial separations in excess of some finite distance which need not be zero. Covariance under the proper, orthochronous Lorentz group is demanded, of course, together with the hypothesis that the ground state of the system is the unique invariant vacuum state.' [103] The elegant proof depended upon the existence of a spectral representation for the vacuum expectation value of the product of two Hermitian fields $\chi$ at different points,

$$\langle\chi(x)\chi(x')\rangle = \int \frac{(\mathrm{d}p)}{(2\pi)^3}\mathrm{d}\kappa^2\eta_+(p)\delta(p^2+\kappa^2)m(p), \tag{11.5}$$

where

$$\eta_+(p) = \begin{cases} 1, & p^0 > 0 \\ 0, & p^0 < 0 \end{cases}. \tag{11.6}$$

Here $m(p)$ was a non-negative, finite-dimensional Hermitian matrix, which cannot be zero for all $p$. A contradiction was achieved if it was assumed that half-integral spin fields commuted for sufficiently large space-like separations, or if integer spin fields anticommuted for large enough space-like separations. As Lowell Brown noted, Brown and Schwinger acknowledged that Burgoyne and other authors[30] had exploited similar requirements, but with more elaborate mathematical machinery, and without Schwinger's reconstruction of quantum mechanics [91, 93, 96–98], which we discussed in Chapter 10.

There remained one flaw in this paper. It did not directly apply to electrically charged fields, because then when a Lorentz transformation was made, an operator gauge transformation must accompany it. Brown and Schwinger promised to elaborate on this point elsewhere, but that further work never appeared. They did conclude the paper with a simple argument that if it was assumed that the statistics did not depend on the magnitude of the charge, one could form electrically neutral composite fields for which the arguments of the paper applied, and then it was easy to see that the spin-statistics connection could not be reversed.

## Euclidean field theory

In passing we noted that when Schwinger in 1957 discussed the spin-statistics connection in [84], he expressed the connection between Lorentz transformations and Euclidean rotations. He fleshed out this idea in earnest the following year. Eventually, he came to believe that the Euclidean formulation was fundamental, even raising it to the level of a postulate: 'It is a remarkable fact that all F.D. [Fermi–Dirac] particles carry some kind of charge. The experimental proof of non-identity between electron and muon neutrinos confirms an early suggestion [82] that neutrinos would be no exception to that rule. A representation of that regularity is given by the following abstract Euclidean postulate: The vacuum probability amplitude must be transformable into the attached Euclidean space in such a way that the original time axis cannot be identified.' [135]

In fact, Schwinger recalled that this work, like so much of his other developments in field theory, grew out of his research on radar during the war. In 1958 this was the situation: 'Green's functions [were] universally recognized as carrying the information of physical interest. . . . Now the point is that one had differential equations for these Green's functions and then came the necessity of picking out of the vast infinity of solutions the physical ones of interest: physical ones which refer to the fact that the vacuum is the ground state of the system and the lowest state has energy zero, momentum zero, and is a relativistic invariant thing.'[8] This was enforced by appropriate boundary conditions, that the wave propagate outwards, that is, the idea of causality. 'I recognized somewhere along the line that the condition that the waves move outward could be expressed by an extension into complex space. That is, if you rotated the time axis into a complex space, then the boundary conditions are such that the Green's functions . . . [are] decreasing exponentials. . . . I simply recognized that by moving from real time into complex time in a certain way that would select just the physically acceptable states of the Green's function. In fact it must go back to the electrical engineering days of waveguide stuff because . . . in a waveguide if you have a high enough frequency the wave propagates. If the frequency gets too low, it exponentially attenuates. And if you have a general solution, you must always choose the right sign of the square root so it goes down and not up.'[8]

In 1957 Schwinger offered George Sudarshan a postdoctoral fellowship. He came from Rochester, having done a thesis on the $V - A$ theory of weak interactions, which we will discuss in the next chapter. However, he soon came to disbelieve in the Euclidean formulation. 'I remember his remark when I told him lightly about this Euclidean formulation. He said, "That simply can't be true." Did we work much together? I don't think so.'[8]

In June 1958 Schwinger communicated 'On the Euclidean structure of relativistic field theory' [86] to the *Proceedings of the National Academy of Sciences*. The thesis of the paper is given in the language of group theory: 'It is well known that some representations of the Lorentz group can be obtained from the attached Euclidean group (the "unitary trick" of Weyl). What is being asserted is that *all* representations of physical interest can be obtained in this way.'

Schwinger presented the results of this paper at the International Conference on High Energy Physics in July 1958, which was held at CERN, in Geneva. Clarice recalled that on this trip they visited Paris, Italy, Vienna, and Zurich, where they visited the Paulis and Oskar and Gerda Klein, and their daughter Elsbeth and their son-in-law Stanley Deser, before going to Geneva.[13] Schwinger's contribution to the *Proceedings* [87] is identical to the National

Academy paper [86] with the exception of a rather extended opening paragraph. Here, he gave more motivation for this new thrust. The view was very modern. In quantum mechanics 'we know that the nature of states is fundamentally related to the underlying symmetry group. That is, we can say that the physical states are, in a sense, representations of the underlying Lorentz group on the one hand, or of the Euclidean group on the other, and these two groups have completely different topologies. This means that while you can certainly take a representation of the Euclidean group and from it derive a representation of the Lorentz group, you will not get all possible representations this way. What I would like to assert is that while one does not get all the mathematical representations of the Lorentz group, all the representations of physical interest are actually obtained. The essential point to be made is that this possibility of a correspondence between the quantum theory of fields with its underlying Lorentz space, and a mathematical image in a Euclidean space—if one adopts a postulate that one should be able to do this in detail—gives results which go beyond what can be obtained from the present theory of fields. These I shall try to indicate. But besides this, by freeing ourselves from the limitation of the Lorentz group, which has produced all the well-known difficulties of quantum field theory, one has here a possibility—if this is indeed necessary—of producing new theories. That is, one has the possibility of constructing new theories in the Euclidean space and then translating them back into the Lorentz system to see what they imply. Concerning the second feature, I have done nothing. I am merely suggesting that when one finds formulations that are equivalent, one of these will be distinguished as the one that makes contact with the future theory.' (Interestingly, Feynman would make a similar point later, at the time of his receiving the Nobel Prize. See Chapter 16.[32])

The paper, and the lecture, conclude with a practical comment. 'Although we have emphasized the fundamental implications of the Euclidean representation, it will be evident that the Euclidean-type Green's functions also have practical advantages. Indeed, the utility of introducing a Euclidean metric has frequently been noted in connection with various specific problems, but an appreciation of the complete generality of the procedure has been lacking.' As Schwinger noted many years later, 'I still recall the utter disbelief this idea engendered.'[16] Indeed, the chairman of the session at CERN reacted caustically: 'I thank you very much for this inspiring report. To open the discussion I wish to say that for the audience it is perhaps a bit more interesting than for the speaker that the idea of analytical continuation has been anticipated by Wightman. Instead of more general transformations the speaker has selected a particular case, of rotation of 90 degrees, and I hope I interpret him correctly that he means that this has a special significance for physics and for the formalism in that particular case.' Unsurprisingly, that chairman was Wolfgang Pauli. As Lowell Brown has noted, time has been on the side of Schwinger: 'Well, the rich vacuum structure of quantum field theory that we now understand arises from instantons which exist in Euclidean space–time, and the thermodynamics of quantum field theory is really Euclidean theory, and so forth, and it's Euclidean this and Euclidean that, and I think that history has certainly proven that Schwinger was right and Pauli wrong.'[33]

In spite of this and other confrontations* with Pauli, socially the Schwingers got along with him very well. Clarice still has a photograph of him in Geneva that July. She always regarded him as warm, funny, and gallant.[13] After the meeting, the Schwingers returned to the US and spent the rest of the summer of 1958 in Madison, Wisconsin, where Clarice was bored to distraction.[13] However, it was a very productive summer for Julian.

As we see, Schwinger's paper on Euclidean field theory [86] was impressive. He was able to transform the inherently complex Green's function of the Lorentz description into purely real Euclidean Green's functions (apart from the imaginary, antisymmetrical charge matrices). And, as a mark of Schwinger's newly won acceptance of the violation of parity symmetry, he showed that the Lagrange function was not invariant under space reflection, or parity ($P$), and charge reflection, or charge conjugation ($C$) separately, but only under their product, $CP$, due to 'the postulate that Euclidean Green's functions exhibit a relativistic invariance with respect to charge reflection.' (It was only six years later that $CP$ symmetry was found to be weakly violated in weak interactions, a symmetry breaking still not understood.[14])

At the end of this paper, Schwinger promised the reader a fuller discussion of 'transformation and representation theory' in his forthcoming *Handbuch der Physik* article. As we note below, this article was never completed. The reader was further promised a recasting of quantum electrodynamics into the new Euclidean framework.

That reformulation was submitted to the *Physical Review* in March 1959 as 'Euclidean quantum electrodynamics' [88], with the note that this paper was largely written the summer before in Madison. In this paper he began by constructing the Green's functions of the theory in the radiation gauge. Although these Green's functions 'are of direct physical significance,' the result could not be cast into Euclidean form because it retained reference to the unit timelike vector that characterizes the radiation gauge. But then he was able to transform this dependence away by performing a gauge transformation to a Lorentz

* Apparently there was also some disagreement with Stückelberg at the same meeting. 'Stückelberg was pointing out that he had developed a formulation of quantum mechanics in which only real numbers occur. And I acknowledged that but said that what I was doing was something else.'[8]

gauge, 'which lacks immediate physical interpretation.' Schwinger stated his opinion that it was unnecessary to reconstruct the theory in terms of an indefinite metric,[34] because 'the physical operator basis used in the definition of the radiation gauge Green's functions is entirely adequate.' Rather, the Lorentz-gauge Green's functions were merely a path to the Euclidean functions, which Schwinger then proceeded to construct. In terms of the action operator for the electron and photon fields, $W[\psi, A]$, the Green's functions are

$$G(x, \xi) = \frac{\langle 0|\psi(x_1)\cdots\psi(x_{2n})A(\xi_1)\cdots A(\xi_\nu)e^{-W[\psi,A]}|0\rangle}{\langle 0|e^{-W[\psi,A]}|0\rangle}, \tag{11.7}$$

which are real. After yet another reference to the *Handbuch* article that was never to appear, Schwinger expressed these Green's functions in terms of functional differential equations, and solved them by functional integrals, what others would call a path integral.* 'Now this is a far cry from Feynman but nevertheless I suppose I never would have thought of the functional solution of the differential equations without knowing what Feynman had done. Or would I? You know in statistical mechanics long before there were the Wiener–Hopf integrals.'[8]

A technical note remained. In a closing footnote Schwinger noted that the term 'Lorentz gauge' referred not to a specific gauge but to a class of gauges. Gauge transformations between members of this class, which were particularly discussed in the Russian schools,[35] cannot affect the radiation-gauge Green's functions, and hence the physics. This Schwinger proved in a short paper 'Euclidean gauge transformations' [95] he sent to the *Physical Review* in September 1959. He finally proposed that the transverse Lorentz gauge 'be used in future work on the Euclidean Green's function of electrodynamics,' since only that choice was free from ambiguities. But, in fact, Schwinger did not pursue this idea further. However, the work on multiparticle Green's functions that we discussed in Chapter 9 was an outgrowth of the Euclidean formulation. 'This could not have happened, I don't think, without the previous line of development.'[8]

## Schwinger terms

There was a time, in the middle to late 1960s, when Schwinger's name was mentioned only, but seemingly in nearly every theoretical talk, in connection with an anomaly, the 'Schwinger terms,' that was brought up to be then disregarded.†

* We have seen previously Schwinger's tremendous contribution to the development of functional techniques, both integral and differential, which contributions are at least as important, and indeed as pervasive, as Feynman's.

† The authors of the present volume have recently become painfully aware that this phenomenon still occurs with regularity. However, the pervasiveness of Schwinger's

This was the heyday of current algebra, wherein it was proposed to regard the algebra of currents, that is, their commutation relations, as a fundamental basis for constructing a theory of strongly interacting particles. But Schwinger had already noted in 1959 that these commutation relations could not be deduced independently of field-theoretic considerations, in the brief note 'Field theory commutators' [90]. As Schwinger noted years later, 'This is a famous paper in certain circles. It may be the only paper that the modern generation knows me for.'[8]

Schwinger had a very painful memory of writing this paper. 'Clarice and I were traveling somewhere [perhaps to Virginia Beach] and we were driving back to Boston and I suddenly began to feel worse and worse and I ended up in bed. It turned out I had a viral attack of something called the shingles. I gather it comes in various forms. This is the one that goes around the waist. The whole body was such that I could hardly move. As a result, I found myself confined to bed. That is where this was written, in bed.'[8,*]

The paper began by noting the contradictions between the formal commutation relations of field theory and positivity of the energy spectrum, which could only be resolved by recognizing that products of field operators must be understood as the limit of products of fields at different points. 'It is customary to assert that the electric charge density of a Dirac field commutes with the current density at equal times, since the current vector is a gauge-invariant bilinear combination of the Dirac fields. It follows from the conservation of charge that the charge density and its time derivative, referring to any pair of spatial points at a common time, are commutative. But this is impossible, if a lowest energy state—the vacuum—is to exist.' [90] The argument is very simple. If the stated commutator is zero,

$$[\mathrm{j}(\mathbf{x}, t), \rho(\mathbf{x}', t)] = 0, \tag{11.8}$$

then taking the divergence with respect to $\mathbf{x}$ and using current conservation,

$$\nabla \cdot \mathrm{j} + \frac{\partial}{\partial t}\rho = 0, \tag{11.9}$$

and the Heisenberg equation

$$\frac{\partial}{\partial t}\rho = \frac{1}{\mathrm{i}}[\rho, H], \tag{11.10}$$

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contributions to physics are becoming increasingly recognized, so that it is now not the difficulty, but the power of his techniques, that is emphasized.

* Robert Warnock recalled that as a consequence he had to have his thesis defense at Schwinger's home on Fayerweather Street.[36]

$H$ being the Hamiltonian, we have for any linear functional $F$ of the charge density

$$[[F, H], F] = 0. \tag{11.11}$$

If we take the expectation value of this equation in the vacuum, for which $H|0\rangle = 0$, we find

$$\langle 0|FHF|0\rangle = 0, \tag{11.12}$$

which cannot be true: The operator is positive definite, and the vacuum is in general not an eigenvector of $F$. Schwinger went on to show, in this one-page letter, that the electric field does not commute with the current at equal times,

$$\langle \mathrm{i}[E_k(\mathrm{x}, t), j_l(\mathrm{x}', t)]\rangle = \delta_{kl}\delta(\mathrm{x} - \mathrm{x}')K^2, \tag{11.13}$$

where $K$ is a constant with dimensions of mass. From Gauss's law, then, we obtain the form of the equal-time commutation relation between charge density and current, instead of the zero value postulated in Eqn (11.8),

$$\langle \mathrm{i}[j^0(\mathrm{x}, t), j_l(\mathrm{x}', t)]\rangle = \nabla_l\delta(\mathrm{x} - \mathrm{x}')K^2. \tag{11.14}$$

Schwinger concluded by constructing $K^2$ in terms of a Dirac-field bilinear in the limit as the displacement between the field points, $\epsilon$, goes to zero. For non-interacting fields, $K^2$ is given as the divergent limit

$$\frac{2}{3}\frac{e^2}{\pi^2}\frac{1}{\epsilon^2}, \qquad \epsilon \to 0. \tag{11.15}$$

Schwinger remarked later, 'This is the beginning of the inspection, you might say, of extended structures. Although, at this time, this was only to be finally of interest in the limit. But it was recognized that products of field operators at the same point are mathematically undefined. Spin-$\frac{1}{2}$ has exceptional properties that are being overlooked or mistreated, and the only way to emphasize them is to look at non-local products first. A very fundamental thing. And it waited until 1959—hard to believe. I think one could say this is the beginning of my overt dissatisfaction with operator field theory.'[8]

Commutators of structures composed from the field operators continued to occupy much of Schwinger's interest in the next few years. As we will see, in the next section and the following chapter, the consistency of non-Abelian gauge theories was a primary concern of his in the early 1960s. In the course of that investigation he showed, in a special case, the following commutation relations between the 00 components of the energy–momentum tensor, the energy density, for equal times,

$$-\mathrm{i}[T^{00}(x), T^{00}(x')] = -[T^{0k}(x) + T^{0k}(x')]\partial_k\delta(\mathbf{x} - \mathbf{x}'). \qquad (11.16)$$

This was the analog of the well-known equal time commutator of charge densities,

$$[j^0(x), j^0(x')] = 0. \qquad (11.17)$$

In fall 1962 Schwinger wrote 'Commutation relations and conservation laws' [111] in which he derived the latter, in general, by using the quantum action principle to see the response of the system to an external electromagnetic field, and then derived the former commutator for the energy density from the response to an external gravitational field. The proof required a certain technical assumption, which he called time locality, about the independence of certain energy–momentum tensor components from the time derivative of the gravitational field for a special class of such fields. When was that condition true? In the following paper, 'Energy and momentum density in field theory' [112] written about a month later, Schwinger showed that this condition was true for spins 0, $\frac{1}{2}$, and 1, but not for fields of higher spins (he explicitly excluded the gravitational field from these considerations). In particular, there were additional terms in the $T^{00}$ commutator, involving fourth derivatives of delta functions, for spin-$\frac{3}{2}$.*

Schwinger regarded this work as extremely fundamental, and referred to it repeatedly in the next few years. For example, in the 1963 Belfer conference[10] he stated that time locality, a sufficient condition for relativistic invariance, permitted only spins 0, $\frac{1}{2}$, and 1. He also referred to Kenneth Johnson's recent efforts at making field theory finite:[38] 'Over this whole field, over the attempt to make practical calculations in the domain of field theory in the past ten or more years, has lain the dead hand of Källén's dictum[39] that one of the renormalization constants is infinite,' but Schwinger's belief then was that this was merely a reflection of perturbation theory, and that a non-perturbative approach, such as Johnson's, would show the way to a finite theory. At the time,

* Carl Bender recalled trying to discuss these consistency conditions with Schwinger as a student around 1966. (He had examined the consistency conditions for spin-$\frac{3}{2}$ fields in a term paper for Schwinger's field theory course.) Schwinger gave him little attention, referring him to Shau-jin Chang. The latter confirmed Bender's results, which were in contradiction with some of Schwinger's claims, but Schwinger took no notice, but did give him an A in the course.[37] Undoubtedly, by that time, Schwinger was on the point of giving up conventional field theory for his new source theory.

at least, that was not to be.* In short, Schwinger's view was that 'field theory was essentially unexplored.'

One of the last times Schwinger referred to this work was in his Nobel Lecture [132]. There he summarized his position succinctly. The commutation relation (11.16) was a sufficient condition for Lorentz invariance. 'Additional terms with higher derivatives of the delta function will occur, in general. But there is a distinguished class of physical systems, which I shall call local, for which no further term appears. The phrase "local system" can be given a physical definition within the framework we have used or, alternatively, by viewing the commutator condition as a measurability statement about the property involved in the response of a system to a weak external gravitational field. Only the external gravitational potential $g_{00}$ is relevant here. A physical system is local if the operators $T^{\mu\nu}$, which may be explicit functions of $g_{00}$ at the same time, do not depend upon time derivatives of $g_{00}$. The class of local systems is limited to fields of spin 0, $\frac{1}{2}$, 1. Such fields are distinguished by their physical simplicity in comparison to fields of higher spin. One may even question whether consistent relativistic quantum field theories can be constructed for non-local systems.'

It may be worth concluding this section by noting that the Standard Model, which at the end of the millennium is believed to describe matter and the forces of nature, only includes fundamental particles of spin 0, $\frac{1}{2}$, and 1. However, proposed unified theories which involve supersymmetry (which we will describe on pp. 521–522) do contain not only the graviton (spin 2) but the gravitino (spin $\frac{3}{2}$), which Schwinger had been the first to describe consistently in 1941 [25].

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* Schwinger's unhappiness with Källén may have had more than professional causes. We have already mentioned Källén's caustic rejection of Schwinger's spectral representations. About the same time, in 1956, while editing *Quantum electrodynamics* [83], a selection of papers outlining the development of the subject, Schwinger agreed to write a long article on 'The quantum theory of wave fields,' in the new *Handbuch der Physik*, being edited by Siegfried Flügge. He agreed to this onerous task because he saw this as a way to stimulate progress on 'The Book,' the definitive field theory treatise he had been trying to complete for years (but never did, although substantial portions were written). In 1957 Flügge wrote a polite, but dunning letter, saying that Pauli and Källén, whose articles were to have bracketed Schwinger's, were being rather unpleasant concerning Schwinger's slow progress. The *Handbuch* article was never completed, perhaps never begun. (Concerning this volume of the *Handbuch*, it was said that the real part was by Källén, and the imaginary part by Schwinger.[40]) As to the field theory book, sometime around the same period, Addison-Wesley wrote to Schwinger, mentioning the new Feynman and Hibbs book[41] (which, however, did not appear until 1965) on path integrals, prodding him, unsuccessfully, to complete his treatise.[10]

## Gauge invariance and mass

The key to Schwinger's success in solving quantum electrodynamics was the recognition of the key role played by gauge invariance. It was not enough to have a gauge-invariant theory, one must calculate gauge-invariant quantities by gauge-covariant methods [64]. Only in this way could one achieve meaningful results, and resolve unambiguously the divergence difficulties of the theory. Yet the apparently unshakeable consequence of this symmetry was the implication of the masslessness of the photon [52]. For electrodynamics, this was eminently satisfactory, and in perfect accord with experiment.* However, in the 1950s the concept of gauge invariance had been enlarged. Chen-Ning Yang and Robert Mills had proposed the relevance of non-Abelian gauge fields, that transformed according to a non-commutative group, rather than the commutative group of rotations in a plane, $U(1)$ transformations, that characterized electrodynamics.[42] That is, instead of the fermion field and the vector potential transforming as

$$\psi \to e^{ie\lambda}\psi, \quad A_\mu \to A_\mu + \partial_\mu \lambda, \tag{11.18}$$

where $\lambda$ is an arbitrary function, Yang and Mills proposed (in effect) thinking of the fermion field as a column vector, and the vector potential as a matrix, so that the fields transformed as (mostly simply written for an infinitesimal transformation) $\psi \to \psi + \delta\psi$, $A_\mu \to A_\mu + \delta A_\mu$, where

$$\delta\psi = ig\delta\lambda\psi, \quad \delta A_\mu = ig[\delta\lambda, A_\mu] + \partial_\mu\delta\lambda, \tag{11.19}$$

where $\delta\lambda$ is an arbitrary infinitesimal matrix. Shortly thereafter this idea was applied to the strong and weak interactions, and Schwinger himself, only three years later, was to propose that the weak forces between leptons and between hadrons were to be mediated by charged non-Abelian fields [82]. We shall give details of Schwinger's development of non-Abelian gauge theories, and the path he followed which ultimately led to the standard model of elementary particles, in the next chapter. But there was an immediate, obvious problem. If the gauge fields were to be identified with physical states, be they spin-1 strongly interacting mesons such as the $\rho$, or the intermediate bosons responsible for weak interactions, these particles would have to acquire very large masses, from roughly 1 to 100 GeV. How could this be reconciled with the gauge principle, which apparently necessitated masslessness?

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* The current upper limit on the photon mass is $2 \times 10^{-16}$ eV,[2] based on measurements of the magnetic field of Jupiter, a very small value indeed.

Schwinger first attacked this problem in the Abelian context. He wrote two brief papers in the summer of 1961, the first while he was visiting UCLA.* The physical basis for this investigation is given in the first paragraph of 'Gauge invariance and mass' [104]: 'Does the requirement of gauge invariance for a vector field coupled to a dynamical current imply the existence of a corresponding particle with zero mass? Although the answer to this question is invariably answered in the affirmative [52], the author has become convinced that there is no such necessary implication, once the assumption of weak coupling is removed. Thus the path to an understanding of nucleonic (baryonic) charge conservation as an aspect of a gauge invariance, in strict analogy with electric charge,[43] may be open for the first time.' Schwinger then went on to show that the vacuum expectation value of the product of vector potentials, or of the product of currents, can be characterized by a spectral function $B(m^2)$, which satisfies the sum rule

$$\int dm^2\, B(m^2) = 1. \tag{11.20}$$

The current fluctuations are not sensitive to the zero-mass part of the spectrum, which are therefore associated with a pure radiation field. Schwinger argued that under situations of suitably strong coupling, there might not be any zero-mass contribution, that is, '$m = 0$ disappears from the spectrum of $A_\mu$.'

In the last paragraph of this $1\frac{1}{2}$ page note, Schwinger used these conclusions to consider a 'particularly interesting situation of a vector field that is coupled to the current of nucleonic charge,' and thereby predicted an isosinglet, non-strange meson, with $J^{PC} = 1^{--}$, decaying into three pions. In proof he noted the discovery of such a particle, what is now called the $\omega$.[44]

He concluded this seminal paper with the words, 'The essential point is embodied in the view that the observed physical world is the outcome of the dynamical play among underlying primary fields, and the relationship between these fundamental fields and the phenomenological particles can be comparatively remote, in contrast to the immediate correlation that is commonly assumed.'

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* After driving to California via Florida and Mexico (as usual, by way of Parkersburg, West Virginia, and Cincinnati, where Clarice's brothers lived), the Schwingers (including Clarice's mother Sadie and their cat Galileo) spent the winter of 1961 at UCLA, living in Brentwood, and summer 1961 at Stanford. There they rented a house in Woodside where one was allowed to have one horse per acre; since their property had three acres they would have been allowed to have three horses. Clarice and the gardener walked toward the barn and he wanted to know if they had brought any horses, so Clarice said 'Oh, it was such a long, hard trip for the horses, and it was such a short time we didn't bring them.'[13] They did care for the chickens there, which produced the most expensive eggs: six all summer! Julian recalled he 'lived like a country gentleman in Stanford.'[8]

The following paper, 'Non-Abelian gauge fields. Commutation relations' [105], submitted from Stanford in August 1961, which we will discuss in some detail in the next chapter, was seen by Schwinger as the first step in extending this idea, that gauge invariance need not imply massless gauge bosons, to the non-Abelian regime. Neither Schwinger, nor anyone else, ever completed that demonstration, although 'dynamical mass generation' is the goal of many a theorist to this day.

### Travels and a new home in Belmont

In October 1961 Schwinger travelled alone to the 12th Solvay Conference in Brussels,* followed by a visit to Waterloo. It was highly unusual for Schwinger to travel to Europe without Clarice. (Although he also traveled alone to Dubna in 1964.) Schwinger flew to Paris, and then traveled by helicopter to Brussels.[8] (Schwinger had been invited to the 1948 Conference, but the invitation apparently arrived late, and he had no time to prepare.†) At the 1961 conference Schwinger played no role and did not give a report.[8] 'Certainly, I interacted socially with Oppenheimer, and I recall that we were put up in this utterly charming hotel.'[8]

In 1962 the Schwingers finally left Fayerweather Street and bought a house in Belmont. They were never able to afford a house in Cambridge, so they bought a very nice house in Belmont instead. For Clarice and her mother it was like moving to another world, but they adjusted successfully. They had a nice garden and beautiful trees. It was a 15-minute walk to the bus so it was not too inconvenient for Clarice, for in those days she did not drive, so it was important to be near public transportation.[13]

Clarice had learned early never to interrupt Julian while he was working. This was reinforced soon after they moved to Belmont. Sunday was sacrosanct for him. He had a class the next day, so Sunday was a very hard-working day for

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* Mehra incorrectly stated that he was unable to come, although he is pictured in the Conference photograph.[45] Schwinger had to request emergency funds from the National Science Foundation to attend.[10] He also attended the 1967 Solvay Conference.[45] Mehra's confusion arose because while writing his book,[45] he worked simultaneously on the reports of the eighth and twelfth Solvay Conferences because of their similar themes. At the eighth Solvay Conference (1948), Schwinger was invited, but the invitation did not reach him in time; Mehra mistakenly transferred Schwinger's absence from the 1948 Conference to the twelfth Solvay Conference (1961), and was greatly chagrined to discover his mistake when his book was published.

† 'I have a vague memory that at the time we were somewhere on a vacation up in Maine or New Hampshire and somehow that telegram never got to us, or when it got to us it was really too late.'[8]

him. The Schwingers never went out, so people began to come to them. She remembered distinctly one afternoon a cousin came with a young boy who had adored the aura of Julian. He had come especially to see Julian. But Clarice remembered that years before somebody had come to visit and Clarice had asked Julian to interrupt his work for just a moment to say hello. So he did. But she had broken the thread of his thought that day, so she never did it again. They had both paid a very high price for that hello. So when her cousin and her son came to visit, although everyone knew Julian was up in the study Clarice said not a word about Julian. The boy went home disappointed, but it was just not worth interrupting Julian.[13]

In May 1962 the Schwingers traveled to Leningrad, where Schwinger had a short, three-week, appointment as an exchange professor. Clarice recalled that she had a very good time. She remembered that one day while she and Julian were walking in a park they got hungry, and they bought some sandwiches. They loved the delicious white bread, spread with red caviar and butter. It was so delicious that they continued eating bread that way ever after.[13]

From Leningrad they went to Moscow, where they collected royalties on Julian's book; the Soviets were just beginning to pay royalties in those days. The afternoon before they left Moscow, they went to a publishing house, and sat around a huge table, drinking sweet tea. They were going to give Julian a large number of rubles. Clarice remembered asking her translator if they could have it in dollars, for she wanted to furnish her new kitchen. But they paid her no heed, they paid in rubles.

But Clarice did get sable. They went to GUM, where she began by buying a set of nested Russian dolls. Julian said they'd be there a lifetime if that was the way Clarice was going to spend the money. So they went up to the second floor and stood in a queue at the fur station. By then she had learned the word for sable, *sobla* and for 'how much is it.' So she stood in line and said '*sobla*' and the clerk came out with two little ratty furs, which Clarice refused. Then the clerk went back and got her a bigger one and Clarice would say, 'how much is it,' and she would tell her and Clarice would again say no, until finally she brought two quite nice ones. It cost just the rubles they had, so the Schwingers came home with two little dead animals wrapped up in newspaper in their suitcase. When they got back to Logan Airport in Boston and went through customs, they opened their bags and said that they had bought furs; the customs man said, 'mink?' Clarice said no, I think it's sable. So he called Joe, 'Joe, is this mink?' Joe said yes. Clarice said, 'I think its sable.' 'Mink,' said Joe, very firmly. So consequently, they did not have to pay any duty on it, because mink is much less expensive than sable. Clarice had it made into a charming boa, which she perhaps wore twice.[13] They had to return to Harvard at the end of May, for Julian received an honorary DSc from the University at Commencement in June.

Lowell Brown recalled the first presentation of the mass generation work, in summer 1962, when the Schwingers returned to Europe. 'The gauge invariance and mass work was first presented at the very first Trieste conference—the conference that Salam used as a political ploy to start his institute. The Schwinger students Ray Sawyer (I think) and David Boulware (I'm sure) and I were there. Schwinger's lectures were tape-recorded and then transcribed by some English girl who remarked "Who is this Texan that speaks in complete paragraphs?" '[31, 46] After Trieste, the Schwingers traveled to Zagreb and Bled, Yugoslavia, for a few days with Kenneth Johnson and his wife. Also, on this trip they went to the Rochester meeting in Geneva in July. Clarice recalled a memorable picnic, with Arthur and Janice Roberts, on the French side of border. In those days one had to go through customs. Because Julian's last visit had been to the Soviet Union, and he was not yet accustomed to speaking French, when they got to the border and the customs officer asked if they had anything in their trunk, Julian said 'Nyet.' Consquently, they all had to get out while the French police searched the car.[13]

However, the most memorable event of 1962 was their trip to Yucatan in January. It was one of the few trips that the Schwingers paid for everything with their own money. Clarice regarded it as a wholly joyous trip, no physics at all. They flew to Miami, then to Mexico City, and finally took the flight to Mérida. They visited the famous archaeological places, Uxmal and Chichén Itza. This trip sparked Julian's continuing fascination with archaeology and pre-Columbian culture.[13]

## The Schwinger model

It was 'Gauge invariance and mass. II' [108], wherein Schwinger acknowledged helpful conversations with Kenneth Johnson and Charles Sommerfield, that solidified the idea of the 'Schwinger mechanism' of mass generation in the eyes of the physics world. There he presented an exact solution of a simple model, massless electrodynamics in one space dimension, which is invariably referred to as the Schwinger model. There is, then, no transverse direction, so there are no physical photons. However, the solution of the theory shows the existence of non-interacting scalar bosons of mass $e/\sqrt{\pi}$. (In one space dimension, the electric charge $e$ has dimensions of mass.) This paper has sparked a vast literature, yet it is such a special case that it remains unclear whether something similar can occur in nature, with three dimensions and non-Abelian interactions. 'It's too utterly trivial.'[8] Moreover, as Schwinger noted, 'These simple examples are quite uninformative in one important respect. They do not exhibit a critical dependence upon the coupling constant.' Physicists continue to speculate about such critical behavior. Schwinger concluded with his own speculation and his view of the role of fields at that time: 'One could anticipate that the known spin-0 bosons, for example, are secondary dynamical manifestations of strongly coupled primary fields and vector gauge fields. This line of thought emphasizes that the question "Which particles are fundamental?" is incorrectly formulated. One should ask "What are the fundamental fields?" '[8]

A third paper in this series was started, entitled 'Gauge invariance and mass. III.'[10] There Schwinger paraphrased an argument attributed to J. C. Taylor presented at the International Conference on High Energy Physics (the Rochester Conference) that he attended in Geneva in 1962. If $\partial_k F^{0k} = j^0$ then one concludes that the long-range component of the field is proportional to the total charge. But if the mass is not equal to zero, the static field has finite range, therefore the charge is zero. 'The flaw in this reasoning is the implicit assumption that there are states of non-vanishing total charge. Vacuum polarization produces a partial compensation of charge, and the possibility of a complete charge compensation should not be overlooked. In that circumstance there would be no long-range field, and no suggestion of a massless particle. We must conclude that the dynamical conditions under which a zero-mass particle does not exist are also those for which any charge inserted in the system would be completely screened.' For some reason, this paper was never completed.

We close this section by noting that Schwinger had thus anticipated the two schemes by which physicists imagine that particles acquire masses, through what are now called 'dynamical symmetry breaking' or through 'vacuum expectation values' of scalar fields. The former is what we have just described, whereas the latter was one of the key elements in the 'Theory of fundamental interaction' [82], which we shall describe in the next chapter.

## Quantum gravity

From a modern perspective, the earliest example of a non-Abelian theory is gravity. That is, the gauge boson, the graviton, interacts directly with itself, unlike the photon in electrodynamics. We will discuss Schwinger's important work on non-Abelian gauge theories in the following chapter; here we turn to Schwinger's attempt to quantize gravity, a problem which has resisted the greatest minds up to the present.

'One of the obvious extensions of the non-Abelian gauge theory is to quantize the gravitational field itself. And you will observe two papers on that subject [113] and [114]. I think it was these papers that pushed me over the edge, the complexity that followed from the operator nature of all these fields simply said to me this is not the real physics, this is unnecessarily complicated. So it was just part of the inspiration, stimulus if you like, that finally produced source theory, if not for another few years.'[8]

Because the theory was not explicit, it was not merely difficult to perform calculations, but nearly impossible to verify consistency. 'Such a simple thing as verifying invariance could not be done because everything was implicit. . . . I was beginning to become very unhappy with operators as a fundamental formulation of things, thinking that they introduced difficulties of their own that had no counterpart in the actual physical situation. Just a psychological uneasiness now that's beginning to grow.'[8]

'Quantized gravitational field' [113], submitted to the *Physical Review* at the end of 1962, began with a remarkable statement of Schwinger's view of the connection between gravitation theory and gauge theories: 'Electrodynamics is characterized by the property of gauge invariance—the freedom to alter the phase of any charge-bearing field arbitrarily at each space–time point while subjecting the electromagnetic potentials to a corresponding inhomogeneous transformation. It is not surprising that Weyl, the originator of the electromagnetic gauge invariance principle, also recognized[47] that the gravitational field can be characterized by a kind of gauge transformation. This is the possibility of altering freely at each point the orientation of a local Lorentz coordinate frame while suitably transforming certain gravitational potentials. Such a transformation is quite distinct from the more familiar global coordinate transformation. In a subsequent development of this conception, Yang and Mills[42] introduced an arbitrarily oriented three-dimensional isotopic space at each space–time point thereby relating a hypothetical vector field to isotopic spin. (The occasional remark that the gravitational field can be viewed as a Yang–Mills field is thus rather anachronistic.)' The purpose of this paper was to quantize the gravitational field using the quantum action principle; Schwinger noted that there was contact with the similar semiclassical considerations of Arnowitt, Deser, and Misner,[48] mentioned in Chapter 10.

In this paper, Schwinger used the variables he had already used in [112], where he was considering an external gravitational field, namely the tetrads or vierbeins, $e_a^{\mu}$, and the spin connections, $\omega_{\mu ab}$. Here the Greek indices refer to the usual general coordinates of general relativity, and the Roman indices refer to components with respect to a local Lorentz frame. Because of the freedom to make local Lorentz transformations and arbitrary coordinate transformations, the field equations do not determine the time evolution of the system. Both types of transformations must be restricted. Restriction of local Lorentz transformations, a gauge condition, can be accomplished by 'locking the time axes of the local coordinate system to the time axis of the general coordinate system.' Once matter is introduced as a scalar field, the second restriction, the coordinate condition, can be enforced. In this way Schwinger was able to formulate a canonical theory of quantum gravity that was invariant under three-dimensional translations and rotations. However, in this first paper he was unable to verify Lorentz invariance because the energy density could not be explicitly expressed in terms of the dynamical variables, but only implicitly in terms of constraint equations. This 'could be a formidable barrier to verifying the consistency of the formalism.'

Stanley Deser later commented on Schwinger's work on gravity. 'If you look at Julian's gravity-related work of the early 1960s, the gravitational field is first—and mainly—used as a tool to develop the famous stress tensor commutation relations required for consistency of a field theory; these deep ideas were also being elaborated by Dirac. (Curiously, though, there is no mention of Schwinger terms, which he had invented in the current–current commutator context.) Here we see explicit statements of the difference between lower and higher spins, as well as a mastery of matter–gravity coupling technology in modern vierbein form. In a later paper comes the application to gravity as a dynamical system (rather than as an external field) formulated in terms of a set of canonical variables [similar] to those Arnowitt, Misner, and I[48] had developed.'[49]

'Quantized gravitational field. II' [114] was written in June 1963 while Schwinger was in France, at the Institut des Hautes Etudes Scientifiques, in Bures-sur-Yvette, near Paris. The purpose was to establish the consistency of quantum gravity interacting with matter in the form of integer-spin fields. The problem was that the generators of coordinate transformations were only known implicitly, so that it was non-trivial to verify that they satisfied the required commutation relations. Schwinger appealed to the method he developed two months previously, also at Bures-sur-Yvette, which 'combines physical operator variables with the mathematical group parameters' of infinite-dimensional transformation groups [109]. There he rederived known consistency results for Abelian and non-Abelian gauge theories. The method was not essential there. 'But when one turns to the problem of the gravitational field, it becomes the indispensable instrument for the discovery of a consistent formulation.' This was his purpose in [114]. How far did he succeed? If he made the plausible assertion that certain integrals, over surfaces receding to infinity, of operators vanish, he was able to derive formal verification of Lorentz invariance, in the form of the commutators,

$$-\mathrm{i}[P^0, J^{0k}] = P^k, \qquad -\mathrm{i}[J^{0k}, J^{0l}] = -J^{kl}, \tag{11.21}$$

where $P^\mu$ is the four-momentum operator, the generator of space–time displacements, and $J^{\mu\nu}$ is the angular momentum operator, the generator of rotations and boosts. But the result was inadequate. 'A much more careful examination will be required to test whether the loosely stated physical boundary conditions can be maintained as assertions about operators in relation to a class of physical states.'

### Travel and recognition

As we see, Schwinger completed his work on quantum gravity while he was on sabbatical in Paris in 1963. They rented out their house in Belmont to Murray and Margaret Gell-Mann. In 1963 both the Paul Martins and the Schwingers had a sabbatical in Paris. The Gell-Manns had already arranged to take the Martins' house. But the Martins had a young doctor friend with a family, visiting from Britain, who couldn't afford much and wanted to rent their house. Ann Martin called Clarice, and said she knew they didn't want to rent their house but wouldn't they please consider letting the Gell-Manns have it so that she could give her house to the British doctor. The Schwingers agreed, so the Gell-Manns ended up taking their house without seeing it.[13] Mary Farley, who had been Clarice's mother's housekeeper when Clarice was a little girl, took care of the day cleaning while the Gell-Manns were there, and the Gell-Mann children behaved when she was around. Clarice was pleased with the arrangement. The Gell-Manns enjoyed the Schwingers' house; in retrospect, the Schwingers were very lucky to have had somebody in the house that winter. From that experience they continued to rent out the house when they were away, although they had been determined not to have anybody in the house. It was wise not to leave the house empty during the cold New England winter.[13]

In Paris they stayed in the 16th Arrondissement. Schwinger found the apartment listed in the *Herald Tribune*, and a friend of the Bernard Felds, Leda Cassin, went to look at it for them. It was the apartment of an American journalist who was living in Paris. Although it was stuffy living in the 16th Arrondissement, it was very comfortable and pleasant.[13]

In May 1963 they traveled from Paris to Israel. Clarice was initially unhappy because she feared that she would miss the Parisian chestnut blossoms, but it was a cold winter and the blossoms waited for her return. In Israel they mostly stayed at the Weizmann Institute in Rehovot, but did visit the Giulio Racahs in Jerusalem. Then they went to Greece for a mere two days. In Delphi Clarice was impressed by the statue of the Charioteer. One of the things Clarice remembered about Athens was a little amphitheater, where they encountered a tour group which had left two young girls behind. They were taking a photo of each other; when the tour leader came back for them they explained that they just wanted to take pictures. 'Take your time. Take three minutes.' It became a family joke—'Take your time, take three minutes.'[13]

On the way back to Paris they apparently stopped in Rome for a few days, where Julian bought a blue Flavia Lancia. His first and last cars were red. Clarice believed that all the others in between were blue,[13] but it seems that his first Cadillac was black.

They returned to America by boat, the *S.S. Statendam*, from Southhampton, so they spent some time before that in England. After staying in London, where they visited David Saxon and Harold Levine, who had an apartment there, they visited Stonehenge, where they had a 'mystically' wonderful time, Oxford, Glastonbury, and Blenheim Castle.[13]

On 8 February 1965 Schwinger was awarded the National Medal of Science. The citation read: 'The National Medal of Science for 1964, awarded by the President of the United States of America to Julian Schwinger for profound work on the fundamental problems of quantum field theory, and for many brilliant contributions to and lucid expositions of nuclear physics and electrodynamics. Signed Lyndon B. Johnson.' Both mothers and Julian's brother Harold came to the ceremony at the White House.

In the summer of 1965 the Schwingers again went to Trieste, this time to visit the International Centre for Theoretical Physics which had now been established by Abdus Salam the year before. They went to Trieste via Rome, where Julian picked up his *Iso Revolta*, nicknamed *Sophia Loren*. She was a beautiful car, Mediterranean Blue, curvaceous, and sexy. A man almost got killed watching her while crossing the street in Gibraltar. Schwinger bought the *Iso* because it had a *Corvette* engine, thinking that would simplify service in the United States; it did not. After that, the Schwingers went to the meeting in Feldafing, Austria. Clarice recalled that that was when she and Dirac became friends. They went for walks without saying a word. She remembered walking in the rain with Dirac. She also remembered Heisenberg because after the meeting they went to Munich for a few days with Hans and Marlis Mitter. The Heisenbergs had a party at their house, which was the first time Clarice consciously remembered Heisenberg as who he was—not as a persona, but as a person. He was handsome, warm, and friendly and played the piano very well.[13] Afterwards, they visited Cologne, Aachen, and Paris, and then returned home on the *S.S. Mauritania*.

## Magnetic charge

P. A. M. Dirac had shown in 1931 that magnetic charge was consistent with quantum mechanics provided the strength of the magnetic charge $g$ bore a special relation to the strength of any other electric charge $e$ in the universe.[50] This is the famous Dirac quantization condition,

$$eg = n\frac{\hbar c}{2}, \tag{11.22}$$

where $n$ is an integer. Julian Schwinger read this paper within a year or two of its appearance, at the tender age of 16. He was immediately struck by the beauty of the idea, although he did nothing with it for some time.

One of the first appearances of the concept of magnetic charge in Schwinger's work was in 1946, when he was writing up his volume summarizing his researches on waveguides at the MIT Radiation Laboratory. This book, which he envisaged as a self-contained treatise on electromagnetic theory, was never completed, but in Chapter 2 he used magnetic currents, and duality, as a mathematical tool to simplify the derivations,[10] as had Bethe earlier.[51] The point is, when in addition to electric charge densities and current densities, $\rho_e$ and $\mathbf{j}_e$, there are also magnetic charge and current densities, $\rho_m$, $\mathbf{j}_m$, Maxwell's equations become, in Gaussian units, in vacuum

$$\nabla \cdot \mathrm{E} = 4\pi\rho_e, \tag{11.23}$$

$$\nabla \cdot \mathrm{B} = 4\pi\rho_m, \tag{11.24}$$

$$\nabla \times \mathrm{B} = \frac{1}{c}\frac{\partial}{\partial t}\mathbf{E} + \frac{4\pi}{c}\mathbf{j}_e, \tag{11.25}$$

$$-\nabla \times \mathrm{E} = \frac{1}{c}\frac{\partial}{\partial t}\mathrm{B} + \frac{4\pi}{c}\mathbf{j}_m. \tag{11.26}$$

These equations are invariant under the symmetry called duality: for any electric quantity $\mathcal{E}$ (such as E, $\rho_e$, $\mathbf{j}_e$), and any magnetic quantity $\mathcal{M}$ (such as B, $\rho_m$, $\mathbf{j}_m$), Maxwell's equations are invariant under the rotation ($\theta$ is a constant)

$$\mathcal{E} \to \mathcal{E}\cos\theta + \mathcal{M}\sin\theta, \tag{11.27}$$

$$\mathcal{M} \to \mathcal{M}\cos\theta - \mathcal{E}\sin\theta. \tag{11.28}$$

Because of the symmetrical form of Maxwell's equations, it becomes far more transparent how to derive conservation laws, such as that for the conservation of energy or momentum. [231]

Schwinger first discussed magnetic charge seriously at the end of his field theory period. In 'Magnetic charge and quantum field theory' [129], which he submitted to the *Physical Review* in November 1965, and in a lecture at the Coral Gables conference the following January [130], Schwinger addressed the issue of 'the compatibility of the magnetic-charge concept with the principles of relativistic quantum field theory'; unlike its compatibility with quantum mechanics, the latter had not previously been seriously examined. The issue was whether the theory was relativistically invariant, which, as we saw on pp. 391–392, was given by the condition (11.16).

It is necessary to introduce a vector potential in order to define a canonical theory, in which the transverse electric field and the transverse vector potentials are conjugate variables, or in order to define the energy density. But B cannot be $\nabla \times \mathrm{A}$ because $\nabla \cdot \mathrm{B} = 4\pi j_m^0$, the latter being the magnetic charge density. It can be true almost everywhere, however; the vector potential can be chosen to be singular along a semi-infinite or infinite line, which is sometimes referred to as a *string*. Schwinger chose a vector potential singular on the infinite straight line characterized by the unit vector n; for a point charge at the origin, he took

$$\mathrm{A_n} = \frac{1}{2|\mathrm{x}|}\left(\frac{\mathrm{n}\times\mathrm{x}}{|\mathrm{x}|+\mathrm{n}\cdot\mathrm{x}} - \frac{\mathrm{n}\times\mathrm{x}}{|\mathrm{x}|-\mathrm{n}\cdot\mathrm{x}}\right). \tag{11.29}$$

This string is completely unphysical, and the change from one singularity line to another is a kind of singular gauge transformation. 'Only if $eg = n$, where $n$ is an integer, can the gauge transformation be unique, almost everywhere.' This result differed from Dirac's, in that he permitted integer plus $\frac{1}{2}$ values for this quantity; this discrepancy is attributed to Schwinger's use of an infinite singularity line, rather than Dirac's semi-infinite one.

Schwinger next considered angular momentum. He first established the static field contribution to the angular momentum,

$$-\int (\mathrm{dx})(\mathrm{dx}')j_e^0(x)\frac{\mathrm{x}-\mathrm{x}'}{|\mathrm{x}-\mathrm{x}'|}j_m^0(x'), \tag{11.30}$$

which agreed with the familiar $-eg\hat{\mathrm{r}}$ for point electric and magnetic charges.[52] Then he provided a further argument in favor of integer quantization: if, for example, we wish a spin-$\frac{1}{2}$ field to change sign under a $2\pi$ rotation, integer quantization seems necessary. Finally, Schwinger demonstrated that a system containing electric and magnetic charge is relativistically invariant in which 'localized field operator products must be understood as the limit of products defined for non-coincident points' [90]. Then, precisely because of the charge quantization condition, or equivalently, because

$$\exp 2\pi eg = 1, \tag{11.31}$$

the required commutation relations are satisfied. He closed with an appeal to a non-perturbative stance, with a dig at most of his colleagues. 'This discussion shows clearly how relativistic invariance will appear to be violated in any treatment based on a perturbation expansion. Field theory is more than a set of "Feynman's rules."' His lifelong belief in the relevance of magnetic charge was now expressed: 'The relativistic quantum field theory of magnetic and electric charge is of such beauty that we must repeat after Dirac: "One would be surprised if Nature had made no use of it."'

As a result of conversations with Bruno Zumino, Schwinger added an interesting *Note added in proof* to this paper. The issue was that of the 'Dirac veto' 'who expressly forbids an electrically charged particle to lie on the singularity line ("string") associated with a magnetically charged particle.' Schwinger's analysis of this situation resulted in a doubling of the quantization condition:

$$eg = \begin{cases} n & \text{semi-infinite string,} \\ 2n & \text{infinite string.} \end{cases} \tag{11.32}$$

This was not Schwinger's last word on this subject, as we shall see in Chapter 14.

Schwinger referred to this work some two weeks later in his Nobel Lecture, delivered on 11 December 1965 [132]. He mentioned it in connection with the relativistic invariance condition expressed in terms of the energy density commutator. 'Dirac pointed out many years ago that the existence of magnetic charge would imply a quantization of electric charge, in the sense that the product of two elementary charges, $eg/\hbar c$, could assume only certain values. According to Dirac, these values are any integer or half-integer. In recent years, the theoretical possibility of magnetic charge has been attacked from several directions. The most serious accusation is that the concept is in violation of Lorentz invariance. This is sometimes expressed in the language of field theory by the remark that no manifestly scalar Lagrange function can be constructed for a system composed of electromagnetic fields and electric and magnetic charge-bearing fields. Now it is true there is no relativistically invariant theory for arbitrary $e$ and $g$, so that no formally invariant version could exist. Indeed, the unnecessary assumption that $\mathcal{L}$ is a scalar must be relinquished in favor of the more general possibilities that are compatible with the action principle. But the energy commutator condition can still be applied. I have been able to show that energy and momentum density operators can be exhibited which satisfy the commutator condition, together with the three-dimensional requirements, provided $eg/\hbar c$ possesses one of a discrete set of values. These values are integers, which is more restrictive than Dirac's quantization condition. Such general considerations shed no light on the empirical elusiveness of magnetic charge. They only emphasize that this novel theoretical possibility should not be dismissed lightly.'

In May 1966 Schwinger submitted two more papers on magnetic charge, 'Electric- and magnetic-charge renormalization. I' and 'II' [133, 134]. As the title implies, here he investigated the somewhat subtle issue of the renormalization of electric and magnetic charge. His mature view of renormalization was that it expressed the relation between the particle level of description and the more fundamental field level. At both of these levels, one should have charge quantization,

$$\begin{aligned} e_0 g_0/\hbar c &= n_0 = 0, 1, 2, \ldots, \\ eg/\hbar c &= n = 0, 1, 2, \ldots. \end{aligned} \tag{11.33}$$

The first equation refers to the unrenormalized, the second to the renormalized, charges. The question was whether electric and magnetic charges were renormalized by the same, or a different, factor. Schwinger had already argued the former in his Coral Gables lecture [130], 'asserting that charge renormalization is a property of the electromagnetic field, not of any specific entity that interacts with it' [133]. In this paper, Schwinger explicitly verified this assertion. He did so by evaluating 'the long-range interaction of static charges, as modified by vacuum polarization phenomena,' and showing that

$$\frac{e}{e_0} = \frac{g}{g_0} = C < 1, \tag{11.34}$$

where, by virtue of the set of renormalization conditions (11.33), $C^2$ must be the ratio of two integers, $C^2 = n/n_0$. The Coulomb energy of interaction between electric, and between magnetic charges, were renormalized by the same factor, as was the (radial directed) angular momentum

$$K = eg = \int (\mathrm{dx}) j_e^0(x) \int (\mathrm{dx}') j_m^0(x'). \tag{11.35}$$

The second paper in this set [134], submitted less than a week after the first, provided further evidence for this universal charge renormalization. The reason for this follow-up note seemed to be indicated by the last remark of [133], namely that the definition of charge was given by the long-range interaction between quasistatic specified charges, and not through the expectation values of the total charge operators. So in [134] Schwinger turned to the interaction of an arbitrarily weak external electric current with a system composed of dynamical electric and magnetic currents, and the electromagnetic field. Use of the quantum action principle led to the expected interaction energy between quasistatic external sources:

$$E_{12} = C^2 \int (\mathrm{dx})(\mathrm{dx}') J_1^0(\mathbf{x}) \frac{1}{|\mathbf{x} - \mathbf{x}'|} J_2^0(\mathbf{x}'), \tag{11.36}$$

where $C$ was the renormalization factor given above. The analogous formula was derived for the interaction between current densities. He further showed that the conventional radiation formula emerged in terms of renormalized prescribed currents.

This paper was the last one Schwinger was to write on operator field theory. Less than two months later, he penned his first source theory paper [135], which we shall describe in Chapter 13. But this revolution did not in any way diminish Schwinger's growing fascination with magnetic charge. We shall recount Schwinger's further contributions to the theory of magnetic charge, which remain of great importance in a poorly developed field, in Chapter 14.

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# Electroweak unification and foreshadowing of the standard model

Much has been written about the development of the 'standard model' of particle physics, in particular about the convoluted process which led to the theory of electroweak interactions for which, ultimately, Sheldon Glashow, Steven Weinberg, and Abdus Salam received the Nobel Prize in 1979. A particularly important chapter in this story is the discovery of the $V - A$ structure of the weak interactions. Controversy still exists concerning priority of this discovery. But undoubtedly Schwinger's major role in this history has not yet been fully described. In particular, it is clear that Schwinger was the grandfather of the electroweak synthesis.

In fact, Schwinger recalled, much later, that in the early 1940s he had anticipated the notion of the intermediate weak boson, and the unification with electromagnetism, but it was not much appreciated. 'Here is an anecdote of 1941, unattested and unfortunately now unattestable. I had been thinking about Fermi's theory of $\beta$-decay, wherein appears a very small coupling constant of order $10^{-12}$. It occurred to me that the electron mass, then used as the significant mass scale, was not necessarily the relevant quantity. The neutron and the proton were also involved, and possibly the nucleon mass was the appropriate unit. On introducing it, the coupling constant became of order $10^{-5}$. And then I thought perhaps the really significant mass unit is several tens of nucleon masses, for then the coupling constant could be the electromagnetic coupling constant $\alpha = 1/137$. One day I mentioned this bit of numerology to Oppenheimer. He stared at me, and then said coldly, "Well, it's a new idea." Indeed it was, and it is.' [197]

## A brief history of weak interactions

The subject of weak interactions goes back to the discovery of radioactivity by Henri Becquerel in 1896,[1] followed closely by Marie Curie, who discovered the new radioactive elements polonium and radium.[2] What was soon dubbed $\beta$-decay was the process by which the nucleus of an atom transformed from one species to another, with the emission of a beta-ray, what ultimately turned out to be identical to the electron of the atom. Because it was soon discovered that the electrons were not emitted with a unique energy, the decay could not be a two-body one, and Wolfgang Pauli proposed in 1930[3] that a massless neutral particle, having nearly no interaction with matter, the *neutrino*, was emitted as well. The fundamental example of beta-decay, then, is the decay of the neutron (discovered in 1932 by Chadwick[4]), into a proton, an electron, and a neutrino,

$$\mathrm{n} \rightarrow \mathrm{p} + \mathrm{e}^{-} + \bar{\nu}. \tag{12.1}$$

The bar signifies an antineutrino; we anticipate the concept of *leptonic charge* (or lepton number), $L$, where $L = +1$ for the electron and neutrino, and $L = -1$ for the positron and the antineutrino; as far as we know, leptonic charge is exactly conserved, so an electron and an antineutrino must be produced together in a weak decay, just as electric charge conservation requires that the positively charged proton be accompanied by the negatively charged electron in this decay.*

Another strand in the weak interaction story involved a confusion with the strong interactions, responsible for holding the nucleus together.† In 1934 Hideki Yukawa proposed[6] that the strong force was mediated by the exchange of a (scalar) particle called a mesotron (later the term was shortened to meson). Just as electromagnetic forces are understood quantum-mechanically through the interchange of the massless helicity-1 photon, which gives rise to a long range Coulomb force, or a potential, falling off with distance $r$ as $1/r$, the exchange of a mesotron of mass $m$ would give rise to a short range potential,

$$V_s \propto \frac{1}{r} \mathrm{e}^{-mr}, \tag{12.2}$$

the range of the force being the Compton wavelength of the exchanged particle $\lambda_c = \hbar/mc$. (In the formulas we use natural units, where $\hbar = c = 1$.) Thus, since it was clear that nuclear forces had a range of only about 1 fermi $= 10^{-15}$ m, the mass of the mesotron must be of the order of 100 MeV, much heavier than the electron, but small compared to the proton mass, 940 MeV.

Within a few years a particle, dubbed the $\mu$ meson, now the muon, was discovered[7] in cosmic rays that seemed to be Yukawa's particle. It had about the

* The concept of leptonic charge was apparently introduced by Konopinski and Mahmoud[5] in 1953, but the notion of two neutrinos awaited Schwinger's work in 1957, which we will describe below.

† We have seen how this confusion affected Schwinger's work in the late 1930s in Chapter 3.

right mass, 106 MeV, and was copiously present. But something was wrong. It lived much too long, 2 microseconds, 100 times longer than Yukawa's particle should have. Worse yet, in 1946 it was discovered[8] that this mesotron interacted very weakly with nuclei, not strongly, as the carrier of the strong force must. (It was already known that the mesotron had great penetrating power, and could travel from the upper atmosphere, where it was produced, to sea level.) Fortunately, within a year or so, the $\pi$ meson, or pion, was discovered,[9] which had a short life, a bit heavier mass (139 MeV), and strong interactions. The muon turned out to be just a much more massive replica of the electron—as Rabi said, 'who ordered that?'*

The nature of the interaction responsible for beta-decay was to prove much more difficult to unravel than the existence of the neutrino and the muon. Enrico Fermi started off brilliantly in 1933 with his theory of the four-fermion interaction,[11] directly describing the beta-decay process, with one overall coupling constant, the Fermi coupling $G_F$. He explicitly motivated the idea by analogy with electromagnetism, where the force between charged particles (electrons) is due to an exchange of photons between electric currents. Those currents are bilinear in the electron fields $\psi$, so that, schematically, the interaction is described by the Hamiltonian

$$H_{em} = e^2 \int (\mathrm{dr})(\mathrm{dr}')\bar{\psi}(\mathrm{r})\gamma^\mu\psi(\mathrm{r})\frac{1}{|\mathrm{r}-\mathrm{r}'|}\bar{\psi}(\mathrm{r}')\gamma_\mu\psi(\mathrm{r}'), \tag{12.3}$$

which is constructed from the vector current $J^\mu = \bar{\psi}\gamma^\mu\psi$, where $\gamma^\mu$, $\mu = 0, 1, 2, 3$, are the Dirac matrices. If the interchanged particle were massive, the Coulomb potential would be replaced by a Yukawa potential, as we saw above, and indeed, if the mass were very great compared to the energies involved in the process (just a few MeV, typically), the interaction between currents would effectively be at a point,

$$H_w = \frac{G_F}{\sqrt{2}}(\bar{p}\Gamma_i n)(\bar{e}\Gamma_i \nu), \tag{12.4}$$

where we now have let the particle names represent the fields. (The square root of 2 is a matter of convenience.) The problem was, what was the nature of the currents? These are given by the Lorentz transformation properties of the matrices $\Gamma_i$. The simplest possibility was vector currents as in electromagnetism, in which case $\Gamma_i = \gamma_\mu$, yet that was not to be the case. Already in 1936, George Gamow and Edward Teller observed there were five interaction terms possible[12], scalar (S), corresponding to $\Gamma_1 = 1$, vector (V), corresponding to

* For a brief account of this history, see Ref. 10.

$\Gamma_2 = \gamma_\mu$, tensor (T), corresponding to $\Gamma_3 = \gamma_\mu\gamma_\nu$, axial-vector (A), corresponding to $\Gamma_4 = \gamma_5\gamma_\mu$, and pseudoscalar (P), corresponding to $\Gamma_5 = \gamma_5$.* It was therefore a difficult experimental problem to discover which combination of these interactions nature chose to describe weak processes.

Indeed the experiments were subtle and errors were made. By the mid-1950s, by one count,[13] there were, as it would soon turn out, 13 wrong experiments. These seemed to lead unequivocally to the conclusion that the weak interaction Hamiltonian was a combination of scalar and tensor terms. This would prove to have a significant effect of delaying theoretical understanding of weak interactions. But an even more profound revolution lay ahead.

This had to do with what was called the $\theta$–$\tau$ puzzle. To describe this, we need to recount the discovery of 'strangeness.' In 1947 Rochester and Butler[14] discovered long-lived particles in cosmic rays that decayed in a cloud chamber. These were called at the time $V$ particles (from the shapes of the resulting tracks), but are what we now know as $K^0$ and $K^+$, with a mass 1000 times that of the electron, just below 500 MeV. Many physicists found this proliferation of new particles distasteful, but there they were. During the next few years, other particles in the same class were found. Notable among these was the neutral $\Lambda^0$, a baryon like the proton or neutron. These particles could be produced strongly, but only together; for example, in a proton–proton collision, one could produce a $\Lambda^0$ and a positive kaon,

$$\mathrm{pp} \to \Lambda^0 K^+ \mathrm{p}. \tag{12.5}$$

That one had to produce two of these new particles together was dubbed associated production. On the other hand, these new particles were relatively long-lived: the $\Lambda$ baryon lives $2.6 \times 10^{-10}$ s, far longer than a characteristic strong decay time of $\sim 10^{-23}$ s, which is, for example, the lifetime of the first nucleon resonance, the $\Delta(1232)$. It was soon recognized that these two facts could be explained by assigning to these new particles a new quantum number, what Gell-Mann would call strangeness.[15] Thus, the ordinary particles, the proton, neutron, pion, etc., have strangeness zero, $S = 0$; the $K^+$ and $K^0$ have $S = +1$, and the $\Lambda$ has $S = -1$. Strangeness is conserved by the strong interactions, so $K^+$ and $\Lambda$ must be produced together. Weak interactions violate strangeness, so the strange particles decay weakly, that is slowly.

This was all very pretty. But the difficulty lay in the decays of the kaon. Some 20% of the time $K^+$ decays into two pions, while 7% of the time it decays into three pions. This cannot be if parity, space reflection, is conserved. So, for a

* $i\gamma_5$ is the chirality operator, with eigenvalue $\pm 1$ depending on whether the spin is parallel to, or antiparallel to, the direction of the fermion's motion. In terms of gamma matrices, $\gamma_5 = \gamma^0\gamma^1\gamma^2\gamma^3$.

while it was thought that there were two distinct particles, $\tau$ which decayed into three pions, and $\theta$ which decayed into two.* Of course, the identity of the masses and lifetimes of $\tau$ and $\theta$ soon made this hypothesis untenable. But it was nearly unthinkable to give up parity conservation, since it seemed as self-evident as rotational invariance. It took T. D. Lee and C. N. Yang to point out that, in fact, there were no tests of parity conservation in weak interactions, and to propose specific experiments to see whether reflection invariance was a good symmetry or not.[16]

## 'The dynamical theory of $K$ mesons'

Schwinger at first had difficulty in accepting the concept of parity violation, which, as we have just noted, had been proposed by Lee and Yang[16] to explain the $\tau$–$\theta$ puzzle. In fact he claimed he had a proof that parity could not be violated,[17] based on an assertion that it was merely a coordinate redefinition, and hence it was meaningless to talk of its breaking. This idea was the basis of the 'The dynamical theory of $K$ mesons' [81], which proposed the existence of hypercharge $Y$, 'with $Y = +1$ characterizing the $K^+K^0$ multiplet, and $Y = -1$ describing the antiparticles $\bar{K}^0K^-$. ... The slow disintegration of $K$ particles into $\pi$ mesons thus implies that, unlike electrical and nucleon charge, hypercharge is not absolutely conserved.' Thus, with these words, Schwinger introduced the modern concept of hypercharge† (denoted by the symbol $Y$), which is connected with the electric charge $Q$ and the third component of isospin $T_3$ by

$$Q = T_3 + \frac{1}{2}Y. \tag{12.6}$$

It is related to the 'strangeness' quantum number introduced by Nishijima, Gell-Mann, and Pais[15] by $S = Y - N$, where $N$ is the nucleonic charge. So far, so good. But now Schwinger's bias against the possibility of parity violation shows. 'In remarkable contrast with these arguments for the hypothesis that $K$ mesons possess an interaction with the $\pi$ field that is analogous to the pion–nucleon coupling, there must now be placed the fact that the $K$ particles with

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* Actually, the scenario envisaged at the time was rather more complicated than that. See Ref. 10.

† The editor of *Physical Review* at the time, S. Pasternack, would object to this terminology in the following paper 'On a theory of fundamental interactions,' [82] with the remark that 'hypernumber' or 'hyperonic number' was better than 'hypercharge;' Schwinger thus withdrew the paper from the *Physical Review*, and sent it to *Annals of Physics*, newly inaugurated and edited by his good friend Herman Feshbach of MIT, instead. In this paper Pasternack let this terminology pass, but objected to 'isospin' and 'isovector.'[18] Schwinger's terms in all these cases have stuck.

a definite intrinsic parity and spin 0 (as the energy distributions in the $3\pi$ decay indicate) cannot interact in this manner with the pseudoscalar $\pi$ field. Expressed in terms of a spin-0 field $\phi_K$, and omitting the necessary isotopic spin matrices, this assertion is simply the observation that $\phi_K\phi_K$ is necessarily a scalar and cannot be a pseudoscalar. Then, if we insist upon a coupling of the form $\phi_\pi\phi_K\phi_K$, we can only conclude that the intrinsic parity of the $K$ particle is a dynamical quantity capable of assuming either value, $+1$ or $-1$.' [81] As a result of rapid parity fluctuations between the $K$ particles, Schwinger argued that it was not the parity eigenstates, $\phi_{K\pm}$, that are relevant, but

$$\phi_{K1} = \frac{1}{\sqrt{2}}(\phi_{K+} + \phi_{K-}), \quad \phi_{K2} = \frac{1}{\sqrt{2}}(\phi_{K+} - \phi_{K-}). \tag{12.7}$$

Schwinger then asserted that the masses and lifetimes of the two states $K_1$, $K_2$ are identical (although in an *Added note*, composed while Schwinger was at Stanford in the summer of 1956, he had to justify this point at great length, and point out possible ways of observing discrepancies in the lifetimes, in analogy with the CP eigenstates $K_L$ and $K_S$ of the neutral $K$ meson), and hence explain 'the physical decay of the $K$ particle yielding 2 or 3 $\pi$ mesons, the $\theta$ and $\tau$ modes.' [81]

So although there is much that was, and remains, useful in this paper, originally titled 'Properties of $K$ mesons,' and submitted as a letter to *Physical Review* (but published as a regular article because of it being 'extremely long'[18]), its central thesis was erroneous. We now know that the kaon, as the $K$ meson is now called, is a pseudoscalar, like the pion, but that the weak interactions violate parity maximally, so that sometimes $K \to \pi\pi$, a decay into a even parity state, and sometimes $K \to \pi\pi\pi$, a decay into an odd parity state. The difference in branching fractions is purely kinematical, because there is more phase space available for a two-body decay than a three-body one. But Schwinger's error was a reflection of the times. Weak interactions were just beginning to be understood in the mid-1950s, as witness the fact that Schwinger pointed out that although the decay of the pion into a muon and a neutrino, $\pi \to \mu\nu$, was well-known, $\pi \to e\nu$ was not observed, and therefore 'we shall conclude that the $\mu$, $\nu$, and the e, $\nu$ fields are associated with different kinds of interactions, the muon being coupled to heavy B.E. [Bose–Einstein] particles and the electron to F.D. [Fermi–Dirac] fields, in the manner of the Fermi $\beta$-decay interaction. (The hope persists, of course, that this interaction can be ascribed to some coupling of the e, $\nu$ field with a boson field.)' [81] This hope, and the universal $(V - A)$ $\beta$ interaction, is of course realized in the standard model, to which Schwinger pointed in the following paper. (In fact, there Schwinger explained the suppression of $\pi \to e\nu$ mode.)

Schwinger later explained his reluctance to believe in parity violation with these words: 'I think I was so pleased by the symmetry concepts, invariance under rotations, Lorentz transformations, reflections, and so forth, that [I could not accept] that nature could be so mischievous as to destroy one of them. When it was proved experimentally, I accepted it immediately. But before, when it was at a speculative stage, I regarded it as a somewhat improbable speculation. One has the right to [resist], but you don't have the right to deny when it becomes a certainty.'[19]

It was not until the first few months of 1957 that Schwinger accepted the violation of parity. This was the result of overwhelming evidence provided by the experiment performed by Chien-Shiung Wu. 'As she told the story, in early spring 1956, Tsung-Dao Lee and Chen-Ning Yang were exploring the possibility that a single particle (the $\tau$–$\theta$) might decay into states of both odd (three-pion) and even (two-pion) parity, leading them to question whether parity was truly conserved in the weak interaction. Non-conservation of parity might be demonstrated, they reasoned, by measuring a pseudoscalar quantity such as the product of the nuclear spin and the linear momentum of the electron in beta-decay, averaging over all decays. This product should be zero (symmetric decay with respect to the spin axis) if parity is conserved, but non-zero otherwise. Wu suggested to Lee that instead of using either polarized nuclei produced during nuclear reactions or a polarized slow neutron beam from a reactor, "the best bet would be to use a $^{60}$Co beta source polarized by adiabatic demagnetization, by which one could attain polarization as high as 65%." And at a time when there was no guarantee that parity non-conservation might be possible (in total violation of conventional wisdom), Wu dedicated herself to actually doing the experiment.

'Wu's group at Columbia and her intimate knowledge of beta-decay gave her the confidence to overcome the problem of beta counting from a source at millikelvin temperature. On 4 June 1956, she called Ernest Ambler at the National Bureau of Standards (NBS), who enthusiastically accepted her proposition that the experiment be done at NBS using the bureau's expertise in nuclear orientation and equipment. Their manuscript submitted to *Physical Review* on 15 January 1957 demonstrated near-maximal violation of parity conservation. It was published 40 years and one day before her death as "Experimental Test of Parity Conservation in Beta Decay"[20] with Ambler, Raymond Hayard, Dale Hoppes, and Ralph Hudson of NBS as co-authors. Wu and the NBS group were thus the first to establish the non-conservation of parity and the violation of particle–antiparticle charge-conjugation symmetry in physics, forever altering our view of the universe.'[21]

In view of this definitive experiment, Schwinger had to concede that parity was violated maximally in weak interactions. Indeed in the lectures upon which the following 'Fundamental interactions' paper was based, given at Harvard and MIT during October through December of 1956, Schwinger continued to insist on parity conservation. (Some of these lectures were recorded in mimeographed notes prepared by L. S. Rodberg, which, however, ignore weak interactions.[18]) But by early 1957, when Wu's paper came out,[20] he had to accept the facts. As he stated in his following paper [82], 'It must be admitted that, despite its natural place in our scheme, this conclusion [parity non-conservation] required the stimulus of certain recent experiments, and was not drawn in the lectures upon which this article was based.'

This failed attempt to save parity had, not surprisingly, a negative impact on some of Schwinger's students. Apparently, it was first Schwinger's student Daniel Kleitman who showed that Schwinger's theory of $K$ mesons was inconsistent with experiment. Unfortunately, at that point another student, Norman Horing, was just at the point of finishing his thesis on this topic. Although he believed that Schwinger would have approved his thesis anyway, he did not want to use a failed theory, so he had to choose a new topic, on the dielectric properties of a solid state plasma. Even though this redirection cost Horing at least an extra year, he was not bitter about the experience.[22] Others were less sanguine.

## 'A theory of fundamental interactions'

When Schwinger selected this paper [82] for inclusion in *Selected papers of Julian Schwinger*[23] he gave the following reasons. It was 'a speculative paper that was remarkably on target: VA weak interaction theory, two neutrinos, charged intermediate vector meson, dynamical unification of weak and electromagnetic interactions, scale invariance, chiral transformations, mass generation through vacuum expectation value of scalar field. Concerning the idea of unifying weak and electromagnetic interactions, Rabi once reported to me: "They hate it." ' As he stated at the beginning of the article, it grew out of the previous $K$ meson paper, as further elaborated in lectures given at Harvard and MIT in the Fall Semester of 1956.* It will be relevant to the subsequent discussion to note that it was received by the *Annals of Physics* on 31 July 1957, after being withdrawn from *Physical Review* earlier. (Recall the footnote on p. 415.) Because it contains so many prescient ideas, we will analyze the content of this paper in considerable detail.

Schwinger started with some group theory, developed, of course, in a self-contained way. He decomposed the 'general multicomponent Hermitian field $\chi$' into a Fermi–Dirac and a Bose–Einstein part. These parts transformed under certain internal symmetries, which transformations are represented by

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* Notes of some of these lectures are in the Schwinger archive.[18]

antisymmetrical, imaginary matrices $T_a$, obeying a Lie algebra,

$$[T_a, T_b] = t_{abc} T_c, \tag{12.8}$$

where the matrices $t_a$, with components $(t_a)_{bc} = t_{bac}$, the structure constants of the Lie group, form a representation of the group of dimension equal to the number of transformation parameters. If the number of transformation parameters is three, the 'group structure is uniquely that of the three-dimensional Euclidean rotation group.' If the number of parameters is six, the group is that of four-dimensional Euclidean rotations, or, equivalently, the product of two three-dimensional rotation groups. He also considered the dimensionality $\nu$ of the representative matrices. 'For $\nu = 3$, with three antisymmetrical matrices, we obtain the $T = 1$ representation of the three-dimensional rotation group, appropriate to the rotations of a vector in that space. With $\nu = 4$, and six antisymmetrical matrices, we can describe the rotations of a vector in the four-dimensional space, which combine the $T = 0$ and $T = 1$ representations of the three-dimensional group. Alternatively, we can ... give two $T = \frac{1}{2}$ representations of the three-dimensional rotation group. Thus the same four-dimensional matrices can be viewed as referring to the three-dimensional representations $T = 0, 1$, or to a two-fold $T = \frac{1}{2}$ representation.' (The rotations referred to are internal ones, such as isospin rotations.) Because the three-dimensional symmetry is broken by electromagnetism, we need to reduce the three-dimensional symmetry to a two-dimensional one, which is accomplished by writing the generator of rotations in the 12 plane as

$$T_{12} = T_3 + \frac{1}{2} Y, \tag{12.9}$$

where we recall that $T_3$ is the third component of isospin, and $Y$ is the hypercharge (a term which, as we recall, Schwinger coined in [81]), which Schwinger defined as zero for $T = 0, 1$, and as having eigenvalues $\pm 1$ for $T = \frac{1}{2}$. Schwinger explicitly acknowledged this as the Gell-Mann–Nishijima formula[15] (12.6), which is 'the general connection between isotopic spin, electric charge, and hypercharge.' He noted that the existence of the baryon octet, $N = (p, n)$, $\Xi = (\Xi^0, \Xi^-)$ (actually, $\Xi^0$ had not yet been found), $\Sigma = (\Sigma^+, \Sigma^0, \Sigma^-)$, and $\Lambda^0$, had been established, but that the singlet member of the meson octet, completing $\pi^\pm$, $\pi^0$, $K^+$, $K^0$, and $\bar{K}^0$, $\bar{K}^-$ seemed to be missing. He proposed a somewhat clumsy way of excluding that state, which, of course, was unnecessary, because eventually it was found, and called the $\eta$ meson.[24] This failure of courage to trust the straightforward consequences of his theory was to be repeated, with more profound consequences, later in the paper.*

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* It is interesting to recall that a few years later, in 1964, Schwinger proposed a ninth pseudoscalar meson, what we now call the $\eta'$, and a ninth baryon, which does not exist.

So far, this was just kinematics. To describe dynamics, Schwinger introduced interaction terms into the Lagrangian $\mathcal{L}$. How to do this? Schwinger adopted the very modern view of scale invariance: 'In seeking possible forms for the interaction term in $\mathcal{L}$ we shall be guided by the heuristic principle that the coupling between fields is described by simple algebraic functions of the field operators in which only dimensionless constants appear. This principle expresses the attitude that present theories, which are based on the infinite divisibility of the space–time manifold, contain no intrinsic standard of length. The mass constants of the individual fields are regarded as phenomenological manifestations of the unknown physical agency that produces the failure in the conventional space–time description and establishes the absolute scale of length and of mass. On this view, the coupling terms employed within the present formalism should not embody a unit of length that finds its dynamical origin outside the domain of physical experience to which the theory of fields is applicable. For interacting spin-0 and spin-$\frac{1}{2}$ fields only two types of coupling terms are admitted by this principle, $\phi\psi\psi$ and $\phi\phi\phi\phi$.'

On this basis, Schwinger first introduced a coupling of the isovector pion field with nucleonic current,

$$g_\pi \phi_{(1)} \frac{1}{2} \psi_{(1/2)} \beta\gamma_5 \nu\tau \psi_{(1/2)}, \tag{12.10}$$

where the subscripts indicate the isospin, $\beta = \gamma^0$, $\frac{1}{2}\tau$ is the isospin vector for isospin $\frac{1}{2}$, and the nucleonic charge $N$ is represented by the matrix $\nu$,

$$\nu = \begin{pmatrix} 0 & -\mathrm{i} \\ \mathrm{i} & 0 \end{pmatrix}. \tag{12.11}$$

Schwinger then generalized this coupling 'to the concept of a universal $\pi$-heavy fermion interaction in which both integral and half-integral isotopic spin F.D. fields take part .... This universality also implies that nucleonic charge is a general property of the heavy F.D. particle field, without regard to isotopic spin. Thus the $\pi$-field emerges as the dynamical agency that defines nucleonic charge, the absolute conservation of which is the quantitative expression of the stability of nuclear matter.'

Schwinger discussed the transformations of the fields under the various continuous internal symmetries, such as isotopic rotations. In particular, associated with $N$ rotations is the conserved nucleonic charge current:

$$j_N^\mu = \frac{1}{2} \psi\beta\gamma^\mu \nu\psi, \tag{12.12}$$

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[119] These proposals, which we recounted on p. 376, may have been a reaction to his earlier failure of courage here.

whose integral over an arbitrary space-like surface is the total nucleonic charge. Schwinger also discussed discrete transformations, such as nucleonic charge reflection, under which the nucleonic charge operator, and the pion field, change sign.

It was natural to complete the four-dimensional picture by adding an iso-scalar partner, $\sigma$, to the isovector pion. The $\sigma$ field must be a scalar, rather than a pseudoscalar like the pion; Schwinger took it to couple with the fermions in

$$\mathcal{L}_\sigma = g_\pi \phi_{(0)} \frac{1}{2} [\psi_{(1/2)} \beta \psi_{(1/2)} + \psi_{(0)} \beta \psi_{(0)} + \psi_{(1)} \beta \psi_{(1)}]. \tag{12.13}$$

(This marked the beginning of the famous sigma model, important in models of chiral symmetry breaking, to which we will return in Chapter 13. The sigma model was popularized in the work of Polkinghorne[25] and Gell-Mann and Lévy.[26]) He then went on to discuss the question of masses: he saw no way to generate fermion masses, while boson masses could arise through the vacuum expectation values of scalar fields. In modern parlance, this is because the Dirac mass term violates chiral symmetry, which is not true for the coupling $\phi\psi\beta\psi$ under

$$\psi \to \gamma_5 \psi, \quad \phi \to -\phi, \tag{12.14}$$

whereas a $\phi\phi\phi\phi$ term can be written down which 'will produce effective mass terms for each field through the action of the vacuum fluctuations of the other fields.' As Schwinger noted many years later, 'My idea here was, from the very beginning, to use the scalar field as a way of generating masses.'[19] This is the essence of the modern Higgs mechanism, which we will discuss later.

Schwinger then introduced as isotopic doublet for a kaon field, $\phi_{(1/2)}$, which through couplings of the form $\phi_{(1/2)}\psi_{(1/2)}\psi_{(0,1)}$ provides 'the physical agency that differentiates $N$ from $\Xi$,' and is also 'the agency that produces different masses for $\Lambda$ and $\Sigma$.'

The complete strong-interaction Lagrange function, which includes the 'kinetic energy' terms for the spin-0 and spin-$\frac{1}{2}$ fields, the interaction of the pion and the kaon with the heavy fermions, and the $\phi^4$ term that gives masses to the scalars, possesses invariance under $N$, $T$, and $Y$ rotations. It also possesses discrete symmetries, such as $R_T$, the reflection operator in isospin space, the product of nucleonic charge reflection and the reflections of hypercharge, neither one of which, separately, is an invariance operation.

Schwinger then enlarged the picture to include electromagnetism. The coupling of photons to the strongly interacting particles is of course through the electric current,

$$\mathcal{L}_A = e j_Q^\mu . A_\mu, \tag{12.15}$$

where the dot denotes symmetric multiplication, and according to (12.6)

$$j_Q^\mu = j_{T_3}^\mu + \frac{1}{2} j_Y^\mu. \tag{12.16}$$

'This interaction term makes explicit the dynamical role of the electromagnetic field in reducing the three-dimensional $T$ symmetries to the two-dimensional one described by $T_3$ rotations. At the same time, $R_T$ is no longer a suitable reflection operation .... But if one superimposes the rotation $e^{\pi i T_1}$, the entire operator $j_Q^\mu$ reverses sign and an inversion operation is provided by $R_Q$,'

$$R_Q = R_T e^{\pi i T_1}. \tag{12.17}$$

This is the operation usually referred to as charge conjugation.

Schwinger next turned to the weak interactions, and to the leptons: 'The theory thus far devised refers to heavy fermions and heavy bosons,* together with the photon, and gives an account of their strong and electromagnetic interactions. Omitted are the light fermions (leptons) and the various physical processes that exhibit a very long time scale. The interactions responsible for these processes are certainly of lower symmetry than those already discussed, the total effect of the latter being described by various two-dimensional rotational symmetries, or charges, and a single charge reflection operation. Since the leptons carry electric charge, they at least realize a two-dimensional internal symmetry space, which invites an attempt to correlate their properties with the aid of an internal space of higher dimensionality, but one which is presumably of lesser dimensionality than that employed for the heavy particles.' So Schwinger was naturally led to assign the $T = 1$ representation of the three-dimensional rotation group to the leptons, with the electric charge being

$$Q = T_3. \tag{12.18}$$

And by analogy with nucleonic charge he assigned a leptonic charge $L$ to the leptons, represented by the matrix

$$\lambda = \begin{pmatrix} 0 & -i \\ i & 0 \end{pmatrix}. \tag{12.19}$$

* The reader will note that Schwinger uses the term *lepton* but not the corresponding term for the strongly interacting 'heavy fermions,' the *baryons*. He also does not use the collective term for strongly interacting particles, *hadrons*. Concerning the latter, he once remarked: 'The term hadron has been introduced in opposition to lepton, which designates particles, other than the photon and graviton, that do not have strong interactions. Lepton was well chosen since the Greek combining form *lepto-* includes "small, weak" among its meanings. But, unfortunately, the meanings of *hadro-* are limited to "ripe, thick", which this is, a bit.' [150]

All the leptons known in 1957 could be characterized by the eigenvalues of these two operators:

$$L = +1: \quad \mu^+, \nu^0, \mathrm{e}^-, \qquad L = -1: \quad \mathrm{e}^+, \bar{\nu}^0, \mu^-. \tag{12.20}$$

Schwinger had some difficulty in understanding the 'very striking mass asymmetry' between the muon and the electron. (Of course, we still have this difficulty to this day.) He proposed an interaction of the muon with the scalar $\sigma$ field. Through a non-zero vacuum expectation value of the latter, he imagined a suitable muon mass might arise. Of course, he recognized that this would lead to measurable non-electromagnetic interactions with the muon, which are now ruled out, in view of the largeness of the coupling, and the smallness of the $\sigma$ mass.* At last, Schwinger was ready to discuss the carriers of the weak interaction. 'The symmetry that exists between the heavy bosons and fermions in the isotopic space properties prompts us to ask: Is there also a family of *bosons* that realizes the $T = 1$ representation of the three-dimensional rotation group? The exceptional position of the electromagnetic field in our scheme, and the formal suggestion that this field is the third component of a three-dimensional isotopic vector, encourage an affirmative answer. We are thus led to the concept of a spin one family of bosons, comprising the massless, neutral, photon and a pair of electrically charged particles that presumably carry mass, in analogy with the leptons.' Thus Schwinger introduced a triplet of fields $Z^\mu$, the third component of which he identified with the photon.† He further postulated an interaction with the $\sigma$ field,

$$\mathcal{L}_{Z\phi} = -g_{Z\phi}^2 \frac{1}{2}\phi_{(0)}^2 \frac{1}{2} Z^\mu t_3^2 Z_\mu, \tag{12.21}$$

which would produce masses for the charged $Z$s. Thus a unification of electromagnetism and weak interactions is proposed, and the hitherto mysterious reason for the feebleness of the weak interactions is explained. 'Now we must face the problem of discovering the specific Yukawa interactions of the massive, charged $Z$ particles. From its role as a partner of the electromagnetic field, we might expect that the charged $Z$ field interacts universally with electric

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* A similar coupling occurs in the Higgs interaction in the standard model, but it is very small. For a fermion $f$, it is $-e(m_f/M_W)H^0\bar{\psi}_f\psi_f$, where the experimental limit on the mass of the Higgs boson $H$ is $M_H > 80$ GeV, the precise lower limit depending on model assumptions.[27]

† Schwinger did not discuss the interactions of these fields with each other. A non-Abelian structure seems implicit, yet, as the following equation demonstrates, he did not insist an a local gauge symmetry. It was this step which was the chief innovation of Glashow, whose work we shall discuss in the following section.

charge, or rather, changes of charge, without particular regard to other internal attributes. If this be so, the coupling with the $Z$ field (henceforth understood to be the charged $Z$ field) will produce further reductions of internal symmetry, which raises the hope that this general mechanism may be the underlying cause of the whole group of physical processes that are characterized by a long time scale. Indeed, that time scale becomes more comprehensible, without invoking inordinately weak interaction, if every observable process requires the virtual creation of a heavy particle. Our general viewpoint regarding the systematic reduction of internal symmetry also impels us to seek some internal symmetry aspect of the $Z$ field that is destroyed by the coupling with various combinations of electrically charged and neutral fields. There is no question, presumably, of a breakdown of invariance under the two-dimensional rotations that define electric charge, which leaves no choice other than a failure of the charge reflection symmetry property for the $Z$ field interactions. (It must be admitted that, despite its natural place in our scheme, this conclusion required the stimulus of certain recent experiments, and was not drawn in the lectures upon which this article is based.)' Schwinger further went on to note that 'a failure of invariance under charge reflection must be accompanied by a failure of invariance under space reflection.' However, the product of the these two reflections would still be an invariance.*

Schwinger was thus led to the following vector interaction between the $Z$ field and the leptons, represented by a field $\psi_l$:

$$\mathcal{L}_{Zl} = g_Z Z^\mu \frac{1}{2} \psi_l \beta \gamma_\mu (t - i\gamma_5\{t_3, t\}) \psi_l. \tag{12.22}$$

This is the famous $V-A$ structure of the weak interactions, anticipated somewhat before the contributions† of Feynman and Gell-Mann[29] and of Sudarshan and Marshak[30]. This Lagrangian possesses another invariance, which corresponds to the conservation of what Schwinger called 'neutrinic charge' $n$, which for the charged leptons, $\mu$ and e, is the negative of the electric charge, but which for the neutrino is the eigenvalue of $i\gamma_5$, the chirality. 'Thus a neutrino with

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* As we have noted, these two symmetries are usually denoted by $C$, for charge conjugation, and $P$, for parity reflection. What Schwinger said here is that, although $C$ and $P$ are separately violated, $CP$ is not. But in 1964, Cronin and Fitch[28] found that $CP$ invariance is slightly violated in the neutral kaon system. The origin of $CP$ violation is still not understood.

† Recall that Schwinger's paper was received by *Annals of Physics* on 31 July 1957, and considerably earlier by *Physical Review* before it was withdrawn. The Sudarshan–Marshak paper was completed early in July 1957, and circulated as a preprint on 16 September 1957, the same day the Feynman–Gell-Mann paper was received by the *Physical Review.*

$n = +1$ or $-1$ can be designated as a right or left polarized neutrino. In a process involving the creation of a pair of leptons through the intervention of the $Z$ field, the conservation of neutrinic charge states that a lepton of positive electric charge appears with a right polarized neutrino, and a negatively charged lepton with a left polarized neutrino.' Thus the concept of neutrinic charge conservation played the role that 'lepton family number' conservation plays in the modern theory with three families of charged leptons and neutrinos.* Schwinger further observed that at high energies, where the electron mass may be neglected, the electron is also produced with a definite polarization, which led to the electron asymmetry observed in the successive decays

$$\pi \to \mu + \nu, \quad \mu \to \mathrm{e} + \nu + \nu, \tag{12.23}$$

that is, that 'the electron must be emitted predominantly in the direction of the $\mu$ spin, or oppositely to the initial direction of the $\mu$ meson,' in accord with then recent experiments.[31]

It was natural that Schwinger should extend this $V - A$ coupling to the heavy fermions. Indeed, Schwinger wrote down a simple compact formula for the coupling of $Z$ to baryons, similar to (12.22). 'Through the intervention of the $Z$ field, physical processes involving heavy particles take place that conserve nucleonic charge and electrical charge only of the list of internal attributes, to which *parity* [i.e. $CP$] should be added as a joint internal and space–time property. These processes include known particle decays: $\Sigma, \Lambda \to N + \pi$; $\Xi \to \Lambda + \pi$; $K \to 2\pi, 3\pi$, and the theory must meet various quantitative tests, including its effectiveness in suppressing the decay $\Xi \to N + \pi$.' The question was how the understand the suppression of $\Delta Y = 2$ decays.†

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* Thus, Schwinger predicted the existence of two neutrinos, what are now called $\nu_e$ and $\nu_\mu$. Five years later he tried to readvertize this result with a paper called 'Two neutrinos,'[18] where he also corrected the chirality of the neutrinos, the sign of $i\gamma_5$ in (12.22), which was unknown in 1957. 'When the two neutrinos were discovered experimentally for entirely different reasons, I wrote a little note to the *Physical Review Letters*, which Goudsmit was then in charge of, pointing out that it had been anticipated, and Goudsmit sent it back saying not even for you would we publish such—yet I do believe it was necessary to draw attention—I mean to the extent that I was right about two neutrinos, there was a suggestion that maybe the whole line of thought was not to be ignored.'[19] Pontecorvo had also predicted two neutrinos, but 'he was just anticipating the possibility for some reason. This is building it into a theory. It's rather different.'[19]

† This is now understood as occurring only in second order, and hence very small. In the quark language, each of the two $s$ quarks in the $\Xi$ must be changed to a $d$ quark. For further discussion of $\Delta S = 2$ processes in the standard model, see Ref. 32.

Of course, the $Z$ particle couples the baryons to the leptons, and thereby implies the leptonic decays of the mesons, such as

$$\pi \to \mu + \nu, \quad K \to \mu + \nu, \tag{12.24}$$

$$\pi \to e + \nu, \quad K \to e + \nu. \tag{12.25}$$

The decays on the second line had not been observed to that date. (They are now known to have small branching fractions, of order $10^{-4}$ and $10^{-5}$, respectively.) Schwinger could now understand the suppression of the decays into electrons from angular momentum conservation. 'Now it is encouraging that these electron processes would not occur if the electron mass were zero, for then electron and neutrino are oppositely polarized, which produces a net angular momentum about the axis of disintegration and contradicts the zero spin of $\pi$ and $K$. It will be noted that the vector nature of the $Z$ coupling is decisive in this argument. It can be concluded that the theory discriminates against the electron decay of the spinless bosons, but the precise ratio of decay probabilities may depend upon the specific dynamical origins of the electron and muon masses.' In fact, the $V - A$ theory agrees with the observed branching ratio to better than 1%.*

Schwinger's prediction of the $V - A$ structure of the weak interactions was hardly noticed at the time, nor recognized later. Schwinger commented on this years later: 'There was no doubt in my mind that the *VA* interaction was a fundamental thing, but then because I did this too early, you come into this terrible conflict with experiment.'[19] As to Feynman's statement that the $V - A$ theory gave him much pleasure because this was the only time *he* had discovered a law of nature, Schwinger commented 'I remember him saying that. It has given me no pleasure because my prior discovery has never been recognized. Isn't that funny. Well, too many people had too much at stake to say, "Oh yeah, but . . . ." I think I'm angry still. Do I have a right to be? . . . I think Feynman gets credit, although since it did happen later, experiments were beginning to get shaky, for insisting that maybe the experiments are wrong. But at this stage there was no possibility of questioning the experiments. . . . I was not so bold as to suggest that maybe the experiments were wrong. That was my fault. But I had no great tradition in questioning experiments. I had no idea the experiments were so tricky and unreliable. Who would? So my fault was not simply to believe enough in what I had done and to try desperately to accommodate the experiments. Yet every time I tried to accommodate, it didn't work.'[19]

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* In fact Ruderman and Finkelstein[33] calculated the ratio of decay rates for the $\mu\nu$ and $e\nu$ decay modes in 1949, which depended on the then unknown pseudoscalar and axial-vector couplings. The experimental value[27] $1.23 \times 10^{-4}$ is obtained with the pseudoscalar coupling set equal to zero, when the radiative corrections, which amount to about 4%, are taken into account.[34]

All of the foregoing is an amazing foretaste of what was to become the 'Standard Model' 15 years later. But now Schwinger had to confront the data on $\beta$-decay, and chose to follow the accepted experimental results, rather than confront them, as Feynman and Gell-Mann,[29] and Sudarshan and Marshak[30] would shortly do. To quote again Schwinger's words, from [82], 'We then come to the comparatively well-known $\beta$-decay processes, where there is evidence that the lepton field appears in a tensor form combined with either a vector or a scalar coupling, the latter being currently favored by angular correlation measurements. The empirical tensor interaction fits naturally into the $Z$-particle picture.' He then went on to write down a tensor interaction for the leptons. But he had difficulty in extending this interaction to the baryons, so he suggested that the weak interactions are perhaps not universal after all. Schwinger's strong phenomenological bent had led him up a blind alley, and he ended the paper on a tentative note. 'From the general suggestions of a family of bosons that is the isotopic analog of the leptons, and the identification of its neutral member as the photon, we have been led to a dynamics of a charged, unit spin $Z$-particle field that is interpreted as the invisible instrument of the whole class of weak interactions. The direct identification of this hypothetical particle will not be easy. Its linear couplings are neither so strong that it would be produced copiously, nor are they so weak that an appreciable lifetime would be anticipated. And as to the detailed implications of this model for the effective weak interactions that it seeks to comprehend, although the theory is definite enough about the fundamental predominance of vector and tensor coupling, the rest of the structure is hardly unique, and the profound effects of the various strong interactions obscure the actual predictions of the formalism. The definitive results of the group of experiments that exploit the newly discovered lepton polarization properties of the weak interactions will be particularly relevant in judging this hypothesis.'

Some time later, a two-page fragment of an *Added note* was written.[18] It included the following statement. 'At the time of this conclusion of the $Z$ particle hypothesis (Nov. 1956) and for some time after, it was generally believed that the beta-decay coupling was scalar and tensor ($ST$), and thus in I [82] an alternative was made to provide the tensor interaction. ... But the experimental situation has changed drastically, and [now] favors the *VA* interaction.' He went on to correct the electron helicity:

$$\begin{aligned} Z^+ &\to \mu^+\tilde{\nu}_L, \quad e^+\nu_L, \\ Z^- &\to \mu^-\nu_R, \quad e^-\tilde{\nu}_R. \end{aligned} \tag{12.26}$$

This note was never published.

Although this paper is remembered chiefly for its seminal insights into the weak interactions, it was far bolder than that. It is dense with ideas, far more ambitious than the electroweak synthesis papers that followed. As we have seen, it attempted to encompass all the interactions, strong, electromagnetic, and weak, into a coherent framework. In fact, in the penultimate paragraph, Schwinger noted that 'in the hierarchy of fields there is a natural place for the gravitational field.' This entire paper is a magnificent example of Einstein's motto, which opens the paper, 'The axiomatic basis of theoretical physics cannot be abstracted from experience but must be freely invented.' Indeed, where Schwinger lost his way is when he followed experience too closely.

Shortly after completing this paper, in the summer of 1957, Schwinger gave a lecture on the subject to the Canadian Association of Physicists in Edmonton; the lecture was identical to the published paper, but for the introduction.[18]

After the meeting Julian and Clarice went to an absolute jewel of a place called Lake Moraine, in Alberta, which consisted of a lodge with little cottages. It was very quiet. Clarice remembered a beautiful morning when she skipped from the cabin down to the lake, thinking she was in the wilderness, when all of a sudden she heard 'Mrs Schwinger.' It was Marshall Baker, one of Julian's students.[35]

In fact Marshall Baker had just finished writing his thesis, and had come to the meeting in the hope that Schwinger would look it over. He was told that Schwinger had left early for this remote lake. Undaunted, he had tracked them down. As Baker tells the story, it was more exciting, for when he saw the Schwingers, they were in the process of shooing a bear away from their cabin. In any case, it proved to be a memorable reunion, and any thought of having Schwinger look at his thesis vanished from Baker's mind. Of course, his thesis was approved when Schwinger returned to Harvard.[17]

## Glashow's thesis ($V - A$ and all that)

Sheldon Glashow gave a remarkable, brief account of his interactions with Schwinger while he was a graduate student at Harvard during the period 1954–58 in his talk at the Schwinger Memorial Session at the Washington APS-AAPT meeting in Washington, 20 April 1995.[36] He began by recounting his starting to work for Schwinger: 'Schwinger was the central feature of my Harvard years. In every one of my eight semesters, I sat in the front row and listened raptly to Julian's virtuoso performances. And I learned the substance of physics. After a year or two of study, I felt prepared to ask to become his research student. Along with a dozen of my similarly enchanted peers, I dared venture into Schwinger's office to put the question. Somewhat put off by an invasion of the masses, Schwinger assigned us all a test problem to work out at home. (I think it was to express the photon propagator in the Coulomb gauge.) No doubt, he hoped that we might return in a thinner and more manageable stream. A subset of us attacked the problem immediately and collaboratively and returned to the master's office at the very next opportunity: "Can we become your students?" we said. And so we did.'

Glashow was fortunate to be assigned just the right problem.* 'On my turn, Julian turned to the then-mysterious weak force. Julian was convinced of the existence of an "intermediate vector boson" and of a fundamental connection between weak interactions and electromagnetism. How else to explain their common vectorial nature and their universality? My task was not precisely delineated. It was to seek and perhaps find such a relation, and to explore its observable consequences. I remember little more of this encounter with Julian, except that it set me on a long and treacherous voyage.

'Why treacherous? In those days of yore, our understanding of the microworld was expanding at breakneck speed. A once theoretically "dictated" and experimentally "established" parity conserving $S$, $T$, $P$ model of the weak force was bit by bit giving way to the correct parity-violating $V - A$ picture. Schwinger's first stab at electroweak synthesis took place during a short-lived $V$, $T$ interregnum. Nonetheless, he convinced himself (and me!) that a triplet of vector bosons, linked to each other as a Yang–Mills gauge theory, could possibly offer a plausible, elegant, and unified explanation of all electromagnetic and weak phenomena. Only a few vexing details remained, such as the large mass of the charged intermediary (which Steve [Weinberg] would later provide) and its failure to conserve parity and strangeness.'

Glashow went on to recount how the seeds of universality were planted in in his mind by Schwinger 'during our many conversations in his office or over lunches at Chez Dreyfus (where he inevitably ordered steak).' An example was the two-neutrino hypothesis. 'Long before Lederman, Schwartz, and Steinberger discovered[39] the muon neutrino, students at Harvard knew there had to be two. Schwinger's logic was impeccable. If we choose to believe in a conserved lepton number, it would be foolish (of us or Nature) to assign it in such a way that negative electrons and muons were not distinguished. Thus if electrons are leptons, so are *positive* muons. And as day follows night, the electron neutrino cannot be the same as the muon neutrino. Julian concluded that the electron, the neutrino, and the positive muon form a weak isospin triplet. Once again, the basic idea was right, but the details were not. There is

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* The other students were also assigned superb problems as well. For example, Schwinger suggested to Charles Sommerfield that he re-examine the fourth-order correction to the magnetic moment of the electron, which had first been calculated by Karplus and Kroll[37]. Schwinger suspected their result, which he had used in his own work, might be erroneous. Indeed it was, and Sommerfield got it right.[38] 'Interestingly enough, although Feynman–Dyson methods were applied early [by Karplus and Kroll], the first correct higher-order calculation was done by Sommerfield using [my] methods.'[19]

such a thing as weak isospin, but the leptons form doublets (three of them!), not triplets.'

Eventually, Glashow finished his dissertation. 'Late in the Spring of 1958, I had assembled what I hoped might pass for a thesis. By that time, Schwinger had decamped for a summer in Madison, Wisconsin, where my thesis defense was to be held. The examining committee consisted of Bob Sachs, Paul Martin, Frank Yang, and Julian. During my presentation, Yang asked me what it could mean to say that electron and muon neutrinos were not the same. At that point, Julian took over the discussion and my safe passage was ensured.'

But life is never secure: 'A few days later, after a somewhat raucous celebration, while Julian and I were sitting in his (pre-Lancia, pre-Iso) baby-blue Cadillac on a quiet street in suburban Madison, we were water-bombed by some irate citizens.'

So, finally, Glashow went his own way, and soon the seeds planted by Schwinger came to blossom: 'Having earned my degree, I planned to spend a year in the Soviet Union. I set out for the Bohr Institute in Copenhagen, where I would await the promised visa which would never come. So I spent my time recommuting my small matrices, just as Julian had taught me. The trouble was that the algebra of charges couldn't deal with parity-conserving electromagnetism and parity-violating weak interactions—unless (and it took me over a year to see this, since I no longer had direct access to Julian) the group was made just a wee bit bigger. It was only a small step from Julian's lepton triplet to two lepton doublets, from his SU(2) model to Nature's SU(2)×U(1) theory, and hence to the $Z^0$ boson. The imprinting had been done at Harvard.'[36]

Glashow finished his PhD under Schwinger's direction in 1958. He then spent two years at the Institute for Theoretical Physics (now called the Niels Bohr Institute) at the University of Copenhagen. In September 1960 he submitted his famous paper proposing the essentials of the standard electroweak model.[40] Very explicitly he built upon the foundation laid by Schwinger three years earlier. As Glashow stated in the first paragraph of his paper, 'Schwinger first suggested the existence of an "isotopic" triplet of vector fields whose universal couplings would generate both the weak interactions and electromagnetism—the two oppositely charged fields mediate weak interactions and the neutral field is light. A certain ambiguity beclouds the self-interactions among the three vector bosons; these can equivalently be interpreted as weak or electromagnetic couplings. The more recent accumulation of experimental evidence supporting the $\Delta I = \frac{1}{2}$ rule characterizing the non-leptonic decay modes of strange particles indicates a need for at least one additional neutral intermediary.' (Note that Glashow was now using $I$ rather than $T$ to denote isospin.) Thus Glashow introduced a triplet of vector bosons, $Z_1^\mu$, $Z_2^\mu$, $Z_3^\mu$, transforming as a vector under (internal) three-dimensional rotations (equivalently, under $SU(2)$),

$$\mathrm{Z} \to (1 + \mathrm{ia} \cdot \mathrm{t})\mathrm{Z}, \tag{12.27}$$

'where the t are the conventional anti-symmetric imaginary 3 × 3 matrices.' These gauge bosons interacted with themselves, and with each other, through the interaction Lagrangian

$$g\mathrm{Z}_\mu \cdot \mathrm{J}^\mu, \quad \mathrm{J}_\mu = \mathrm{Z}_{\mu\nu} \times \mathrm{Z}^\nu + \psi\beta\gamma_\mu \mathrm{O}\psi, \tag{12.28}$$

where $\mathrm{Z}_{\mu\nu}$ was the field strength for the gauge field,* and O were imaginary antisymmetrical matrices satisfying the angular momentum algebra,

$$\mathrm{O} \times \mathrm{O} = \mathrm{iO}. \tag{12.29}$$

The structure of these leptonic currents was not unique; one must be guided by experiment. Indeed, Schwinger had foreseen the correct structure for the charged currents, because 'negatons are produced ... only in association with left-handed neutrinos whereas positrons are accompanied by right-handed ones.' Thus

$$O_1 = \frac{1}{\sqrt{8}}(t_1 + i\gamma_5\{t_1, t_3\}), \quad O_2 = \frac{1}{\sqrt{8}}(t_2 + i\gamma_5\{t_2, t_3\}), \tag{12.30}$$

which agrees with Eqn (12.22), apart from the change in the sign of $i\gamma_5$. The neutral current, as a consequence, was not the electromagnetic one,

$$O_3 = -\mathrm{i}[O_1, O_2] = \frac{1}{4}\left(t_3 + i\gamma_5\left(3t_3^2 - 2\right)\right). \tag{12.31}$$

'Thus the theory containing only the necessary weak interactions of two oppositely charged decay intermediaries together with the electromagnetic interactions of both the leptons and the bosons is not partial-symmetric.' By the latter term, Glashow meant what we would now refer to as non-Abelian gauge invariance, but only a partial one because the necessary mass terms break the symmetry. (The notion of partial symmetry would be extensively exploited by Schwinger later in the decade, in connection with strong interactions; see pp. 473–477. However, see also Schwinger's comment below about destroying the non-Abelian symmetry in this way.)

* Glashow had made explicit what was implicit in Schwinger's approach: the self-coupling of the $Z$ fields. Yet even here, local gauge transformations were not discussed, and the construction of the field strength was not given. It is noteworthy that Schwinger himself did not discuss non-Abelian gauge fields until 1961 [105].

To synthesize these interactions with electromagnetism Glashow then introduced an additional neutral vector boson $Z_S$. The corresponding lepton current $J_S^\mu = \psi\beta\gamma_\mu S\psi$ had a generator which commutes with $\mathbf{O}$,

$$[\mathbf{O}, S] = 0, \tag{12.32}$$

and was given through the relation

$$Q = t_3 = O_3 + S, \tag{12.33}$$

which was analogous to the Gell-Mann–Nishijima relation (12.6). [Indeed, following Weinberg, one usually calls $\mathbf{O}$ the weak isospin, and $2S$ the weak hypercharge.] The interaction Lagrangian was now taken to be

$$e \sec\theta \mathbf{Z}_\mu \cdot \mathbf{J}^\mu + e \csc\theta Z_\mu^S J_S^\mu, \tag{12.34}$$

where 'the parameter $\theta$ appears in order to permit an arbitrary choice of the strength of the triplet and singlet interactions.'

Neither $Z_3$ nor $Z_S$ may be identified with the photon. Yet the mass matrix may not be diagonal. If one of its eigenvalues is zero, a linear combination of $Z_3$ and $Z_S$ may be found which may be identified as the photon, while the orthogonal combination, with a necessarily large mass, is the neutral weak boson $B$, 'the price we must pay for partial symmetry.' However, at this point, this hypothesis is *ad hoc*, and 'the masses of the charged intermediaries $M_Z$ and of the neutral $M_B$ are as yet arbitrary.'

Glashow concluded by noting that 'it seems remarkable that both the requirement of partial-symmetry and quite independent experimental considerations indicate the existence of neutral weakly interacting currents.' He then went on to worry whether a single neutral $B$ is sufficient to produce the $\Delta I = \frac{1}{2}$ rule. He remarked that two neutral gauge bosons seemed to be necessary if the selection rules of strangeness as well as of isotopic spin were to be satisfied, which would violate $CP$ invariance. The concluding paragraph summarized what Glashow achieved, as well as the major missing link: 'We have argued that any underlying symmetries relating weak interactions and electromagnetism are obscured by the masses of elementary particles. Without a theory of the origins of these masses, any study based upon the analogy between decay-intermediaries and photons may make use only of partial symmetries. The simplest partially-symmetric system exhibiting all known interactions of the leptons, the weak and the electromagnetic, has been determined. Although we cannot say *why* the weak interactions violate parity conservation while electromagnetism does not, we have shown *how* this property can be embedded in a unified model of both interactions. Unfortunately our considerations seem without decisive experimental consequence. For this approach to be more than academic, a partially-symmetric system correctly describing all decay modes of all elementary particles should be sought.' To complete the picture, the contributions of Weinberg[41] were essential.

## Non-Abelian gauge theory

As we have seen, the interactions of the weak gauge bosons, the $Z$s, among themselves, were not mentioned at all by Schwinger, and only in passing by Glashow. It is perfectly clear why: since those intermediaries were then beyond experimental reach, only the interactions with the fermions were accessible. Yet, if the partial-symmetry ideas of Glashow were truly valid, it was obvious what those self-interactions had to be. In 1954 C. N. Yang and Robert Mills[42] had already generalized the notion of the Abelian gauge theory of electrodynamics to a non-Abelian one, where, instead of a single gauge boson, the photon, there would be $n$ gauge bosons, where $n$ is the number of generators $t^a$ of the group. But it would seem that those bosons, like the photon, must be massless—hence Glashow's partial symmetry, where large masses for the $Z$s are inserted by hand.

Schwinger had the idea that such masses might emerge dynamically. We recall from Chapter 11 that in 1961 Schwinger had shown that in electrodynamics, at least in two space–time dimensions, gauge invariance could be compatible with the vector particle acquiring a mass [104]. There, that demonstration was of little but academic interest, because there was and is no indication that the photon has a mass (experimentally, the mass is less than $2 \times 10^{-16}$ eV[27]). But it seemed plausible that a similar mechanism might be operative in the Yang–Mills domain of non-Abelian theories, which would be directly applicable to the intermediaries of the weak interactions.

It was for this reason that, at Stanford in Summer 1961, immediately following 'Gauge invariance and mass' [104], Schwinger wrote 'Non-Abelian gauge fields: commutation relations' [105]. The motivation for this work appeared only in the fifth paragraph: 'The concept of an internal symmetry group has long been considered a possible basis for describing the non-space–time properties of physical particles. To relate such a group to gauge transformations of vector fields is an attractive idea, but one which seems to run into difficulty immediately if it is accepted that a gauge field implies a corresponding massless particle. Only the photon is known as an example of this class of physical particle. It is hard to agree that the objection is overcome by destroying completely* the gauge invariance which is the entire motivation of the gauge fields. But there may be an escape from this dilemma. The author has remarked that

* The reference is to Sakurai,[43] in connection with vector dominance models. 'You might say he had a direct physical instinct that somehow the mass problem would take care of itself someday, but that the idea of these particles was important enough to develop. I

gauge-invariant systems of the electromagnetic, or, more generally expressed, Abelian type need not have an accompanying massless particle if the coupling is sufficiently strong [104]. The question is whether a similar possibility exists for systems with non-Abelian gauge groups. To discuss this problem requires at least a full knowledge of the operator properties of the gauge field, treated as a quantum-mechanical system without reference to weak coupling approximations. These commutation relations are not known. And it is not a trivial query whether a consistent quantum field theory is possible at all for a system that admits a non-Abelian group. But the latter can hardly be answered until a set of commutation relations has been displayed, for, without these, the nature of the operator description, with its necessary attribute of completeness, remains unknown. It is the purpose of this paper to produce such commutation relations, but we shall leave untouched the more difficult question of consistency.' [105]

This paper, and the following two in the sequence, 'Non-Abelian gauge fields. Relativistic invariance' [107] and 'Non-Abelian gauge fields. Lorentz gauge formulation' [110] are thus important technical papers aimed at establishing the quantum-mechanical and relativistic consistency of non-Abelian gauge theories. They are still fundamental for those who are concerned about the canonical formulation of gauge theory.* However, Schwinger's initial question still remains unanswered: is it possible to break the gauge invariance dynamically so that the gauge bosons acquire masses?† The route followed by Weinberg[41] was much simpler: he was able to generate the required masses spontaneously through vacuum expectation values of scalar fields. Yet Schwinger's concept remains a goal for many theoretical physicists to this day; the program of technicolor is a (failed) example of this idea.[46]

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think that was correct. But where did their masses come from? And why did giving them their mass not destroy all the good things you were trying to infer?'[19]

* It is interesting that Schwinger asserted that 'the essential contribution' of paper [107] was the determination of an extra term in the Hamiltonian density describing a non-Abelian field beyond the usual $\frac{1}{2}(E^2+B^2)$. 'The people who by this time were legion who used functional integral methods asserted that this term did not exist. Benjamin Lee and Ernest Abers wrote a review paper on non-Abelian gauge theories[44] and they decided that this additional term that I found and was so important was wrong. I did not get justified until many years later. People when they went through it a little more carefully found I was right.[45] I tend not to make algebraic or conceptual errors.'[19] Schwinger concluded from this that the path integral and canonical formulations of quantum field theory are not equivalent.[18]

† The question of masses in non-Abelian gauge theories was raised again by Schwinger in 'Non-Abelian vector gauge theories and the electromagnetic field' [117].

It is illuminating to recognize that, as noted in the previous chapter, Schwinger perceived the prototype of 'non-Abelian gauge theories' to be gravitation. 'Think of electrodynamics in which the photon interacts with charged particles but does not carry charge itself. So the photon is . . . a mediator only of interactions among charged particles. Now suppose we consider a "photon" that does carry charge. In other words, "In this familiar situation [electrodynamics] the gauge field does not carry the internal property to which it is coupled. A different example is furnished by the gravitational field, for this couples with energy and momentum, to which all physical systems must contribute" [105] including the gravitational field itself.'[19] Although the concept of non-Abelian fields 'entered the consciousness of most physicists with Yang and Mills in 1954, from my point of view it began, as I have made very clear when talking about the gravitational field, when Herman Weyl gave his *vierbein* formulation of gravitational theory in which exactly this concept is used explicitly and this group-theoretical context holds that the graviton which carries the gravitational force between massive particles also has mass and therefore interacts with itself. That was really the starting point.'[19]

Apparently, Schwinger started one final paper in the 'Non-Abelian gauge field' series, subtitled 'External sources,' but only a two-page fragment exists in the archives.[18]

## Glashow, Weinberg, Salam, and 't Hooft

As we have recounted, by 1960 the experimental situation had become clear. Weak interactions were indeed $V - A$ in their structure, so Sheldon Glashow could put together the ingredients assembled by Schwinger three years earlier, and propose that weak and electromagnetic interactions were unified, with vector (spin-1) bosons mediating the force between charged and neutral currents. The partial symmetry of non-Abelian gauge invariance required the existence of four gauge bosons; when the masses were diagonalized, these became the massless photon, familiar from electrodynamics, the charged $W^+$ and $W^-$ gauge bosons, and the neutral $Z$. (We now use the standard notation, which is that of Weinberg, in which the charged $Z$s of Schwinger and Glashow are called $W$s.) Phenomenologically, to explain the feebleness of the weak interactions, $W^\pm$ and $Z$ had to be very heavy, several tens of GeV. Glashow had to put these masses in 'by hand,' and could not explain why they were so heavy.[40]

That important detail was left to Steven Weinberg[41] and to Abdus Salam.[47] They, in turn, built upon the Higgs mechanism[48] in which masses are generated when a scalar field acquires a vacuum expectation value. (We may recall that the notion of a non-zero vacuum expectation value of a scalar field generating boson masses first appeared in Schwinger's 'Fundamental interactions' paper

[82].) That idea, an outgrowth of the earlier 'Goldstone mechanism,'[49] is very simple. Suppose we have a charged scalar field, represented by a complex field $\phi$ governed by the Lagrangian

$$\mathcal{L} = \frac{1}{2}\partial_\mu \phi^* \partial^\mu \phi + \frac{\mu^2}{2}\phi^* \phi - \frac{1}{4}\lambda^2 (\phi^* \phi)^2. \tag{12.35}$$

This seems to represent a charged particle of mass $\mu$, with quartic self-interaction. However, with $\mu^2 > 0$, that mass is imaginary, $\mathrm{i}\mu$, so the resulting theory seems crazy, having tachyonic states. However, there is a classical extremum, a constant field, identified with the vacuum expectation value of $\phi$, $\langle\phi\rangle$, for which the potential energy, represented by the negative of the last two terms in Eqn (12.35), is a minimum:

$$\langle\phi\rangle = \frac{\mu}{\lambda}. \tag{12.36}$$

The idea is to regard the quantum field as a fluctuation around this classical extremum. That fluctuation is sensible, having a zero mass and positive coupling, this being the Goldstone boson.

Now what happens when this charged particle is coupled to a gauge field? For illustration, we consider an Abelian field $A$. The Lagrangian describing that field, interacting with $\phi$, is obtained by the usual minimal substitution, replacing the particle momentum by the gauge covariant one:

$$\begin{aligned}\mathcal{L} =& \frac{1}{2}\left[\left(\partial_\mu - \mathrm{i}eA_\mu\right)\phi^*\right]\left[\left(\partial^\mu + \mathrm{i}eA^\mu\right)\phi\right] \\ &+ \frac{1}{2}\mu^2 \phi^* \phi - \frac{1}{4}\lambda^2 (\phi^* \phi)^2 - \frac{1}{4}F^{\mu\nu}F_{\mu\nu}.\end{aligned} \tag{12.37}$$

When $\phi$ acquires a vacuum expectation value, it does so in a particular direction in the complex $\phi$ plane. Without loss of generality, we may suppose that it is the real part of $\phi$ that acquires a non-zero vacuum expectation value. In fact, we may then choose a gauge in which $\phi$ has only a real part. Then, a bit of algebra reveals that the following magic occurs for the system consisting of the vector field $A_\mu$ and the real scalar field $\phi - \langle\phi\rangle$:

- The scalar field has a positive mass-squared, $\mu^2$, and
- The vector field also acquires a mass, equal to

$$m_A = e\frac{\mu}{\lambda}. \tag{12.38}$$

If the coupling with the gauge field had not been present, the imaginary part of $\phi$ could not have been transformed away: that would have been the famous massless Goldstone boson which is present whenever a continuous global symmetry (here, rotations in the complex $\phi$ plane) is spontaneously broken. But here, that massless degree of freedom has been 'eaten' by the photon, which then goes from the two degrees of freedom of a massless helicity-1 particle to the three degrees of freedom of a massive spin-1 particle. This is the Higgs mechanism.

It was the inclusion of this Higgs mechanism that was the principal innovation of Weinberg and Salam. Although the above illustration was confined to an Abelian gauge field, the mechanism may be easily generalized to a non-Abelian theory. There are, in fact, many possible ways of proceeding. The simplest possibility is to introduce a Higgs doublet of complex scalar fields, one being charged and one neutral, four states in all, $(\phi^+, \phi^0)$. Putting in a suitable Higgs potential, as above, it can be easily arranged so that all but one neutral Higgs component $H$ is eaten, so that the charged gauge bosons $W^\pm$ and the neutral weak boson $Z$ acquire masses. If $g$ is the $SU(2)$ coupling constant, and $g'$ is the $U(1)$ coupling, the mass of the charged and neutral gauge bosons are

$$M_W = \frac{1}{2} v g, \quad M_Z = \frac{1}{2} v \sqrt{g^2 + g'^2}, \tag{12.39}$$

where $v$ is the vacuum expectation value of the neutral scalar field. The photon remains exactly massless. The Higgs mechanism can also give masses to the fermions, the quarks and leptons in the theory. So the masses of the physical degrees of freedom, the fermions and the intermediate vector bosons, need not be put in by hand, but arise spontaneously.*

In 1967, when Weinberg wrote his paper, 'A model of leptons,'[41] he did not appreciate how crucial the Higgs mass generating mechanism was to be. He, and everyone else, still expected that the theory was non-renormalizable, that is, contained infinities that could not be absorbed by redefining parameters in the Lagrangian. This would mean that as calculations were carried out to higher and higher orders in perturbation theory, more and more infinities (divergences) would appear. In effect, the theory would not be truly predictive.

However, Weinberg held out the hope that the theory might prove renormalizable. 'Is our model renormalizable? We usually do not expect non-Abelian

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* However, it must be admitted, not without considerable artificiality. The scalar fields have no other role in the theory, and have unattractive features theoretically. Moreover, the masses of the various particles are not predicted, and only the ratio $M_W/M_Z$ is predicted once $g$ and $g'$ are determined from experiment. It remains a hope among most theoretical physicists that someday a consistent dynamical symmetry-breaking mechanism may be found, analogous to the way in which the 'photon' acquires a mass in two-dimensional massless electrodynamics, the Schwinger model [104, 108]. See pp. 433–434.

theories to be renormalizable if the vector-meson mass is not zero, but our $Z_\mu$ and $W_\mu$ mesons get their mass from the spontaneous breaking of the symmetry, not from a mass term put in at the beginning. Indeed, the model Lagrangian we start from is probably renormalizable, so the question is whether this renormalizability is lost in the reordering of the perturbation theory implied by our redefinition of the fields. And if this model is renormalizable, then what happens when we extend it to include the coupling of $\mathbf{A}_\mu$ and $B_\mu$ [the gauge fields before diagonalization] to the hadrons?'[41]

Salam shared this attitude: 'The whole thing has therefore worked out in a unified and beautiful manner; a renormalizable theory of massive vector mesons, in which electromagnetism is built in as part of the theory and the Lagrangian is perfectly symmetrical.'[47]

Indeed, four years later, Gerard 't Hooft showed, in a *tour de force*, that the Weinberg–Salam model was indeed renormalizable, very much like QED.[50] His proof depended on the sophisticated use of Feynman's path integrals, the formal 'solution' to Schwinger's functional equations, and on a new method of regulating the divergences of a quantum field theory, invented by 't Hooft and his advisor Martinus Veltman, called dimensional regularization.[51] Crucial to the demonstration was the fact that there were no mass terms in the Lagrangian, but masses introduced through the Higgs mechanism wriggled through the net. Once this paper appeared, everyone accepted the Weinberg–Salam model;* Weinberg wrote a second paper,[52] and the rest is history.

## The standard model and its successes

The first half of the 1970s was a very exciting period for the elementary particle physics community, for it marked the birth of the standard model of fundamental interactions. First the electroweak synthesis of Schwinger, Glashow, Weinberg, and Salam was recognized as a serious model of weak interactions, which was rapidly confirmed. Indeed, by the time the $W^\pm$ and $Z$ bosons were discovered in 1982,[53] all physicists believed firmly in their existence, so well established was the theory predicated upon them.

Even before 't Hooft had established the electroweak model's renormalizability, it was becoming clear how to incorporate the weak interactions of hadrons. Recall that Weinberg's 1967 paper[41] was called 'A model of leptons.' But if one took the quark model seriously, it is was quite obvious how to bridge the gap to

* By this point, most physicists had forgotten Glashow's 1961 paper,[40] and almost no one remembered Schwinger's seminal work [82].

the strongly interacting particles. The charged $W^{\pm}$ bosons couple to the lepton doublets,

$$\begin{pmatrix} e \\ \nu_e \end{pmatrix}, \quad \begin{pmatrix} \mu \\ \nu_\mu \end{pmatrix}, \tag{12.40}$$

so one could similarly imagine a coupling to a doublet made of the up and down quarks. Yet it was not so simple, for experimentally it was known that strange particles decayed weakly, but somewhat more weakly than non-strange particles. This could be accommodated (which is not the same thing as explained) by the introduction of the Cabibbo angle,[54] so, in fact, the appropriate quark doublet that couples to the charged weak bosons is

$$\begin{pmatrix} u \\ d\cos\theta_C + s\sin\theta_C \end{pmatrix}. \tag{12.41}$$

Here $u$, $d$, and $s$ are the up, down, and strange quarks, and $\theta_C$ is the Cabibbo angle, which has the experimental value $\sin\theta_C = 0.220 \pm 0.002$.[27]

Just as in the lepton sector, the existence of this charged current, coupling to $W^+$, must by the group structure imply a neutral current, composed out of the same doublet, coupling to the neutral weak boson $Z$. But then a serious difficulty emerges: Such a coupling implies 'strangeness-changing neutral currents,' that is, interactions which change strangeness but not electric charge, which is contradicted by an enormous body of experimental evidence. For example, such an interaction would imply that the decay of $K_L^0 \to \mu^+\mu^-$ would be comparable to the the predominant decay of the charged kaon, $K^+ \to \mu^+\nu_\mu$, whereas in fact the ratio of these two processes is less that $10^{-8}$.[27]

Glashow, Illiopoulos, and Maiani[55] proposed the solution to this problem in 1970.* If there was a second doublet of quarks, analogous to the second doublet of leptons, $(\mu, \nu_\mu)$, composed of a new quark called the 'charmed' quark $c$, such that the second doublet that coupled to the $W^+$ had, as its lower component, the orthogonal combination of $d$ and $s$ compared to that in the first doublet,

$$\begin{pmatrix} c \\ -d\sin\theta_C + s\cos\theta_C \end{pmatrix}, \tag{12.42}$$

* Glashow recalled that the idea of charm came somewhat earlier: 'In 1964, after a brief visit to Harvard, I returned to Copenhagen, where James Bjorken and I worried about the Cabibbo matrix, which is a sort of $3 \times 3$ matrix pretending to be a $2 \times 2$ matrix. It would be much prettier, we thought, as a $4 \times 4$ matrix. Then there would be a precise analogy between quarks and leptons. And so it was that playing around with small matrices (again!) that led us to charm; and somewhat later, to the explanation of the absence of strangeness-changing neutral currents.'[36]

it is easy to see that the strangeness changing neutral currents would exactly cancel. This cancellation was dubbed the GIM mechanism. In fact this was not sufficient. This cancellation would break down in second-order weak interaction, through processes in which two $W$ bosons were exchanged. These second-order processes would again exactly cancel if the masses of the $u$ and $c$ quarks were identical. The experimental limits implied therefore an upper bound on the mass of the $c$ quark of a few GeV.

In 1974, as we will recount on pp. 493–494, the $\psi/J$ particle was discovered,[56] followed by a host of new related states. These particles were remarkable for their long lifetimes, considering their great masses of around 3 GeV. It was soon understood that these were bound states of $c$ and $\bar{c}$ quarks, with their amazing stability due to the properties of the emerging theory of strong interactions, quantum chromodynamics (QCD). Within a matter of months, new baryons $\Lambda_c^+$ were found carrying the charmed quantum number,[57] followed by new charmed mesons $D^0$, $D^+$.[58] So experimentally then there were two families of leptons and quarks, the first consisting of (e, $\nu_e$), $(u, d)$, and the second $(\mu, \nu_\mu)$, $(c, s)$, with the Cabibbo mixing of the $d$ and $s$ quarks in the weak interactions.

But before this pretty picture could be appreciated, new states continued to appear. A new charged lepton, the $\tau$ was discovered,[59] which presumably was associated with its own neutrino, $\nu_\tau$. In 1977, the newly opened Fermilab discovered a state[60] carrying a new quark dubbed $b$, for bottom or beauty. The obvious picture, which was nearly required for a generalization of the GIM mechanism to suppress 'flavor-changing neutral currents', was that $b$ was the second component of a third doublet of quarks, the first being called $t$, for 'truth' or top. Then the third generation of quarks and leptons would be $(\tau, \nu_\tau)$, $(t, b)$. But, as Glashow sadly noted a few years later, although the evidence for beauty was compelling, there was no evidence for truth. In fact, it took 20 years to find the top quark, which was at last found at Fermilab in 1995[61] at an amazingly large mass of 174 GeV. No further quarks have been found, and there is indirect evidence that there are only three families of quarks and leptons—why, we don't know.

Once a consistent electroweak theory had been found, it was but a short time until a similar gauge theory of strong interactions was proposed. This theory, quantum chromodynamics or QCD, was the work of many physicists. A key ingredient was the concept of color, introduced[62] in order to explain a conflict between the Fermi–Dirac statistics of spin-$\frac{1}{2}$ quarks and the nonrelativistic quark model. The name *color* was adopted because quarks had three of these attributes, so the corresponding 'rotations' were those of the group of $3 \times 3$ matrices of determinant one, $SU(3)$. It was then rather obvious to consider the coupling of the color current to a set of eight massless gauge bosons, which were dubbed gluons, since they glued the quarks together:

$$\mathcal{L}_c = g j^a_\mu A^\mu_a, \quad j^\mu_a = \bar{\psi}\gamma^\mu \frac{\lambda^a}{2}\psi, \tag{12.43}$$

where $\lambda^a$ is the generator of the group of matrices, as the Pauli matrices $\tau^a$ are the generators of $SU(2)$. What made this theory viable was the remarkable discovery by Politzer, Gross, and Wilczek that non-Abelian gauge theories are asymptotically free—that is, that the couplings between the quarks get weaker as the energy gets higher.[63] Thus, a crude, but remarkably accurate picture of hadrons is that they consist of quarks rattling around nearly freely in a volume of order of 1 fermi in radius, yet never able to escape from each other (confinement). This picture was formalized in the MIT 'bag' model,[64] which remains a useful starting point for understanding hadrons.

This then is the Standard Model: quantum electrodynamics (QED), brought to completion in the work of Schwinger, Feynman, Tomonaga, and others in the late 1940s and early 1950s, joined (but not unified) with weak interactions in the electroweak synthesis of Schwinger, Glashow, Weinberg, and Salam; this $SU(2) \times U(1)$ theory sits side by side with the $SU(3)$ theory of strong interactions, QCD. The gauge bosons are the photon, the weak bosons, $W^\pm$ and $Z$, and the eight gluons. Matter is made of quarks and leptons, which come in three families; in all there are six quarks and six leptons. Each flavor of quark carries three colors; and of course each quark and lepton has its antiparticle.

But this picture leaves much unanswered. We do not know why the group structure is the way it is, or why there are exactly three families. We certainly do not understand the origin of mass, which in the standard model is put in 'by hand' through the Higgs mechanism, involving the introduction of unesthetic scalar fields. There are a great many arbitrary parameters to be put into this picture: the masses of all the quarks and charged leptons (and perhaps of the neutrinos), the mixing angles between the quarks, generalizing the Cabibbo angle, the strengths of the various couplings, which are usually described by the value of the fine structure constant, $\alpha$, the weak angle, $\theta_W$ (the $\theta$ of Eqn (12.34)), and the strong coupling constant at some energy scale, say $\alpha(M_Z)$. So nearly everyone is sure that there must be something 'beyond the Standard Model.' But, in fact, there is hardly any evidence for any breakdown of this picture, in spite of the strenuous efforts of thousands of talented physicists for two decades. And attempts to unify these forces and particles of nature, from Grand Unified Theories, through Supergravity, to SuperStrings, have led to almost no verifiable contact with the real world. For example, most of these unified theories predict the existence of supersymmetry, which we will discuss on pp. 521–522. Yet there is no serious evidence for such a symmetry, which taken literally would imply that for every boson there must be a fermion of exactly the same mass, and vice versa. This is the *fin de siècle* crisis of elementary particle physics.

But Schwinger, characteristically, was largely unconcerned with all of this. He had his own path to follow, not that of the crowd.

## Conclusions

One outsider's view of the impact of the 'Fundamental interactions' paper has been given by Nina Byers[65]. She recalled that while she was a graduate student at Oxford at the time that paper came out, she was assigned by Rudolf Peierls the task of reading the paper to present to the theoretical group at Oxford. As she was not familiar with Schwinger's work and methods of thought, she had to spend a great deal of time preparing, reading earlier papers and the like. She discovered, in her words, 'the first use of Lie algebras in physics.' She eventually gave very successful reports on the paper at Oxford, so successful that Abdus Salam invited her to Imperial College to make presentations there. This, she recalls, was the precursor to Salam's work on the electroweak unification. Thus, two of the three 'authors' of the electroweak synthesis trace their source to Schwinger.

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# 13

# The Nobel Prize and the last years at Harvard

## The Nobel Prize and its aftermath

As soon as Schwinger burst upon the stage, one could hardly doubt that a Nobel Prize was in the offing. Certainly, after his solution of the problems of quantum electrodynamics in the late 1940s the award of the Nobel Prize was just a matter of time. Yet years passed with no news of the award. Clarice Schwinger, his devoted wife, described waiting for the Prize. Clarice thought he would get the Prize soon after they got married. When it did not happen, she remembered the story of Goudsmit and Uhlenbeck* whom everybody felt should have received it and did not, and she then decided Julian simply was not going to get it. Obviously he deserved it. It just was not going to happen. His mother never gave up. Every year she would call Clarice and want to know 'Why didn't he get it?' Clarice could never quite tell her why, but after a certain year she never thought of it again.[2]

But at last the call came: the Schwingers did not have a telephone in their bedroom because Julian hated the telephone and he certainly did not want to be awakened to hear chatter. Clarice's mother, on the other hand, had a telephone in her room and so, on the fateful day, a reporter woke her at 7 o'clock in the morning. She knocked on Julian and Clarice's door, frightening them because she wouldn't knock on the door unless it was something awful and she needed them. She said 'They say you've won the Nobel Prize. You must come.' Clarice said, 'They're joking!' She told Julian the report, and went to the telephone to talk to the reporter. He repeated the message, and at first Clarice did not believe him, but it was true.[2]

Schwinger had apparently had some advance warning of what was coming: When Clarice came tearing back saying, 'Julian, Julian! You've got the Nobel Prize!' Julian was not quite as excited as Clarice. Apparently someone from Sweden had been at Harvard a few days before and talked to him, which to

* Sam Goudsmit and George Uhlenbeck proposed the concept of electron spin in 1925.[1]

him was sort of a precursor of things that might come. But Julian had not told Clarice about that hint.[2]

Clarice went on to describe that wonderful, hectic day. They got dressed; Clarice's mother fixed coffee, and by the time they got downstairs there were reporters already at the door. It was a day filled with telegrams and telephone conversations and people coming to the door. Complementing the excitement, the weather was perfect: it was an absolutely beautiful fall day with golden leaves on the ground. Later, Julian and Clarice went for a walk. They scuffed at the leaves with their shoes, and people they hardly knew came out of their houses when they saw them to tell them how happy they were for them. In the evening Julian's students organized a fantastic party at their house. Clarice retained a photograph of empty champagne bottles lined up on the dining room table in their home. They had invited friends and family and colleagues. It was a lovely time, with warmth and affection that was never to be forgotten by Julian and Clarice. It was really one of the happiest days of their lives.[2]

It took no time at all to determine how to spend the prize money. The incident occurred on that very day. Their house in Belmont was one of only three houses made of concrete, a remnant of an experiment made in the 1930s. It was a very expensive house to maintain and it needed sandblasting; in Boston not many people sandblasted. They had a dreadful time getting anybody to come look at the house to give an estimate; however, it was done, but the estimate was for an outrageous sum, which the Schwingers had agreed to think about. They had an appointment with one of the contractors that morning and Clarice remembered looking out and seeing the man under the elm tree kicking up the leaves. When they came out he said, 'Now you can have the house done.' And he was right, because that was where nearly all the prize money went.[2] Schwinger's share of the Prize came to only 73 200 Swedish Kronor, or $13 200,[3] so after sandblasting the house there was only enough money left to pay for the air fare to Stockholm for Clarice's mother and to buy Clarice a *Volvo* in Sweden. Julian's brother Harold paid his own air fare.

Schwinger immediately sent congratulatory messages to his co-recipients of the Prize, Sin-itiro Tomonaga and Richard Feynman; apparently neither of them sent anything to him.[2]

Of course, congratulatory letters and telegrams poured in from all over, from friends, relatives, and colleagues throughout the world. Clarice still keeps these treasured mementos. One telegram, from Steve and Miriam White, consists of the lyric, 'It ain't the money, it's the principle of the thing.' This was quoted from a song composed by physicist Arthur Roberts on the occasion of I. I. Rabi's receiving the Nobel Prize in 1944.[3,4] The line was certainly appropriate, given the small monetary value of the Prize in those days; Schwinger had previously referred to the line in connection with the Charles L. Mayer Nature of Light Award.

The University had an evening party for him. Unfortunately, Clarice got a low-grade infection and was sick through most of it, but it was a very warm party.[2]

When the time came to travel to Stockholm, Clarice and Julian traveled separately: in spite of their love of travel, they were, in Clarice's words, 'dreadful travelers.' They had great difficulty with jet lag. And so they had planned to go to London for a few days early; at the last minute, Clarice simply could not leave. Julian went by himself to London and Clarice flew to Sweden on her own. Consequently, Clarice arrived there a day before Julian. She and her mother were met by a charming couple from the Swedish government, Kerstin Evers and Jan Kronholm, who took them to the hotel. The following day they went to meet Julian's plane; the escorts kept waiting at the first class exit, but Clarice said, no, look at the coach class exit. And she was right, he had come by coach. The Schwingers immediately struck up a friendship with Jan and Kerstin. They were very kind and helpful: There was nothing that the Schwingers wanted that they would not do.[2]

Clarice recalled walking in Stockholm. They were recognized everywhere as celebrities. If they had walked into Cambridge, nobody would have known them, but in Stockholm everybody knew who they were.[2]

Seniority had its privileges: Everything was done by prestige. Clarice went into dinner with the king, 'not because of my charm and wit and beauty,' but because Julian was three months older than Feynman; if Tomonaga had come, Mrs Tomonaga would have been escorted by the king. (Tomonaga did not come because he had an accident while celebrating the announcement of the prize.) Again, because Julian was the most prestigious, they got the most beautiful bedroom in the Grand Hotel. Everybody else's—her mother's and Schwinger's brother Harold's—were unimpressive rooms.[2]

The banquet was held at the City Hall. Because of his seniority they were in the first car in the entourage and Clarice sat next to the king at dinner at the banquet. On the other hand, at the dinner at the palace, the chemist's wife Mrs Woodward and Gweneth Feynman sat on either side of the king. The banquet was a little difficult; one thing Clarice had forgotten was that she was not supposed to speak to the king until he spoke to her. The first course was fish, and the king had a fish bone, so Clarice leaned over to remove it just like at home.[2]

Unfortunately, Clarice was not well that evening, so somebody was sent up to wash and set her hair. They went off on a rainy night in the car; they were assembled and took their places and were introduced to the king. Clarice curtsied, as she had been taught to curtsy, and he gave her his arm and they went off to dinner. Clarice recalled that it was not the chattiest evening she had ever had, but it was one of the grandest.* Clarice sat with Julian the next night at the palace and then she had a good time. But they left early, because Clarice was not feeling well; it was all she could do to stand up. Ordinarily she would have 'danced all night.'[2]

Feynman enjoyed himself at the City Hall, and afterwards Clarice and Feynman sat together for the ceremony of the frog that the students have. They sat and chatted and had a good time that night. He was having a hard time with his collar, which was scratching his neck; Clarice persuaded him to line it with a piece of Kleenex which made it feel better. Clarice always loved Feynman and they enjoyed each other's company very much on the rare occasions when they saw each other.[2]

We will discuss Schwinger's Nobel lecture in the following section. Undoubtedly, he was too excited by the whole glittering affair to pay close attention to the other lectures, even Feynman's. Indeed in a letter published by *Physics Today*[6] amending his retrospective article about Feynman [212], Schwinger pointed out that when he belatedly reread Feynman's Nobel lecture he found that it largely anticipated the initial stages of Schwinger's ideas about source theory, six months before that development. This may have led to a 'subliminal implantation' in Schwinger's mind. Consequently, he was then [1989] 'happy to acknowledge Richard Feynman as a virtual source of source theory.'

Finally it came time to leave. Everybody held their hands until it was time to leave and then there was nobody; they had to bring down their own bags and take themselves to the airport. Nobody was there to say anything or to do anything. They stole quietly away. They met the Feynmans at the airport and laughed because after all that incredible treatment it just vanished 'like Cinderella's pumpkin.'[2]

Schwinger bought Clarice her first car, a *Volvo*, in Sweden. Earlier, he had taken her to an automobile show, because he wanted her to have a car, while Clarice had no real desire to drive. But Julian loved cars, so he persisted in taking her to automobile shows. Julian sat Clarice in car after car but she would say it did not feel right. Then he put her in a *Volvo* and she said this was great, this was right. But it did not mean anything to her. Finally, when he got the prize they decided it felt right enough for Clarice to have one.[2]

They drove Clarice's new *Volvo* to Lund afterward to visit Gunnar Källén. Clarice again enjoyed meeting him very much. Although he was famous, like

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* Gweneth Feynman, who also sat next to the king, said on the contrary that 'The king was marvelous, he really was. Conversation was no problem at all. He's so practiced he could talk to anybody.'[5]

Pauli, for having a sharp tongue, Clarice had no problem with him. His wife and Clarice became very good friends when they met in Trieste in 1962, so after the ceremony in Stockholm they went to Lund to visit them.[2]

Life back home did not change that much, partly because, by and large, Schwinger refused to accept invitations or speaking engagements. There was a slight excitement in the air, but in general they lived their lives as they had always done. People did not come at them, as they did to other laureates. Bethe, for example, after he got his Prize, was absolutely exhausted. He did not know how Schwinger had withstood all the things that were put on him. But Bethe was already out in public life, giving many public lectures. Since Julian had never done this, nobody bothered him. He did begin to get some requests for autographs; for a while he signed them and Clarice sent them off. Clarice felt very dutiful about that. Then she saw Willis Lamb, who looked at her as though she were absolutely mad. He said it was an imposition; he never answered such requests. And so they stopped responding.[2]

In the wake of the Nobel Prize, in 1966 Schwinger received an endowed chair: he was appointed Higgins Professor at Harvard.

## The Nobel lecture and the new perspectives

We now turn to Schwinger's Nobel lecture, entitled 'Relativistic quantum field theory,' delivered on 11 December 1965 [132]. He reviewed the path he had blazed in formulating a covariant quantum electrodynamics, which we have outlined in the previous chapters, with particular attention to the quantum action principle and Lorentz invariance. He pointed out the connection between Euclidean transformations and the TCP theorem [65, 73]. The distinguished role of 'local systems,' spin 0, $\frac{1}{2}$, and 1 fields, in satisfying the local energy–momentum commutation relations was brought out [107, 111, 112]. He presented his view of renormalization as the connection between fields and particles. And he mentioned his very recent work on the renormalization of magnetic charge [133, 134]. We discussed all these developments in Chapter 11. But the provocative part of his address appeared when he discussed strongly interacting systems and remarked: 'A field operator is a localized excitation which, applied to the vacuum state, generates all possible energy–momentum, or equivalently, mass states that share the other distinguishing properties of the field. The products of field operators widen and ultimately exhaust the various classes of mass states. If an isolated mass value occurs in a particular product, the state is that of a stable particle with corresponding characteristics. Should a small neighborhood of a particular mass be emphasized, the situation is that of an unstable particle, with a proper lifetime that varies inversely as the mass width of the excitation. The quantitative properties of the stable and unstable particles that may be implied by a given dynamical field theory cannot be predicted with presently available calculational techniques. In these matters, to borrow a phrase of Ingmar Bergman, and St Paul, we see through a glass, darkly. Yet, in the plausible inference that a substantial number of particles, stable and unstable, will exist for sufficiently strong interactions among a few fields lies the great promise of relativistic quantum field theory.' [132]

He then went on to discuss $SU(3)$ (flavor) symmetry of strongly interacting particles, and his own model of fields [118, 124], transforming as triplets under $U(3)$, where 'products of two and three fields ... represent the general properties of mesons and baryons, respectively.' Such field products might 'suffice to describe the excitation of the known relatively low-lying particles. The resulting quasi-local structures are in some sense fields that are associated with the physical particles. I call these fields phenomenological, as opposed to the fundamental fields which are the basic dynamical variables of the system.' [132]

He concluded the lecture with the vista from the top of the mountain he had heroically scaled: 'Phenomenological fields are the basic concept in formulating the practical calculation methods of strong interaction field theory. They serve to isolate the formidable problem of the dynamical origin of physical particles from the more immediate questions referring to their properties and interactions. ... One has still to appreciate the precise rules of phenomenological relativistic field theory, ..., given that the strong fundamental interactions have operated to compose the various physical particles. And when this is done, how much shall we have learned, and how much will remain unknown, about the mechanism that builds matter from more primitive constituents? Are we not at this moment,

> ... like stout Cortez when with eagle eyes
> He star'd at the Pacific—and all his men
> Look'd at each other with a wild surmise—
> Silent, upon a peak in Darien.[7] [132]'

One is tempted to read into this 'phenomenological field' approach to strong interactions a prefiguring of his attempt to create a source-theory revolution six months later.

And indeed it was. Only two years later, at the 1967 International Conference on Particles and Fields held in Rochester (not to be confused with the 'Rochester Conferences') before a distinguished group of physicists, Schwinger gave one of his first public presentations of the new source theory. He opened with the words 'Several years ago during the course of a talk that I happened to be giving in Stockholm, I issued an appeal for the development of a logically founded phenomenological field theory. Since that call was met with widespread apathy, I had to do the job myself. The result of that is the theory of sources, which is what I want to tell you about in a highly inadequate fashion today. Actually it worked out rather differently from what I had in mind when I proposed the program several years ago. While the theory of sources is a field theory, it is not an operator field theory. The original idea was to follow the lines of conventional field theory, that is, to introduce field operators which would be directly connected with the physical particles, and to do this in a way that would somehow bypass the presently impossibly difficult question of the actual constitution of the strongly interacting particles, and in this way seek to find a technique that was useful for direct confrontation with experimental data.'* [139a]

## Source theory

It surely was the difficulty of incorporating strong interactions into field theory that led to 'Particles and sources,' received by the *Physical Review* barely six months after his Nobel lecture, in July 1966 [135], based on lectures Schwinger gave in Tokyo that summer. Clarice recalled that first trip to Japan. They left from Los Angeles, went to Honolulu for a few days, then arrived in Tokyo, where they were met by Kazuhiko Nishijima and H. Fukuda. They arrived at 11:00 at night and they were exhausted. Their hosts said, you know it's now August, and Tokyo in August is hell, it's so hot and humid. They decided to give them a light supper. So they took them to a beautiful restaurant that served only sashimi, which is raw fish. The Schwingers were petrified. There was an enormous and beautiful platter of sashimi. If they didn't like it, how were they going to behave? But they had nothing to worry about: it was absolutely delicious and they ate it all. It was a traditional Japanese restaurant in which one had to take off one's shoes. They were the last ones to leave the restaurant, and when it came time to leave, Julian was presented with a pair of black pointed-toe shoes, size 16. He had come in with brown shoes, size 9D. Someone had taken his shoes. But at 9:00 the next morning there was somebody at the door with Julian's shoes, and he gave back the black ones. That was their first day in Japan. Shortly thereafter

* In another version of these remarks he added: 'I originally had in mind that the fluctuation phenomena inherent in an operator field theory would largely cancel out in consequence of the total particle dynamics, giving some consistency with the initial phenomenological description. That is a very difficult program. Several people have also remarked recently on the utilty of a phenomenological field description, while maintaining puzzled reference to the necessity for omitting the higher order effects, the fluctuation phenomena, that an operator theory logically demands. The difficulty is eliminated in the source formalism which is a numerical description of the actual processes, while retaining the freedom to explain dynamics correlating different phenomena.'[3] This, or a similar talk, began with the words, 'My talk today will resemble a political speech. I shall attack past policies and methods, and call for a wholly new approach to our problems.'[3]

they went on an excursion to Nikko. They went to Chūzenji-ko Lake and to Kegon Falls.[2]

This trip was also probably the first time Schwinger really talked to Tomonaga.[8] On this trip also, Clarice recalled, there occurred a 'great triumph' for Julian. Before they had left home, he had studied a little Japanese. Everybody had told them that when they got to Tokyo they must get the hotel to provide written instructions explaining how to get where they wanted to go to give to the taxi driver. So they did indeed do that, in order to go to a particular restaurant. Nevertheless, as happens so frequently in Japan, the taxi driver could not find the place; they went round and round and up and down the streets, when all of a sudden Julian said, 'There it is.' The taxi driver had not seen it but Julian had.[2]

In this first source theory paper, particle phenomenology is primary, and, for example, he cited George Kalbfleisch *et al.* in the second sentence of the introduction for the discovery of the $\eta'$ meson,[9] a particle which, as we noted in Chapter 11, Schwinger had predicted in 1964 [119]. As he noted later [160], although the source concept had been introduced in 1951, it was only 15 years later that he realized the whole theory could be based upon such idealized particle creation and annihilation processes. 'But it was not until the spring of 1966, while teaching a Harvard graduate course, that I suddenly realized how the phenomenological source concept could be freed from its operator substructure and used as a basis for a completely independent development, with much closer ties to experiment.

'The reconstruction of electrodynamics proceeded rapidly, at UCLA that summer, and during a repetition of the Harvard course that was, instead, devoted entirely to the new approach.* Developments in pion physics that winter (1966–1967), in which the new viewpoint was most successfully applied, convinced me, if no one else, of the great advantages of mathematical simplicity and conceptual clarity that its use bestowed. The lack of appreciation of these facts by others was depressing, but understandable.'[10]

The source concept was introduced as an idealization of what happens in the laboratory. A particle, say the $\eta'$, is produced in some collision process, it travels some distance, so its mass and momentum can be more or less well specified, and then it decays or is detected. Schwinger put it well in his Rochester talk: 'In high energy physics we are concerned by and large with phenomena which involve the transmutation of particles, the creation of particles, the decay of particles, and so on. It is the dynamics of these processes that we want to try to understand. In general, we deal with highly unstable particles, and therefore

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* The notes from Schwinger's lecture courses of spring 1966 and fall 1966 show that source theory appeared full blown in the latter course, but the germination had started during the previous semester.[3]

part of the general process of studying the phenomena is to create the very particles we are concerned with.'

'Obviously, the details of that creation process are not of interest. They are there merely to create the particles that we want to study. But once those particles are created and then, at the other end of the experimental apparatus, detected, we have the means of studying their interactions. The description is conveniently given in terms of the source idea, the source being simply a word to describe the fact that when we create particles, by and large, all that is relevant in any actual collision process is that the very attributes of the particle in question are present and the others simply supply the necessary energy, angular momentum, isotopic spin and so on. The source concept is an idealization of the realistic processes, and is used to bring the physical processes into existence, to create the particles of interest. We then study the actual phenomenology, and finally the particles are detected. It is not relevant by what particular means we detect the particles. It is part of the experimenters' creed, after all, that we are studying the properties of the particles but not the particle plus the detection apparatus. So, the actual details are irrelevant and the source concept is introduced as a convenient way of describing the fact that the particle passes on its attributes to other things, which, as part of the detection apparatus, act as sinks for those properties. Otherwise the details are by and large irrelevant.' [139a]

For electrodynamics the photon source is a vector function of space–time, $J^\mu(x)$, which is conserved,

$$\partial^\mu J_\mu(x) = 0, \tag{13.1}$$

while the electron source is an anticommuting numerical function, $\eta(x)$,

$$\{\eta_\zeta, \eta_{\zeta'}\} = 0, \tag{13.2}$$

where the spinorial indices, $\zeta, \zeta' = 1, 2, 3, 4$, have been made explicit. The vacuum persistence amplitude, that is, the quantum-mechanical probability amplitude that if the system is in the ground state (vacuum) before the sources turn on, it remains in that state after the sources are turned off, can be written as

$$\langle 0_+ | 0_- \rangle^{\eta J} = \exp\left\{ \mathrm{i} \left[ \int (\eta\gamma^0\psi + JA) + W[\psi, A] \right] \right\}, \tag{13.3}$$

'where $\psi(x)$ and $A(x)$ are now *numerical* fields, numbers of the same type as $\eta(x)$ and $J(x)$, and [the effective action] $W[\psi, A]$ is only implicitly defined through a stationary requirement:

$$\frac{\delta}{\delta\psi\gamma^0} W + \eta = 0, \quad \frac{\delta W}{\delta A} + J = 0. \tag{13.4}$$

In consequence of the stationary property, we have

$$\frac{1}{\mathrm{i}}\frac{\delta}{\delta\eta\gamma^0}\langle 0_+|0_-\rangle^{\eta J} = \psi\langle 0_+|0_-\rangle^{\eta J}, \tag{13.5}$$

$$\frac{1}{\mathrm{i}}\frac{\delta}{\delta J}\langle 0_+|0_-\rangle^{\eta J} = A\langle 0_+|0_-\rangle^{\eta J}, \tag{13.6}$$

and the Dirac functional equation, for example, becomes [notice that this is equivalent to Eqn. (9.56)]

$$\left[\gamma\left(\frac{1}{\mathrm{i}}\partial - e_0 q\left(A + \frac{1}{\mathrm{i}}\frac{\delta}{\delta J}\right)\right) + m\right]\psi = \eta; \tag{13.7}$$

[where $e_0$ is the bare charge, and $q$ is the charge matrix, with eigenvalues $+1$ for the positron and $-1$ of the electron] .... Here, then, is a formulation completely equivalent to the original one in terms of operator fields, commutation relations, and all the rest, but now expressed in the language of numerical sources, numerical fields. And it is this formulation that has the flexibility to permit a new beginning, a fresh, more physical approach to particle theory. The sources were initially tied to the operators $\psi$ and $A$ which describe elementary, multi-particle excitations. Why not abandon the whole operator framework and define the sources *ab initio* in terms of the excitation of single, physical particles? This is the starting point of source theory.' [160]

One must appreciate the milieu in which Schwinger worked in 1966. For more than a decade he and his students had been nearly the only exponents of field theory, as the community sought to understand weak and strong interactions, and the proliferation of 'elementary particles,' through dispersion relations, Regge poles, current algebra, and the like, most ambitiously through the $S$-matrix bootstrap hypothesis of Geoffrey Chew and Stanley Mandelstam.[11–14] What work in field theory did exist then was largely axiomatic, an attempt to turn the structure of the theory into a branch of mathematics, starting with Arthur Wightman,[15] and carried on by many others, including Arthur Jaffe at Harvard.[16] (The name changed from axiomatic field theory to constructive field theory along the way.) Schwinger looked on all of this with considerable distaste; not that he did not appreciate many of the contributions these techniques offered in specific contexts, but he could not see how they could form the *basis* of a theory. 'I think of all these developments, the only one I was mildly interested in was the dispersion relations, which I saw not as a new foundation but simply as a useful phenomenological connection between different parameters that could be measured .... To me I saw dispersion relations simply as something that was a prediction of field theory, a prediction perhaps of any relativistic causal theory. In fact, that is what bothered me, that the relations were too general to be obviously a suitable foundation for the theory. ... I entirely rejected the notion that this was a new starting point.'[8] (We might recall that he had sufficient interest in dispersion relations to work on them for a time in 1957. And, in effect, they played a major role in his presentation of source theory.)

As for current algebra, 'The attempt was to replace all fields by currents and so it was a direct attack on field theory. Again it had some empirical consequences that were useful and there were some things that could be compared with experimental data.... In fact I remind you that since current algebra is based on hypotheses about commutators and I had by then already established the difficulties in working with commutation relations that I saw no reason to trust—the famous Schwinger terms were there to say beware, that if you write down a whole list of commutators they may be inconsistent with other physical requirements.'[8] (See Chapter 11.)

And the axiomatic approach was antithetical: 'I am convinced that there's new physics to be found as we go to higher and higher energies or whatever and I regard it as a mistake to try to axiomatize—You have said no new phenomena will ever be found that lie outside this framework and that struck me as a quite absurd approach to what is obviously an open universe with new things to be found.'[8]

But the practical successes of current algebra made an impact on Schwinger's thinking: 'Now, here pressed by the necessity of making contact with fundamental data, I was beginning to think of effective or phenomenological Lagrangians and that ... would inevitably drive me into saying perhaps one should not start out with a fundamental theory at all, which after all involves speculations about arbitrarily high energies when the concern is with correlating and understanding and predicting data at accessible energies.'[8]

Recall that Schwinger actually began to become unhappy with operator field theory in 1962, while working on the two papers entitled the 'Quantized gravitational field' [113, 114]. 'I think it was these papers that pushed me over the edge, the complexity that followed from the operator nature of all of these fields simply said to me that this was not the real physics, this was unnecessarily complicated. ... The difficulties seemed out of proportion to the nature of the physical questions being asked. It seemed as though the operator formalism was creating problems of its own rather than being the best way of representing the field situation.'[8]

Schwinger attributed his iconoclastic attitude, which eventually led to source theory, to Rabi, in a talk Schwinger gave at the Rabi Symposium at Columbia in 1967: 'My story begins in this room a third of a century ago (and a third of a century ago I had no trouble talking to the people up in that last row). I was then a graduate student. Excuse me. I was not a graduate student. I was then an undergraduate student at Columbia where I had come, thanks to the kind offices of Professor Rabi, and I was then giving a colloquium. The colloquium was on the fairly recent subject of the theory of beta decay, and since I had never learned to end a colloquium on time—I certainly didn't appreciate the problem then—I was going on at great length, approaching the hour of 6 o'clock. Professor Rabi, I believe, was becoming quite worried about the people who he knew had to catch various trains to their suburban houses. And so, he began to, or he attempted to, stop me. At that particular point, I was discussing the then very fashionable beta-ray theory of nuclear forces, which surely practically none of you have ever heard of, and I was describing the merits of this theory with great enthusiasm when Rabi saw a possibility to stop me. He then said, "You don't really believe that, do you?" And that had its effect. I'm sure I mumbled something affirmative, but the cause was lost and the lecture ceased very soon after that.

'Now Rabi accomplished more than he knew in making that statement. Of course, his immediate purpose was to stop me, and that succeeded. But he did more than that; he planted the seeds of doubt. You see, to that point it had never occurred to me to question the wisdom of my colleagues and co-workers, or people who were soon to become co-workers. But here was the possibility of doubting the ideas that were then in vogue—the ideas that were current. And of course, as you know, or are about to learn, the beta-ray theory of nuclear forces was sheer nonsense and was practically immediately superseded by the ideas of Yukawa which are still with us in some more or less unrecognizable form. Now, here then was the point which I began to appreciate, that it was possible—in fact, it was sometimes desirable—to move against the current of what was then generally accepted thought, that what one's colleagues believed at a particular moment of time was not necessarily the actual, effective, eventual development of thought in the realm of physical theory.'[3]

### What is Source Theory?

Although Schwinger had invented the notion of a source at least as early as 1951, it was only in 1966 that he realized that he could base the whole machinery of particle physics on the abstraction of particle-creation and annihilation acts. One can define a free action, say for a photon, in terms of propagation of virtual photons between photon sources, conserved in order to remove the scalar degree of freedom. But a virtual photon can in turn act as a pair of electron–positron sources, through a 'primitive interaction' between electrons and photons, essentially embodied in the conserved Dirac current. So this multiparticle exchange gives rise to quantum corrections to the photon propagator, to vacuum polarization, and so on. All this without any reference to renormalization or 'high-energy speculations.'

Source theory 'was a final stage which required everything before it. The recognition that the physical quantities that you are interested in were not the fields but the correlations between fields and the recognition that the correlations between fields are really Green's functions, relates it back to inhomogeneous differential equations, which therefore take into account not only how the particles behave but how they are created. The sources are the way of cataloging the various Green's functions. The final point at which the theory asks to be compared with experiment, not in some numerical detail, but in its general structure, involves just pure numbers, Green's functions and sources, not operator fields. . . . If you did a calculation of some interesting physical quantity and it took you 10 pages then surely there was another way of doing it that would take one page and you would get to the heart of the matter. . . . The whole line of development forces one to take the next step, and it has always astonished me that nobody wants to take that step with me.'[8]

Schwinger made an analogy with his reformulation of quantum mechanics in 1951. 'I said, let's look at a measurement, and let us idealize it and symbolize it and out of that evolved all quantum mechanics. So I said, very well, particularly in the area of high-energy physics, particles do not exist until you create them and the act of creation is an identifier and a way of characterizing the particle and all of that machinery of accelerators and collisions and whatnot is symbolized, abstracted by the source. A source is simply a symbolization of the act of beginning an experiment. The source is now introduced quantitatively as a direct measure of the probability amplitude for creating that particle.'[8]

The basic quantum-mechanical amplitude is the vacuum-to-vacuum transition amplitude. 'You are interested in the particles, you abstract from the apparatus. Now, what is the starting point before the particle has been created? Nothing, a vacuum. So you begin with a vacuum. Then mentally but not explicitly you describe the physical apparatus acting to produce a particle. The particle propagates, then it gets detected. The detection means in fact, of course, that it gets transmuted to other more easily accessible forms. But what have we abstracted from that? When the particle has been detected, the only thing of interest is that the particle has disappeared, you are back to the vacuum. So the description of all phenomena begins with the vacuum and ends with in the vacuum and all physics is in that single, shall we say, time transformation function.'[8]

There were great formal advantages in the new approach in Schwinger's view. 'You have talked yourself in a few lines what in [operator] formalism takes weeks. There are no equations of motion, commutation relations, here. So by conversation using just the simplest aspects of quantum mechanics and special relativity, you end up with a quantum-mechanical description of non-interacting particles.'[8]

Interactions between particles are now introduced as 'the simplest descriptions of how the particles can interact in which the interaction now acts as a source. From these idealized sources we are now getting to realistic sources produced by the collisions of the particles to produce other particles, which is what we had in mind in the first place. So there's an element of self-consistency.' Numerical fields are introduced as a measure of the change in the system caused by a change in the source. In terms of fields and sources, the vacuum persistence amplitude can be expressed as the exponential of a numerical action expression, which satisfies a stationary principle. Here interactions do not imply infinities or renormalization. 'When you enter into new areas of experience, you learn what the appropriate things to do are. But everything you learn in that larger area of experience must not change what you've already put in. In other words, the approach is not renormalization but *normalization*. Keep what you've already correctly put in. When you introduce new possibilities, don't change what you had before.' The approach is from below, 'whereas the usual theory is supposed to be the theory of everything. Now you perhaps limit it to electrodynamics, but all electrodynamic processes are supposed to be in the original operator field equations. Here you don't do that. You begin by describing the simplest situations which are controllable, and then you begin to enlarge, always with this feedback that you started from the source as an idealization of interactions and then when you put in an interaction it must use that idealization as a model.'[8]

There are no 'rules.' 'The word *rule* bothers me tremendously. It is just a mindless application of agreed upon procedures. If there is any difficulty with source theory it is that you are required to think. It is not a mathematical scheme in which somebody says here are the equations, now you are a mathematician. You must proceed more like an experimenter. Does an experimenter operate under rules? He keeps an open mind.' Schwinger's $2\frac{1}{2}$ volume treatise on source theory, *Particles, sources, and fields* [211] 'repeated and vastly improved every calculation in electrodynamics, almost every one that's ever been done. I think it's like—aren't we back to the old days of the master and the apprentice? To see how it works you have to see the master at work. Then after a suitable period, you are inducted into the society who are capable of doing—it is consciously a motivated approach which means there are as such no rules. There are general lines of thought, there are procedures, there is a pattern, but not rules in the sense that you mindlessly, when you're given this combination of symbols, do that.'[8]

With this last remark Schwinger, of course, was recapitulating his own highly successful record as a mentor of successful students, but also revealed his elitist view of physics, as most famously put in his putdown of Feynman, who 'brought computation to the masses.' This attitude also likely had much to do with the limited reception of source theory in the theoretical community.

Let us illustrate the ideas in the simplest context of a scalar (spin zero) particle of mass $m$. We define a weak source $K$ in terms of its effectiveness in producing a particle in the invariant momentum element

$$d\omega_p = \frac{(dp)}{(2\pi)^3}\frac{1}{2p^0}, \quad p^0 = +\sqrt{\mathbf{p}^2 + m^2}. \tag{13.8}$$

That is, the creation and annihilation amplitudes are given by

$$\langle 1_p | 0_- \rangle^K = \sqrt{d\omega_p}\, iK(p), \tag{13.9}$$

$$\langle 0_+ | 1_p \rangle^K = \sqrt{d\omega_p}\, iK(-p), \tag{13.10}$$

where $|0_-\rangle$ represents the vacuum state before the source has acted, and $|0_+\rangle$ the vacuum after the source has acted. A one-particle state of momentum $p$ is represented by $|1_p\rangle$. Now Schwinger started to draw diagrams!* A causal arrangement of sources, in which a localized source $K_2$ acts to produce a particle, which is subsequently detected by a later acting source $K_1$, is shown in Fig. 13.1. Because there can be but one source function—'the unity of the source'—the amplitude that starting with a no-particle state we end with a no-particle state, the vacuum persistence amplitude, is given for a weak source by

$$\langle 0_+ | 0_- \rangle^K = 1 + \frac{i}{2}\int (dx)(dx')K(x)\Delta_+(x - x')K(x'), \tag{13.11}$$

---

* In his rather unpleasant summary talk at the above-mentioned 1967 Rochester conference, Gunnar Källén commented: 'Lagrangian field theory was, shall we say, considerably improved twenty years ago through the work of Tomonaga, Feynman, Schwinger and many others. Twenty years ago we had what appeared to be two rather different formulations. One was Feynman's space–time approach with diagrams, which no one understood when it was first presented. The other formalism was very much easier to understand, it was Schwinger's approach with operators and fields. I think if someone had told us twenty years ago that in 1967 we would at the same conference hear a talk by Schwinger about a space–time approach to strong interactions with diagrams, and Feynman speaking about operators, commutators, singularities, and so on, at least I would not have believed it. However, that's life.'[17] Schwinger recalled this comment as 'amusing,' but went on to remark that 'he quite misunderstood then that in fact our two starting points had long since been amalgamated, at least from my point of view, in this general Green's function theory. I'm sure afterward we got together, Feynman and I, and had a good laugh. Perhaps exchanged a few points.'[8] For further interaction with Källén, see the footnote on p. 476. We may also recall the unpleasantness with Källén over the attempt to write an article on quantum field theory for the *Handbuch der Physik* in 1957, and the confrontation over dispersion relations, both of which episodes we discussed in Chapter 11.

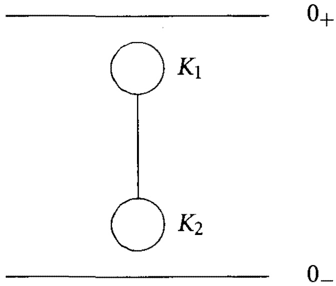


Fig. 13.1 Causal diagram showing the exchange of a single particle between weak sources. In this, and the following figures, time increases vertically.

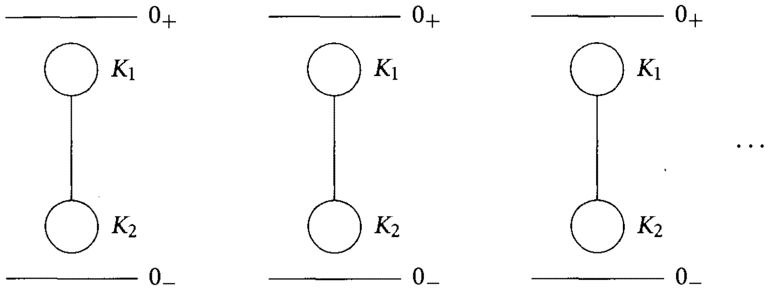


Fig. 13.2 Non-interfering arrangement of pairs of sources.

where the propagation function, under the causal circumstances represented in Fig. 13.1, is

$$x^0 > x'^0: \quad \Delta_+(x - x') = \mathrm{i} \int \mathrm{d}\omega_p \mathrm{e}^{\mathrm{i}p(x-x')}, \tag{13.12}$$

simply a covariant sum of plane waves, and is in general the usual 'propagator' of field theory,

$$\Delta_+(x - x') = \int \frac{(\mathrm{d}p)}{(2\pi)^4} \frac{\mathrm{e}^{\mathrm{i}p(x-x')}}{p^2 + m^2 - \mathrm{i}\epsilon}, \tag{13.13}$$

where the limit $\epsilon \to 0$ through positive values is assumed.

It remains to remove the restriction to weak sources. This can be done by considering a non-interfering arrangement of pairs of sources, 'in such a way that a particle emitted by the emission source of one pair will not be detected by the detection source of another pair' [149]. See Fig. 13.2. The resulting vacuum persistence amplitude is then simply a product of terms like Eqn (13.11), and again the 'unity of the source' allows us to exponentiate:

$$\langle 0_+ | 0_- \rangle^K = \exp\left[\frac{\mathrm{i}}{2} \int (\mathrm{d}x)(\mathrm{d}x') K(x) \Delta_+(x - x') K(x')\right]. \tag{13.14}$$

From this Schwinger showed that it is easy to construct the amplitude for the production, or the detection, of a state consisting of an arbitrary number of particles, where for momentum $p$ there are $n_p$ particles; the appropriate Bose–Einstein combinatorical factors appear, indicating that a scalar source produces, or detects, bosons, the simplest example of the spin-statistics theorem. He proved the consistency of the scheme by verifying that the completeness relation is satisfied. (We recall that it was the completeness theorem that first brought Schwinger to Rabi's attention!)

Extension of these particle exchange ideas to higher spins is straightforward. A helicity-1 photon is produced by a vector source, $J^\mu(x)$, which is conserved,

$$\partial_\mu J^\mu = 0, \tag{13.15}$$

in order to eliminate the scalar mode. Then the vacuum persistence amplitude is given by

$$\langle 0_+|0_-\rangle^J = \exp\left[\frac{\mathrm{i}}{2}\int (\mathrm{d}x)(\mathrm{d}x')J^\mu(x)D_+(x-x')J_\mu(x)\right], \tag{13.16}$$

where the appropriate massless propagation function is

$$D_+(x-x') = \Delta_+(x-x', m^2=0). \tag{13.17}$$

For an electron, the source is taken to be a four-component Dirac spinor $\eta$, and the vacuum persistence amplitude is of a similar form

$$\langle 0_+|0_-\rangle^\eta = \exp\left[\frac{\mathrm{i}}{2}\int (\mathrm{d}x)(\mathrm{d}x')\eta(x)\gamma^0 G_+(x-x')\eta(x')\right], \tag{13.18}$$

where the fermion propagation function, which includes the factor which in the rest frame selects only $\gamma^0 = +1$ [$\mathrm{i}\gamma^0$ is the space reflection matrix], is the Green's function of the Dirac equation,

$$G_+(x-x') = \left(m - \gamma^\mu \frac{1}{\mathrm{i}}\partial_\mu\right)\Delta_+(x-x'). \tag{13.19}$$

Here the $\gamma^\mu$ are the $4\times4$ Dirac matrices satisfying the anticommutation relation

$$\{\gamma^\mu, \gamma^\nu\} = -2g^{\mu\nu}. \tag{13.20}$$

However, it may be noted that, in the Majorana representation being used for the spinors here, $\gamma^0 G_+(x-x')$ is totally antisymmetric in space and spinor indices, so that the vacuum persistence amplitude is identically unity unless the sources $\eta$ are anticommuting numbers (belong to a Grassmann algebra as in Eqn. (13.2)). (Recall that Schwinger had introduced Grassmann numbers in 1962 in [106]; see Chapter 10.) Thus, the spin-statistics connection (discussed in detail on pp. 381–385) emerges automatically from the elementary kinematics considered here.

### Primitive interaction

The next step is to move beyond non-interacting particles. For electrodynamics, the route is perfectly familiar. One recognizes that because an accelerated charge radiates, an electron source must be able to emit not only an electron, but an electron accompanied by a photon, or again, with the proper balance between energy and momentum, a photon source can produce an electron–positron pair. As Schwinger noted: 'But now we want to recognize that dynamically, the creation of a charged particle means, of course, the transfer of that charge from some other particle or group of particles during the collision process, and there is inevitably an acceleration. We create a charged particle, for example, in motion, while the original particles might, for example, be slow moving, such as in the realistic case of beta-decay processes. So, inevitably, the process of photon radiation, which is partially the dynamical meaning of charge, comes into play. It's not meaningful to talk about creating a charged particle and separate that from the process of creating photons, because it is the same dynamical mechanism. One should therefore recognize that part of the process of creating an electron is the creation of a photon. And so one will generalize the idea of source.' [139a] Schwinger then drew the pictures for an electron source shown in Fig. 13.3.

To write the amplitude for such processes requires the introduction of fields. Write the vacuum persistence amplitude for a system of non-interacting electrons and photons as[18]

$$\langle 0_+|0_-\rangle^{\eta J} = \mathrm{e}^{\mathrm{i}W}, \tag{13.21}$$

which is just the product of Eqns (13.16) and (13.18), or

$$W = \frac{1}{2}\int (\mathrm{d}x)(\mathrm{d}x')\eta(x)\gamma^0 G_+(x-x')\eta(x') + \frac{1}{2}\int (\mathrm{d}x)(\mathrm{d}x')J^\mu(x)D_+(x-x')J_\mu(x'). \tag{13.22}$$

Electron and photon fields, $\psi$ and $A_\mu$, respectively, are defined by the response of the functional $W$ to variation in the sources, just as, in electrostatics, the electric field is introduced in terms of the response of the energy of the system

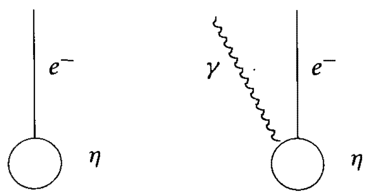


Fig. 13.3 Electron source emitting an electron, or an electron accompanied by a photon.

to the introduction of a test charge. Now, we can write $W$ in the form of an action, which is stationary with respect to variations in the fields:

$$W = \int (\mathrm{d}x) \left[ \psi(x)\gamma^0\eta(x) + A^\mu(x)J_\mu(x) + \mathcal{L}(\psi, A) \right], \tag{13.23}$$

where $\mathcal{L}$ is the Lagrange function of the system,

$$\mathcal{L} = -\frac{1}{2}\psi\gamma^0 \left( \gamma^\mu \frac{1}{\mathrm{i}}\partial_\mu + m \right) \psi - \frac{1}{4}F^{\mu\nu}F_{\mu\nu}, \tag{13.24}$$

and where we have introduced the field strength tensor,

$$F_{\mu\nu} = \partial_\mu A_\nu - \partial_\nu A_\mu. \tag{13.25}$$

Interactions are incorporated by making the so-called gauge-covariant substitution in $W$, that is, by the replacement $\partial_\mu \to \partial_\mu - \mathrm{i}eqA_\mu$. This is the standard field-theoretic way of stating the physical fact that electric charge is conserved—the corresponding symmetry is gauge invariance. The result is the familiar Lagrange function of electrodynamics:

$$\mathcal{L} = -\frac{1}{2}\psi\gamma^0 \left[ \gamma^\mu \left( \frac{1}{\mathrm{i}}\partial_\mu - eqA_\mu \right) + m \right] \psi - \frac{1}{4}F^{\mu\nu}F_{\mu\nu}, \tag{13.26}$$

which implies, in particular, the Dirac equation with interaction,

$$\left[ \gamma^\mu \left( \frac{1}{\mathrm{i}}\partial_\mu - eqA_\mu \right) + m \right] \psi = \eta, \tag{13.27}$$

where $q$ is the antisymmetric $2 \times 2$ charge matrix, with eigenvalues $\pm 1$, corresponding to the charges on the positron and the electron, respectively.

This Dirac equation is equivalent to the integral equation

$$\psi = G_+\eta + G_+\gamma eqA\psi, \tag{13.28}$$

in which space–time coordinates are regarded as matrix indices. From this, and the corresponding integral equation for $A$, we can infer a sequence of increasingly elaborate 'interaction skeletons,'

$$\begin{aligned} W &= \frac{1}{2}\int (\mathrm{d}x)[J_\mu A^\mu + \eta\gamma^0\psi] + \frac{1}{2}\int (\mathrm{d}x)\psi\gamma^0 eq\gamma A\psi \\ &+ \frac{1}{2}\int (\mathrm{d}x)(\mathrm{d}x')\frac{1}{2}(\psi\gamma^0 eq\gamma^\mu\psi)(x)D_+(x-x')\frac{1}{2}(\psi\gamma^0 eq\gamma_\mu\psi)(x') \\ &+ \frac{1}{2}\int (\mathrm{d}x)(\mathrm{d}x')\psi(x)\gamma^0 eq\gamma A(x)G_+(x-x')eq\gamma A(x')\psi(x') + \dots. \end{aligned} \tag{13.29}$$

The first term describes the non-interacting system, the second is the primitive interaction, the third term describes $e^-e^-$ or $e^-e^+$ scattering, and the fourth term describes electron–photon scattering or pair annihilation. For example, the last term represents Compton scattering, in which an electron and photon interact at a point $x$ to give rise to a virtual electron, which then propagates through space–time from the point $x$ to the point $x'$, at which point another real electron and photon are created. (All possible orderings of these events may take place.) 'It should be emphasized that the iterated solution is a classification of processes in terms of increasing degree of complexity. It is not a perturbation expansion. The physical electron mass $m$, and the physical electron charge $e$, which are identified originally under specific physical circumstances, will never change their significance when the class of phenomena under examination is enlarged.' [149]

### Source theory calculation of the anomalous magnetic moment

In its 'purest' or at least original form, such source theory ideas were used to generate amplitudes in 'causal' form; that is, in which real particles were exchanged between real or virtual* sources separated in time. From this one could deduce immediately ('space–time extrapolation') the full amplitude in spectral form, that is, in terms of what most people would refer to as a 'dispersion relation.' Such a direct generation of amplitudes was extremely powerful, and allowed a completely finite calculation to be carried out.

Here is an example, taken from Schwinger's 1969 lectures at Harvard. He started by drawing a causal diagram, showing the exchange of an electron–positron pair between an extended (virtual) photon source $J$ and two electron sources $\eta$, but where the charged particles undergo a scattering process before being detected; see Fig. 13.4. According to the ideas discussed above, the overall process is described by the amplitude

$$\begin{aligned}
&\exp\left[\mathrm{i}\int \eta_1\gamma^0 G_+\eta_2\right]\\
&\quad\to -\frac{1}{2}\int \eta_1(x)\gamma^0 G_+(x-x')\eta_2(x')\eta_2(y')\gamma^0 G_+(y'-y)\eta_1(y)\\
&\quad= -\frac{1}{2}\int \mathrm{tr}[G_+(x-x')\mathrm{i}\eta_2(x')\eta_2(y')\gamma^0 G_+(y'-y)\mathrm{i}\eta_1(y)\eta_1(x)\gamma^0],
\end{aligned} \tag{13.30}$$

* A virtual particle is one which does not satisfy the correct energy–momentum balance: $p^2 + m^2 \neq 0$ or $p^0 \neq \sqrt{\mathbf{p}^2 + m^2}$.

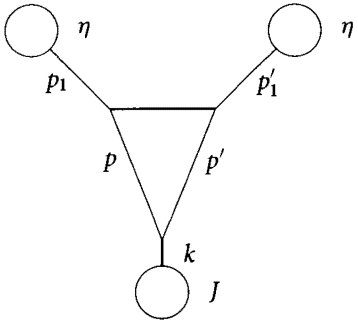


Fig. 13.4 Causal diagram representing the exchange of an electron–positron pair between an extended photon source and a scattering act. Here the thin lines represent real electrons, while the thick lines represent virtual photons. Although this is a space–time diagram, momentum labels corresponding to those in Eqns (13.34) and (13.35) are shown.

where we have picked out the quadratic term in the expansion of the exponential. Here, for brevity, the integration elements have been omitted, and the numbers on the sources are causal labels, 2 being earlier and 1 later. Now the product of earlier-acting electron sources is effectively replaced by the action of the photon source, through its field $A$, as inferred from the interaction term in Eqn. (13.26),

$$\mathrm{i}\eta_2(x')\eta_2(y')\gamma^0|_{\text{eff}} = eq\gamma A(x')\delta(x'-y'), \tag{13.31}$$

and the product of later-acting sources is replaced by the scattering process followed by detection sources, again represented by fields, as inferred from the third term in Eqn. (13.29),

$$\mathrm{i}\eta_1(y)\eta_1(x)\gamma^0|_{\text{eff}} = \gamma^\mu eq\psi_1(y)D_+(y-x)\psi_1(x)\gamma^0\gamma_\mu eq. \tag{13.32}$$

The electrons, before and after the scattering act, are regarded as being real particles, so the corresponding propagators have causal form, from Eqns. (13.12) and (13.19),

$$x^0 > x'^0: \quad G_+(x-x') = \mathrm{i}\int \mathrm{d}\omega_p \mathrm{e}^{\mathrm{i}p(x-x')}(m-\gamma p). \tag{13.33}$$

In this way Schwinger immediately arrived at the form for the vacuum amplitude for this process,

$$\begin{aligned}\text{V.A.} = \frac{1}{2}\int &\mathrm{d}\omega_k \mathrm{d}M^2 \mathrm{d}\omega_{p_1}\mathrm{d}\omega_{p_1'}(2\pi)^4\delta(p_1+p_1'-k)\\ &\times A_\mu(k)\eta_1(-p_1)\gamma^0(m-\gamma p_1)I_\mu(-m-\gamma p_1')\eta_1(-p_1'),\end{aligned} \tag{13.34}$$

where he wrote the momentum element in terms of a spectral mass $M$ as $(\mathrm{d}k) = \mathrm{d}\omega_k \mathrm{d}M^2/2\pi$, $M^2 = -k^2$, and

$$I_\mu = e^2 \int \mathrm{d}\omega_p \mathrm{d}\omega_{p'} (2\pi)^3 \delta(p + p' - k) \\ \times \gamma^\nu (m - \gamma p) eq \gamma_\mu (-m - \gamma p') \gamma_\nu \frac{1}{(p_1 - p)^2}. \tag{13.35}$$

By virtue of the Dirac projection operators in (13.34), $I^\mu$ must be of the form

$$I^\mu = \gamma^\mu I_1(M) + \mathrm{i}\sigma^{\mu\nu} k_\nu I_2(M), \tag{13.36}$$

where $M$ is the mass of the exchanged excitation (electron–positron pair). These two terms are the electric and magnetic moment parts, respectively. We look at the magnetic moment part only, and write it back in coordinate space,

$$\text{V.A.} = \mathrm{i} \int \mathrm{d}M^2 (\mathrm{d}x)(\mathrm{d}x') \frac{1}{2} \bar{\psi}_1(x) \gamma^0 \sigma^{\mu\nu} eq \psi_1(x) \\ \times \mathrm{i} \int \mathrm{d}\omega_k \mathrm{e}^{\mathrm{i}k(x-x')} \frac{1}{2} F_{\mu\nu}(x') I_2(M), \tag{13.37}$$

where we have introduced the field strength according to

$$\mathrm{i}[k^\mu A^\nu(k) - k^\nu A^\mu(k)] = F^{\mu\nu}(k) = \int (\mathrm{d}x') \mathrm{e}^{-\mathrm{i}kx'} F^{\mu\nu}(x'). \tag{13.38}$$

We now adopt this form (13.37) as generally valid, by dropping the causal labels and using the general space–time form for the propagation function describing the exchange excitation (Schwinger called this process *space–time extrapolation*) (cf. Eqns (13.12) and (13.13)),

$$\mathrm{i} \int \mathrm{d}\omega_k \mathrm{e}^{\mathrm{i}k(x-x')} \dot{\to} \Delta_+(x - x') = \int \frac{(\mathrm{d}k)}{(2\pi)^4} \frac{\mathrm{e}^{\mathrm{i}k(x-x')}}{k^2 + M^2 - \mathrm{i}\epsilon}. \tag{13.39}$$

Now we return to the specific dynamics in Eqn. (13.35). By virtue of the projection factors in Eqn. (13.34), we can simplify the Dirac matrix structure in Eqn. (13.35) into terms that have only one Dirac $\gamma$ matrix. No integration over momentum is in fact necessary. In terms of the phase-space integral

$$\int \mathrm{d}\omega_p \mathrm{d}\omega_{p'} (2\pi)^3 \delta(p + p' - k) = \frac{1}{(4\pi)^2} \sqrt{1 - \left(\frac{2m}{M}\right)^2}, \tag{13.40}$$

we only need to evaluate averages, such as

$$\left\langle \frac{(p - p_1)^\mu}{(p - p_1)^2} \right\rangle = -\frac{1}{M^2 - 4m^2} (p_1 - p_1')^\mu, \tag{13.41}$$

where the coefficient has been determined by multiplying the left-hand side by $p_{1\mu}$. Finally, we use the Dirac equation once again to isolate the spin term:

$$(p_1 - p_1')^\mu \to i\sigma^{\mu\nu}k_\nu. \tag{13.42}$$

Thus we find very easily

$$I_2 = \frac{\alpha}{2\pi}\frac{m}{M^2}\frac{1}{\sqrt{1-4m^2/M^2}}, \tag{13.43}$$

which gives the anomalous magnetic moment, that is, the magnetic form factor at $k^2 = 0$, from Eqns. (13.37) and (13.39)

$$\mu' = \frac{\alpha}{2\pi}\int_{(2m)^2}^{\infty}\frac{dM^2}{M^2}\frac{2m^2}{M^2}\frac{1}{\sqrt{1-4m^2/M^2}} = \frac{\alpha}{2\pi}. \tag{13.44}$$

This seems to be an extremely transparent derivation of the anomalous magnetic moment of the electron, first calculated, we recall, by Schwinger in 1947, but now with no 'distracting remarks' about infinite quantities. With this calculation, Schwinger concluded his field theory course in 1969, leaving shortly thereafter for a one-semester sabbatical in Tokyo, where he began to write the second volume of *Particles, sources, and fields.*

## A sabbatical in Japan

The Schwingers went to Japan in January 1970 and stayed until the fall.* This was their second visit to Japan. Before they left, Julian had taken some lessons in Japanese from the wife of a Japanese broadcasting representative, who had offered their Japanese-style house, but they had not taken it because they were assured that it was too small. When they arrived in Tokyo they stayed at the International House for a while, looking for a place to live. Clarice went out to look for apartments. They were hideously expensive and ugly. They were also in areas largely inhabited by foreigners. Then they looked at the little Japanese house which had still been saved for them; they found it enchanting. It was a combination Western and Japanese house in an entirely Japanese area. After they moved in, one of the next-door neighbors taught Clarice *katakana*, which is the Japanese way of writing foreign words. Clarice mastered this to the extent that when she brought film to the shop to be developed, she could see that the shopkeeper had written the word *geijin*, which means foreigner. Clarice said no, no, and made him spell her name. The parents of the young couple who had offered them the house lived immediately adjacent to them, across a

* He had applied for, and presumably received, a Guggenheim Fellowship to fund this trip.[3]

little bridge. Clarice taught the mother English. After breakfast Clarice had an appointment to teach her English at 11:00 and she would give Clarice a sweet. She found it much too sweet, and she did not really want sweets just before lunch. But she eventually learned to like them, enough so that before they left Japan, coming out of a bank, she saw two shops across the street, a Japanese sweet shop and a Western bakery, and, without thinking, Clarice went to the Japanese shop. Then she knew she had arrived, when she realized what she had done.[2] She also offered her Japanese friends a Western-style tea, but they found her cookies much too sweet because of their butter content.

The neighbor, Mrs Odate, who taught Clarice *katakana*, taught English privately. Her father had been a *Noh* dancer, the very traditional and unique Japanese theatrical form. With her Clarice went to a *Noh* performance, which goes on, literally, all day. One arrives at the theater in the morning and does not come home until late at night. Clarice and Mrs Odate were both worried as to how she was going to take it. But although Clarice might not quite have gotten all she might have, they did not come home until 5:00. Clarice really was fascinated and had a marvelous time. In exchange, she took Mrs Odate to see *Roman Holiday* with Gregory Peck, and she just 'absolutely swooned.' Afterwards Julian was working and Clarice was reading in his study. From the outside one could see into Julian's study. When Clarice leaned over to kiss Julian, Mrs Odate observed them and said, 'So American, just like Gregory Peck.'[2]

One memorable side trip was to Hokkaido. The university sent three uninitiated innocent graduate students to take care of them and they had a very hard time. They were very unsophisticated; they knew nothing about traveling or making reservations. They knew Japanese and some English, but none of them knew this unknown territory. Clarice suggested that perhaps it would be a good idea to make a reservation at the hotel. The students said they would do it after they got there. They arrived in Hokkaido in pouring rain and, of course, the hotel did not have a place for them. The next hotel did have a room, but the ceiling leaked. While they had supper, the rain was dripping in. The students stayed upstairs and the Schwingers slept around the puddle. It was a fascinating time. It was unfortunate because it took so long to get things done, it took more than 10 days to do things that should have taken just a week, and it was hard for the students to be with strange people and to speak English, to be traveling, feel responsible, make conversation, and to arrange for the restaurants. But they had a good time.[2]

On this trip the Schwingers met Tomonaga; in fact that was the first time they really got to know him. Julian was very moved. Years later, after Tomonaga died, he was asked to come back and give a memorial lecture for Tomonaga. The more Julian read about him the more moved he became, and he almost wept when he gave that talk,[2] which we will describe in Chapter 16. We will also give another perspective on this Japanese trip in that chapter.

Afterwards, on the way home from Japan, they wanted to visit Cambodia, to see Angkor Wat. But the authorities were allowing no visitors into Cambodia then. So they had to decide where to go to escape the heat and humidity of Tokyo. After studying the temperatures and humidities of all the places they could go to, Julian settled on Tahiti. They had to come all the way back to Honolulu, and then fly to Tahiti. Clarice had a good time there, but Julian was sick when they left Tokyo, with some unknown fever.[2]

### Other travels

In June 1967 Paul and Ann Martin gave the Schwingers a surprise anniversary party. It was at the house of Bertram and Ruth Malenka, who lived nearby in Belmont, and the Stanley Desers and the Richard Arnowitts attended. Clarice did not enjoy the party: she did not appreciate expecting one thing and being confronted with another.[2] That summer they went to the World's Fair in Montreal with the Malenkas and the Kurt Gottfrieds (where Julian first tried Urkell Pilsner in the Czech Pavilion)[19] and visited the United Nations in New York.[2]

The Schwingers visited Trieste again in 1968 for the symposium on contemporary physics organized by Salam. In Clarice's eyes, this was chiefly memorable for meeting Dirac again. He would sit down with Clarice and tell her funny stories, slightly ribald stories. They were funny and Clarice would laugh and that was what their friendship was based on. They never really talked.[2] In Trieste they stayed at the Duino Castle of Prince Raimondo della Torre e Tasso. They had received a letter from Paolo Budini saying that the prince was willing for them to stay at his castle but he would not feed them. At first the Schwingers did not want to stay there under those conditions but finally were persuaded. The Prince changed his mind too, because the guests were all so charming, and he did indeed provide them with meals.[2] Dirac, on the other hand, refused to stay in the castle and his wife was unhappy, because everybody else was at the castle but them.[2] The sleeping arrangements were amusing: they gave the Schwingers two bedrooms, a small room and a big room, and they assumed that Clarice would be in the large bedroom with Julian, but they never slept in the same bed, so Clarice slept in the single bed in the small room. In the morning the maid knocked on the door to bring in the breakfast and there was Julian, resplendent in this satin brocade bed all by himself. Of course, the Schwingers' hours were a bit upsetting to the staff. They would give them toast, but the Schwingers wanted Italian bread, so they made a deal. If they would get up by 9:30 in the morning, they could have bread instead of toast. And so they got their breakfast at 9:30 and managed to stagger out by 10:30.[2] After Italy and Yugoslavia, the Schwingers went to the meeting of Nobel Laureates in Lindau.

### The role of source theory

The new source theory was supposed to supersede field theory, much as Schwinger's successive covariant formulations of quantum electrodynamics had replaced the earlier schemes. In fact, the revolution was to be more profound, because there were no divergences, and no renormalization. 'The concept of renormalization is simply foreign to this phenomenological theory. In source theory, we begin by hypothesis with the description of the actual particles, while renormalization is a field theory concept in which you begin with the more fundamental operators, which are then modified by dynamics. I emphasize that there never can be divergences in a phenomenological theory. What one means by that is that one is recognizing that all further phenomena are consequences of one phenomenological constant, namely the basic charge unit, which describes the probability of emitting a photon relative to the emission of an electron. When one says that there are no divergences one means that it is not necessary to introduce any new phenomenological constant. All further processes as computed in terms of this primitive interaction automatically emerge to be finite, and in agreement with those which historically had evolved much earlier.'[139a]

The problem with conventional field theory is that it makes an implicit hypothesis that the physics is known down to zero distance. 'We have presented a point of view which covers everything that was good about ordinary field theory and gives one the power of absolute calculation without, however, making use of what is, for practical purposes, the irrelevant hypothesis of field theory, namely, that a space-time description is possible down to arbitrarily small distances. This hypothesis is the reason why practical application of the field theory is so difficult. You are supposing that you can begin by describing everything down to arbitrarily small distances, you make use of perturbation theory, which is ill-suited to that fact, you end up with divergences, and you must then remove the reference to the irrelevant parts of the description and recover what is actually physically interesting by expressing it in terms of the actual physical parameters, namely, the observed charges and masses, etc. This roundabout procedure is simply avoided here; we'll begin with the actual physical particles and proceed directly to the physical phenomena. I hope to make it clear that the source attitude is a perfectly general one, which unifies all the general attitudes that have been brought to bear in different parts of physics. There is the fact that it reproduces electrodynamics. It is not incompatible with field theory; it is simply more efficient than field theory. You may regard it as the calculational tool of field theory, if you like, but it is more general. If, indeed, the hypothesis of description down to arbitrarily small distances fails, then field theory in the strict sense will no longer be valid, but this phenomenological attitude will still be applicable.' [139a]

History resumed

The first source theory paper [135] already included particles of all spins through the use of multispinors. It also included the 'Euclidean postulate,' that the theory be transformable into Euclidean space so that the original time axis cannot be identified. This requirement necessitates the observed fact that all fermions carry a charge-like attribute. For example, neutrinos carry lepton number, neutrons have baryon number, etc. In 1967 'Sources and electrodynamics' [142] was published, which put QED into the new framework. This included a discussion of two-particle Green's functions and bound states, with a note making 'a modest contribution to the history of science' with a 'time-ordered list of papers' in which what is now universally called the Bethe–Salpeter equations were derived. As we have noted, the authors in order are Yoichiro Nambu; Schwinger; Murray Gell-Mann and Francis Low; and Edwin Salpeter and Hans Bethe.[20] The following year, Schwinger treated gravitons, and gave *his* demonstration that full general relativity is essentially a consequence of assuming that the mediator of the gravitational force is a massless helicity-2 particle [146, 162, 163, 177]. We will discuss this in detail in the next chapter. The first book treatment of source theory, based on the Brandeis lectures as transcribed by his student Tung-mow Yan, appeared in 1969 [149]. There was considerable excitement associated with Schwinger's source theory treatment of magnetic charge [147], particularly his speculative dyon model of matter, which he published in *Science* in 1969 [150]. (His philosophy here was summed up in his quotation from Faraday: 'Nothing is too wonderful to be true, if it be consistent with the laws of nature, and in such things as these, experiment is the best test of such consistency,' the initial words of which, appropriately enough, were emblazoned on the walls of the old physics building at UCLA, Kinsey Hall, to which department he would shortly repair.) Again, we will discuss magnetic charge and dyons at length in Chapter 14.

Three other books came out in as many years: *Discontinuity in waveguides* (1968) [148], based on David Saxon's notes recording a small portion of Schwinger's wartime radar work, discussed in Chapter 4; *Quantum kinematics and dynamics* (1970) [152], an unfinished textbook on quantum mechanics, which we discussed in Chapter 10; and *Particles, sources, and fields*, Vol. 1 (1970) [153]. The latter was intended to be a comprehensive treatment of source theory, based on the motto 'if you can't join 'em, beat 'em.' Harold, acronymically the 'hypothetical alert reader of limitless dedication,' makes his appearance, and unlike a real student, is allowed to interrupt, particularly when he has 'an historical gleam in his eye.'* We see here Schwinger's continuing homage to his older brother. This book was dedicated to the C.G.S. system, a reference to his devoted wife Clarice.† In the preface he pleads for students to read his book, for minds not 'warped . . . past the elastic limit' by the orthodox methodologies. As noted above, Schwinger started writing the second volume of this book during a six-month sabbatical in Tokyo in 1970; on his return, he announced to his 12 or so graduate students that he was leaving Harvard in February 1971 for UCLA. This was met with considerable consternation, but the three senior students, Kimball Milton, Lester DeRaad, Jr, and Wu-yang Tsai, were soon told that he had arranged with UCLA to bring them along as postdocs. Little did they guess that their affiliation with UCLA would last nearly a decade!

### Teaching continues

While busy reformulating field theory in this new phenomenological guise, Schwinger continued his brilliant teaching at Harvard and elsewhere, and not always about source theory. In 1968–69, for example, he once again taught Physics 251, Quantum Mechanics, continuing his measurement algebra approach begun at least as early as 1952. This course was taught in the large lecture hall in Jefferson Lab (a building constructed without iron fasteners, to facilitate magnetic measurements, but then, inexplicably, finished in Harvard style with beautiful bricks containing an abundance of magnetic iron oxide!), which, as usual for Schwinger's lectures, was well-filled, largely with undergraduate students. As the term wore on, the lectures gradually got longer, from an hour, to an hour and a half, an hour and three-quarters, . . . . The undergraduates got more and more restless. Their houses (Harvard's undergraduate residence halls) stopped serving lunch at 2:00, and there was now barely time to make it back. One day, Schwinger was about to conclude a beautiful development, and at about 1:50, requested, without anticipating a reply, 'if I can just have a few minutes more.' The students let out the canonical Harvard hiss, and Schwinger was taken aback, and furious. He slammed down his chalk, and marched to the nearest exit. Unfortunately, the door was locked, so he had to slink out by a side door. But, that term he never again lectured past 1:30.

As throughout his career, Schwinger's biggest educational impact was on his graduate students. He always had a large number of graduate students at Harvard, and the last few years there were no exception. Indeed, in the late 1960s there was a general crisis in the particle physics theory community, and

* When Harold is introduced, halfway through the book, S., the author, momentarily makes a Galilean confusion of Harold with Sagredo.

† The manuscript for the preface was written on a Chez Dreyfus menu, and originally included the dedication 'to C.G.S. and her system.'[3]

although dispersion relations and current algebra had had some success, most of the theory faculty at Harvard had few ideas for research students. It was unwise for a student to work with an assistant professor, such as Curtis Callan, for he would never be promoted at Harvard, so many students sought advisors outside the department, in the Division of Applied Physics (later Sciences) or at MIT. Only Schwinger welcomed students, and had an abundance of problems on which to work.

We have noted in earlier chapters that typically Schwinger's students saw him rarely. This was to some extent no longer the case after 1965. The pattern was much the same: Every Wednesday afternoon he was available for consultation; students would sign a list at 9:00 in the morning when his secretary arrived (not an easy task, especially if one had stayed up till the wee hours completing a calculation), and then at 3:00 or so in the afternoon, after Schwinger returned from lunch, he would see students in the order on the list. Of course, this might well mean that if you were number 10 or 12 you wouldn't get in that week. But when at last you were admitted, Schwinger gave his undivided attention, and students were never rushed. Of course, if the telephone rang, it was ignored: Schwinger never answered the telephone. The trick was always to do enough in the week between audiences to be able to know more about the problem than Schwinger could see in a few instants. But his interactions with students were invariably kind and attentive, and he was eager to share his insights. One could only accuse him of being too kind—he was rarely if ever known to refuse to take on a student, although the selection effect at Harvard generally kept all but the brightest away. We will discuss Schwinger's relations with his students more fully in Chapter 16.

## Weinberg and effective Lagrangians

As we have remarked, it was largely the difficulty of putting the phenomena of strong interaction physics into the context of field theory that led Schwinger to the development of source theory, which he felt was much closer to the experience of particle physics. Recall that, in his view, (operator) quantum field theory dealt with fields as the fundamental entities, and the particle content of the theory only emerged through the process of renormalization. Thus, direct contact with the burgeoning experimental data, the multitudes of new hadronic resonances, and the emerging regularities in their interactions, was remote. In the meantime current algebra had had great success in describing the physics of pions, the most important manifestation of the strong force between nucleons.

Even in his Nobel lecture, Schwinger alluded to the necessity of constructing a phenomenological theory, and now with his source theory 'revolution' he had done just that. Schwinger was not alone in believing that a phenomenological

Lagrangian field theory could reproduce the successes of current algebra, and lead to a deeper understanding. As he was developing source theory in the context of quantum electrodynamics, he was simultaneously talking to Steven Weinberg about such effective Lagrangian descriptions. Weinberg described this interaction in his contribution to Schwinger's 60th birthday *Festschrift*: 'Julian Schwinger's ideas have strongly influenced my understanding of phenomenological Lagrangians since 1966, when I made a visit to Harvard. At that time, I was trying to construct a phenomenological Lagrangian which would allow one to obtain the predictions of current algebra for soft pion matrix elements with less work, and more insight into possible corrections. It was necessary to arrange that the pion couplings in the Lagrangian would all be derivative interactions, to suppress the incalculable graphs in which soft pions would be emitted from internal lines of a hard-particle process. The mathematical approach I followed at first was quite clumsy; I started with the old $\sigma$-model [introduced by Schwinger in [82]], in which the pion is in a chiral quartet with a 0+ isoscalar $\sigma$; then performed a space–time dependent chiral rotation which transformed $\{\pi, \sigma\}$ everywhere into $\{0, \sigma'\}$ with $\sigma' \equiv (\sigma^2 + \pi^2)^{1/2}$; and then re-introduced the pion field as the chiral rotation "angle." The Lagrangian obtained in this way had a complicated and unfamiliar non-linear structure, but it did have the desired property of derivative coupling, because any space–time independent part of the rotation "angle" would correspond to a symmetry of the theory, and so would not contribute to the Lagrangian.

'Schwinger suggested to me that one might be able to construct a suitable phenomenological Lagrangian directly, by introducing a pion field which from the beginning would have the non-linear transformation property of chiral rotation angles, and then just obeying the dictates of chiral symmetry for such a pion field. [This approach was followed by Schwinger in [137].] Following this suggestion, I worked out a general theory of non-linear realizations of chiral $SU(2) \times SU(2)$, which was soon after generalized to arbitrary groups in elegant papers of Callan, Coleman, Wess, and Zumino, and has since been applied by many authors. The importance of the approach suggested by Schwinger has been not only that it saves the work involved in the transition from an ordinary linear representation like $\{\pi, \sigma\}$ to a non-linear realization, but more important, that it makes clear that the interaction of other hadrons with soft pions does not in any way depend on the chiral transformation properties of whatever fields are associated with these hadrons, but only on their isospin.

'In the decade since 1967, Schwinger's ideas have evolved into what he calls "source theory." I have been pretty much out of touch with this work, mostly because of [my] involvement with other lines of research, but perhaps also because I found Schwinger's conceptual framework unfamiliar. Recently, several problems have led me to think again about the use of phenomenological

Lagrangians, and I find that my ideas have shifted somewhat, to a point of view that seems to me to be now not too different from the point of view of source theory.'[21]

Not only current algebra, but dispersion relations and S-matrix theory were much in the air in the mid-1960s. Recently, Weinberg has described how S-matrix theory failed, and how effective field theory rescued it: 'One problem with the S-matrix program was in formulating what is meant by the analyticity of the S-matrix. What precisely are the analytic properties of a multi-particle S-matrix element? I don't think anyone ever knew. I certainly didn't know, so even though I was at Berkeley I never got too enthusiastic about the details of the program, although I thought it was a lovely idea in principle. Eventually the S-matrix program had to retreat... to a sort of mix of field theory and S-matrix theory. Feynman rules were used to find the singularities in the S-matrix, and then they were thrown away, and the analytic structure of the S-matrix with these singularities, together with unitarity and Lorentz invariance, was used to do calculations.

'Unfortunately to use these assumptions it was necessary to make uncontrolled approximations, such as the strip approximation, whose mention will bring tears to the eyes of those of us who are old enough to remember it. By the mid-1960s it was clear that S-matrix theory had failed in dealing with the one problem it had tried hardest to solve, that of pion–pion scattering. The strip approximation rested on the assumption that double dispersion relations are dominated by regions of the Mandelstam diagram near the fringes of the physical region, which would only make sense if $\pi$–$\pi$ scattering is strong at low energy, and these calculations predicted that $\pi$–$\pi$ scattering is indeed strong at low energy, which was at least consistent, but it was then discovered that $\pi$–$\pi$ scattering is *not* strong at low energy. Current algebra came along at just that time, and was used to predict not only that low energy $\pi$–$\pi$ scattering is not strong, but also successfully predicted the values of the $\pi$–$\pi$ scattering lengths.[22] From a practical point of view, this was the greatest defeat of S-matrix theory. The irony here is that the S-matrix philosophy is not that far from the modern philosophy of effective field theories, that what you should do is just write down the most general S-matrix that satisfies basic principles. But the practical way to implement S-matrix theory is to use an effective quantum field theory—instead of deriving analyticity properties from Feynman diagrams, we use the Feynman diagrams themselves. So here's another answer to the question of what quantum field theory is: it is S-matrix theory, made practical.'[23]

## Schwinger's chiral symmetry papers

Schwinger submitted eight papers on effective Lagrangians in hadronic physics in 1967. In a 10-day period in April, less than two months after submitting

'Sources and electrodynamics' [142], he submitted three letters on the subject. In the first, 'Chiral dynamics' [137], he began by acknowledging his debt to Weinberg: 'This note was stimulated by some recent work of Weinberg[24]. He has shown how the results of current-algebra can be easily reproduced by certain calculational rules used in conjunction with an appropriate Lagrange function. Current-algebra is still considered primary, however. I propose to further this simplification and clarification by eliminating all reference to current-algebra. The non-operator method that replaces it is the phenomenological source theory now under development. For our present purposes, however, it suffices to think of a numerical effective Lagrange function, the coupling terms of which are directly applicable to the corresponding processes.' [137]

He went on to provide alternative possibilities for $\pi$–$\pi$ scattering lengths, and to 'base the weak interaction theory for pions and nucleons on the assumption that leptons are coupled to the charged components' of the chiral current inferred from the effective Lagrangian. This subsumed the Feynman–Gell-Mann connection between pion and nucleon beta-decay couplings, the Goldberger–Treiman relation, and the Adler–Weisberger relation. He extended the analysis to vector mesons, and deduced Adler's mass formula $m_{A_1} = \sqrt{2}m_\rho$, relating the mass of the $A_1$ resonance [now called the $a_1(1260)$] to that of the $\rho$ meson.

The second paper, 'Mass empirics' [138], was a very brief note about the identity of fractional mass splitting between the baryon octet and the baryon decuplet. The third, 'Partial symmetry' [139], combined internal and spin transformations [$U(4)$ instead of the usual $SU(6)$] to derive a striking value for the ratio of axial-vector to vector couplings for the nucleon,

$$-\frac{G_A}{G_V} = \frac{5}{3\sqrt{2}} = 1.18, \tag{13.45}$$

then completely consistent with experiment.* Other interesting results, including quite good estimates for the magnetic moments of the proton and neutron, were found.

* In 1965 the Schwingers visited Källén after Stockholm and 'I thought we were quite friendly.' Then, in 1967 at Rochester, and at the Solvay Conference in Brussels, 'Källén came and listened to the lectures; I thought he listened. He must have gotten the idea I was trying to get across and then later he rose and attacked the whole idea. I thought that was treachery of a high order.' Källén 'got up to say, "Well I've just received word that this has been remeasured and the value is 1.24 so Schwinger with his value of 1.18 is absolutely wrong." You know I had quite a history of being confronted with experiment. Everybody loudly proclaimed as being right that later turned out to be wrong. So that was not a very important remark to me. But Källén was being unnecessarily offensive, really.'[8] However, the currently accepted value is 1.27.[25]

In the summer of 1967 Schwinger wrote 'Gauge fields, sources, and electromagnetic masses' [144], dealing with vector-meson–photon mixing. To give a flavor of the argument, let us follow Schwinger in describing the $\rho$–$\pi$ system by the Lagrangian

$$\mathcal{L} = -\frac{1}{2}(D_\mu \pi)^2 - \frac{1}{2}m_\pi^2 \pi^2 - \frac{1}{4}(\rho_{\mu\nu})^2 - \frac{1}{2}m_\rho^2(\rho_\mu)^2, \tag{13.46}$$

where, in terms of $SU(2)$ isotopic vectors, the covariant derivative is

$$D_\mu = \partial_\mu + g\rho_\mu \times, \tag{13.47}$$

and the rho field strength is

$$\rho_{\mu\nu} = \partial_\mu \rho_\nu - \partial_\nu \rho_\mu + g\rho_\mu \times \rho_\nu. \tag{13.48}$$

If the rho mass term were not present, this Lagrangian would be invariant under the infinitesimal isotopic gauge transformation

$$\delta\pi = -\,\delta\omega \times \pi, \tag{13.49}$$

$$\delta\rho_\mu = -\,\delta\omega \times \rho_\mu + \frac{1}{g}\partial_\mu \delta\omega. \tag{13.50}$$

When $\delta\omega$ has only a component along the third isotopic axis, the gauge transformation of the neutral $\rho$ becomes Abelian,

$$\delta g\rho_{\mu 3} = \partial_\mu \delta\omega, \tag{13.51}$$

so the corresponding mass term can be made invariant if we add the compensating effect of the electromagnetic gauge transformation

$$\delta eA_\mu = \partial_\mu \delta\omega, \tag{13.52}$$

provided the mass term is suitably generalized,

$$-\frac{1}{2}m_\rho^2[\rho_{\mu 3} - (e/g)A_\mu]^2. \tag{13.53}$$

This and its generalization gives the coupling of photons to the $1^-$ mesons, $\rho^0$, $\omega$, and $\phi$—what is usually referred to as vector-meson dominance,[26] without any uneasiness about gauge invariance—which was further the subject of 'Photons, mesons, and form factors' [140] but there the necessity for including a form factor is seen. These ideas were extended to 'Radiative corrections in $\beta$ decay' [141], in particular to the divergent scale factor of James Bjorken,[27] which is discussed in a rather readable way in the Rochester lecture [139a]. The last paper of the series was 'Chiral transformations' [145] which once again treated $\pi$–$\pi$ scattering, and in particular predicted an energy asymmetry in the strong decay $\eta' \to \eta + 2\pi$, for which there was then weak experimental evidence.[28] That asymmetry seems now well established,[29] although smaller than Schwinger first predicted.

## Conclusion

Robert Finkelstein has offered a perceptive discussion of Schwinger's source theory program: 'Source theory represented Julian's effort to replace the prevailing operator field theory to which he had contributed so richly and so fundamentally by a philosophy and methodology that eliminated all infinite quantities. He did in fact succeed in constructing an infinity-free formalism that was also friendly to the introduction of new experimental information and new theoretical ideas. Moreover it was not simply a program: he and his UCLA source group, Kim Milton, Lester DeRaad, and Wu-yang Tsai, made many applications to high-energy physics and showed it was a very effective calculational tool. Since source theory, like every successful physical theory, necessarily shared features with the formalism it was replacing, some felt it was nothing really new—but of course it was.

'In comparing operator field theory with source theory Julian revealed his political orientation when he described operator field theory as a trickle down theory (after a failed economic theory)—since it descends from implicit assumptions about unknown phenomena at inaccessible and very high energies to make predictions at lower energies. Source theory, on the other hand, he described as anabatic (as in Xenophon's Anabasis) by which he meant that it began with solid knowledge about known phenomena at accessible energies to make predictions about physical phenomena at higher energies. Although source theory was new, it did not represent a complete break with the past but rather was a natural evolution of Julian's work with operator Green's functions. His trilogy on source theory is not only a stunning display of Julian's power as an analyst, but it is also totally in the spirit of the modest scientific goals he had set in his QED work and which had guided him earlier as a nuclear phenomenologist.'[30]

A quite accessible overview of the source theory program, with particular emphasis on partial symmetry, was given by Schwinger in his lectures at the 1977 Hawaii summer school [189]. There he discussed pion–nucleon interactions, pion–pion scattering, vector mesons, electromagnetic properties of hadrons, leptonic decays of vector mesons, $\eta'$ decay, hadronic mass splittings, and $\eta \to 3\pi$ decay. This work remains influential.

But more important than the study of specific processes, Schwinger's legacy of a phenomenological approach to particle physics lives on in the mainstream ideology which no longer recognizes its source.* An example can be found in a review by Fermilab's Chris Quigg of Weinberg's book on quantum field theory:[32] 'As quantum field theory and gauge theories have become more central to our study of physics at very short distances, or very high energies, we

* A very similar conclusion has been reached by Schweber.[31]

have changed our attitude about the theories themselves. We no longer demand that our theories make sense up to arbitrarily high energies but regard them as effective theories that are appropriate to describe the important physics in various energy regimes. In many instances, effective field theories provide the most convenient tool for working out the consequences of symmetry and the general principles underlying quantum field theory. Among the many tools Weinberg presents, he shows effective field theories with particular pleasure.'[33]

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# Move to UCLA and continuing concerns

## Reception of source theory at Harvard and UCLA

Why did Schwinger leave Harvard in 1971? Certainly, he perceived that his source theory had received a chilly reception at Harvard, and thought (rather erroneously as it turned out) that UCLA, where he intended to go, would be more hospitable. In fact, in general, reaction to source theory was nearly universally negative; Schwinger's theoretical colleagues refused to learn the new language—understandable, in that a significant investment of time and energy was required, and Schwinger's presentation often put people off. He wished his audiences to rid themselves of all the machinery of operator quantum field theory that they had so laboriously acquired. Less comprehensible was the outright hostility expressed in many quarters.* The first volume of *Particles, sources, and fields* [153] received a rather scathing review† from Arthur Wightman.[2]

But probably at least as important for his relocation was the fact he had been at Harvard for 25 years, and felt the need for a change. The sunny climes of Southern California, where he could and did swim and play tennis‡ every day, and with excellent skiing only two hours away, were an enormous attraction.

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* The beginnings of this hostility can be seen in the questions to the talk given by Schwinger at the 1967 International Conference on Particles and Fields in Rochester [139a]. Milton also recalls how shocked he was when, on returning to his alma mater, the University of Washington, in the summer of 1969, having just completed one year of working with Schwinger, he discovered the extraordinarily negative reaction to the source theory program by some of Schwinger's former students.

† Wightmann 'did not recommend [the book] to the uninitiated,' and complained of 'obscurities' likely to 'baffle or hornswoggle the student.' Schwinger wrote a rebuttal to the review, but *Science* refused to publish it.[1]

‡ Schwinger, who had from his early years been very enamored of tennis, became an active player during his last few years at Harvard, partly through his playing with Asim Yildiz, who had become his student in the late 1960s, but who was an outstanding tennis player, a former member of the Turkish national team.

Apart from enjoying these sports, he became convinced of their necessity for health, an observation brought home to him by the premature death of Pauli in 1958. His doctor apparently recommended daily swimming, an impossibility in Massachusetts.[3] Although it was billed as a temporary move, it was always clear to those close to him that it was to be permanent.

We recall that UCLA in the person of David Saxon had in fact been trying to recruit Schwinger for years.* Clarice Schwinger recalled that without him they never would have gone. Schwinger and Saxon had become good friends during the Radiation Lab days. Whenever Saxon came to Boston he would invite Schwinger to come to UCLA and Schwinger always said yes. He loved to travel and he liked Los Angeles. Whenever they visited Los Angeles they had a very pleasant time.[5]

On the other hand, Clarice was not fond of Los Angeles. They first visited the city together on their honeymoon trip in 1947, when David Saxon invited them to stay with his parents for a few days, which turned into 10. They had a very good time, but Clarice hated Los Angeles. She never understood its attraction. A few years later they again visited Saxon's parents' house, and his father had a colleague visiting from Philadelphia, a man with a thriving sporting goods business there. As soon as he set foot in Los Angeles, he loved it and at the dinner table he announced that he was going back to Philadelphia to sell his business and move to Los Angeles. He did not say anything about consulting his wife and teenage children. Clarice believed that he really did it and she never understood what he saw, because to her Los Angeles was a very pleasant place to live but certainly in those days a very difficult place to visit. It seemed phony to her. There was nothing that appealed to Clarice, but there were people who loved it, and Julian was in that category. But when Saxon kept saying 'Julian, come, come,' he would say no and Clarice thought he was going to continue to say no forever. Then he surprised her.[5]

As David Saxon recalled, UCLA fared favorably compared with Berkeley in Schwinger's eyes: 'I came to UCLA in 1947. Julian and Clarice came then [on their honeymoon trip] and they stayed with my parents who had a pool. Julian didn't like Berkeley—he thought the climate was terrible. I think it was more than that: The intellectual climate was not comfortable. Julian did not like a competitive environment and that was a competitive environment. I asked him about Berkeley and he said, "Oppenheimer's all right." '[6]

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* Of course, other institutions had been seeking to recruit his services as well. For example, Chen-Ning Yang recalled his efforts to persuade Schwinger to come to Stony Brook.[4] In a remark he often repeated, he wrote to Schwinger, 'Harvard, prestigious as it is, cannot add to your lustre. It is you who brings lustre to whatever Institution you choose to join.'

As we recall from Chapter 10, the first substantial visit in the summer of 1952 'was important because systematic lectures on the new version not only of the quantum action principle but on the new attitude on quantum mechanics in which it is viewed as the symbolism of measurement were developed . . . . So I was rapidly transforming quantum mechanics into my own image, if you like.'[7]

Many other visits followed, including one in 1962 when they stayed in Brentwood and met Margaret Kivelson, his single female student, and one of his brightest, shining lights. She has been an overseer at Harvard, has worked for the Geophysical Survey, and she is very involved in the space program. It was she who told Schwinger about Sidney Coleman, who was then at Caltech. It may have been the first time he ever drove, when he drove at night from Pasadena to meet Julian. The four of them had dinner and it was agreed that Coleman would come to Harvard.[5]

Kivelson remembers these visits fondly. 'Julian came here several times. One of the most memorable evenings of my life was one when he and Clarice invited us, when they were here just on leave, to spend an evening with a very interesting group of people, Feynman and his wife, and Helen Curley Brown and her husband. Helen Curley Brown had just written *Sex and the single woman*, and she went on to be editor of *Cosmopolitan*. At that point nobody knew the name, but that didn't last very long. The evening was one of the great mismatches of all time, because Helen Curley Brown arrived full of excitement about this book she was about to publish and she just wouldn't stop talking. Feynman did not like being upstaged. Julian was trying to make peace all around. David Brown, her husband, was a Hollywood producer, and I think they had met them through mutual friends. They didn't really know them very well. They had invited them, and they had invited the Feynmans, and I think the Kivelsons were there as a buffer. It just didn't succeed.'[8]

'When I first came out here, I was a consultant at the Rand Corporation. The reason I was a consultant was that I wanted flexible hours and Rand was very accommodating, and it was an exciting place when I arrived in 1955. I really didn't like it, but I stuck with it for 10 years until the kids were grown. The hours were so good and there were a lot of good people. One of the things that Rand did was to bring talented graduate students, particularly from Caltech, they would hire them as consultants. They had hired two very, very talented young people, one was Sidney Coleman and one was Richard Dolan, who later ended up on the faculty at USC. I was extremely impressed with Sidney, he would come and he would give seminars on mostly the kinds of things Murray Gell-Mann was doing, the eightfold way, unitary symmetry, $SU(3)$, . . . . He was extremely insightful. I heard Murray lecture on the subject and I always found it more illuminating to listen to Sidney, which is quite a tribute because Murray is very interesting. So I just thought this was a great person to bring in contact with Julian and I suggested to Julian that he might consider Sidney and I remember we all went out to dinner somewhere on the Sunset Strip, and Julian, I think, was impressed and hired him. Then I think that didn't really work very well. I think he and Julian had a lot of conflicts, which was unfortunate.'[8]

In Spring 1969 the Schwingers visited UCLA in the month of April. At that point Saxon and the Physics Department had already convinced Julian. They were wooing Clarice. They were very kind to the Schwingers, sending them to the theater, to dinner. They hosted a dinner for them. They tried very hard, but Clarice recalled that she could not have been more ungracious if she tried.[5] Schwinger's official offer of a position at UCLA was preceded by one from Stanford: Leonard Schiff offered him a Professorship there in 1964 at a salary of $23,000. But this was topped by the offer, from UC President Clark Kerr, of $30,000 in 1966. Byron Wright of UCLA wrote to Schwinger the following year suggesting he head an institute. When he finally came, in 1971, his salary, as Visiting Professor, was $34,000. The most interesting aspect of these negotiations was a letter he scribbled to David Saxon in October 1968 (which may never have been sent). It read in toto:

'Dear Dave,

'I had a thought, and this is a good time to describe it. The subject is a sabbatical and the one I should take 7 years after the last one, in academic year 1962–63, i.e., 1969–70, which begins exactly one year from now. What I'd really like to do is spend 6 months in Japan and a few months on the [Continent] or England. One possibility is the fall. Spend July, August, September at UCLA, Oct.–April in Japan, the [Spring in] Europe. Harvard (I presume) will give me full salary for one semester. Would UCLA be willing to let me begin with a Sabbatical, and thus provide their full salary for the second semester? I could certainly not dare to ask this of anyone but an old friend. Just tell me if it is too outrageous, but it is a nice idea.'[1] In fact, as we recall, Schwinger took a half-year sabbatical in Japan in the Spring of 1970, funded in part by a Guggenheim Fellowship.[1]

Saxon, who was Vice Chancellor at UCLA when Schwinger moved there, and became President of the entire University of California system in 1974, emphasized that Schwinger only offered positive reasons for his move to UCLA: 'He really enjoyed this area and he was interested in the kind of outdoor recreation available. I had the strong feeling that Julian was unhappy that to an extent he was taken for granted at Harvard.*

* Paul Martin concurred in this assessment. Harvard could have done more to persuade him to stay. 'People didn't do as much for him as he deserved.' More could have been done, such as naming him a University Professor, 'if he had indicated it was important

'In the beginning [at Harvard] he was the center. He didn't think of himself as a supernova, shining brilliantly and then subsiding, but Harvard managed inadvertently to create that feeling. He never offered a single word of criticism about Harvard ever, not about Harvard or any person there. Only positive reasons for coming.'[6]

Appropriately, Los Angeles greeted the Schwingers' arrival on 9 February 1971 with a major earthquake, 6.6 on the Richter scale, the San Fernando Valley quake that killed 65 people. The first year, while on leave from Harvard, they rented a house in Bel Air; the following year, when the decision to make the move permanent had been made, they bought a beautiful home in Bel Air, with magnificent views of the city and the ocean. Of course, Clarice's mother, Sadie Carrol, lived there with them. The house turned out to be ideal for them because on the other side of the dining room there was a little suite, which they turned into an apartment for Sadie. She never intruded on their privacy in any way. It worked out very well. However, it was terribly hard on Sadie to leave Boston; Clarice thought that it was a dreadful thing to have done to her.[5]

Clarice was never happy about the move, largely for being a continent away from family and friends. As Saxon remarked, 'Clarice lived in her own dream world. The Boston she imagined no longer existed. Boston had changed and Julian was reclusive. I don't think there were a lot of Boston friends. There were a few Harvard friends. Julian was accepting but he didn't reach out. I think Clarice romanticized it.

'She wasn't enthusiastic about the house. Julian picked out the house. I tried at the beginning to help them find their house, but it soon became apparent that Julian knew what he wanted. It's not the kind of house I would have picked.

'Julian was always extremely considerate of Clarice. Except when things were important to him, he kept at it until he got them done. Moving here, the house—that was the only one left. Julian was never an overbearing person.'[6]

During the 1971–72 academic year, Schwinger was on leave from Harvard. Evidently, things went well enough that he resigned the Higgins Professorship the following year, accepting a permanent appointment at UCLA. There is an amusing legend associated with his replacement at Harvard. It took Harvard a few years to find a replacement of sufficiently exalted caliber. Finally, Steven Weinberg accepted the post of Higgins Professor, and felt very flattered to be Schwinger's successor.[10] When he moved into Schwinger's old office in Lyman Laboratory, he noticed a pair of shoes had been left in the closet. Weinberg considered them 'symbolic'; Sidney Coleman suggested he bronze them.[10]

to him.' Moreover, 'not everyone had the same reverence for him as some who were his students.'[9]

Schwinger took the opportunity the move provided to correct the error in his license plates discussed by Lowell Brown.[11] His Massachusetts plates had the number 137 039, which had been the digits of the reciprocal of the fine structure constant, the dimensionless number that expresses the strength of the electromagnetic force between electrons. In terms of the charge $e$ of the electron, Planck's constant $\hbar$, and the speed of light $c$,

$$\alpha = \frac{e^2}{4\pi\hbar c} = \frac{1}{137.035989\ldots}, \tag{14.1}$$

so, in the course of time, increased precision of measurements had rendered Schwinger's number inaccurate. Brown had attempted a temporary correction. But the move provided a permanent rectification. Since California required at least one letter in vanity plates (unfortunately not Greek), Schwinger chose brevity and universality: *A137Z.*

Missing from Julian's life in Bel Air was a cat. For 14 years while they lived in Cambridge the Schwingers had their beloved cat Galileo. They had one in the first house that they rented in Los Angeles in 1971–72 but a coyote got it. Clarice decided that she did not want the hassle of an indoor cat; she really hated changing the litter box. Also she knew from her experience with Galileo that it is not true that indoor cats do not want to go out.[5]

One thing Schwinger did not fully anticipate: the caliber of graduate students at UCLA was far inferior to what he was used to at Harvard. Consequently, after 68 PhDs in 25 years at Harvard, only five received their PhDs at UCLA in a nearly equal span of time. (These were, in order, Luis Urrutia, Walter Wilcox, Greg Wilensky, Evangelos Karagiannis, and Donald Clark. Jack Ng, who came to UCLA with Schwinger in 1971, received his PhD from Harvard in 1974.) David Saxon recalls that he had warned Schwinger that UCLA students were not as good as those at Harvard, but that Schwinger's reputation would attract better students to the University. 'The one thing we both understood as a truly major problem—Julian would bring his brain with him—was the students. "You're not going to have the same kind of students. But you're Schwinger, they ought to come here." But they did not. By that time the neglect of Schwinger was much more widespread.'[6]

Moreover, students rely excessively on recommendations by their undergraduate professors in choosing graduate schools, and those recommendations, in turn, are largely based on the reputations of schools at the time those professors were graduate students. The inertia of the system foiled the anticipation of Saxon and Schwinger.

Of course, also in 1971 gauge theories took off again, which doomed the general reception of source theory. Recall that we have earlier (in Chapter 12) discussed the road to electroweak unification and emphasized that no one took what is now called the Glashow–Weinberg–Salam theory very seriously until Gerhard 't Hooft proved that it was renormalizable. In spite of nearly everyone's doubts, quantum field theory *could* describe weak interactions, and surely strong interactions as well (the $SU(3)$ color theory quantum chromodynamics—QCD—was proposed only one year later by Murray Gell-Mann, Herald Fritzsch, and William Bardeen[12]). The alternatives to field theory, from current algebra to dispersion relations, were now seen to be not contenders at all, but merely consequences of renormalized quantum field theory, as Schwinger had believed all along. Unfortunately, Schwinger's new approach to field theory, i.e. source theory, was, in the eyes of most, to be discarded as well, even though it was meant to be a more effective unification of quantum mechanics and special relativity than conventional operator field theory.

In any event, Schwinger was very much aware of what was going on, and proposed his own $U(2)$ version of the 'standard model' in 1972 [155], phenomenologically acceptable in those days. As we stressed in Chapter 12, and as Sheldon Glashow had reminded us, Schwinger had had a fundamental role in making the electroweak synthesis possible. Glashow remarked: 'Throughout the four decades since my first meeting with Julian, whenever I accomplish something that turns out to be right, I sense that Julian is complimenting me, and at the same time, reminding me that he had said much the same thing decades ago ... and I think he is right.'[13] The three postdocs he had brought from Harvard, DeRaad, Milton, and Tsai, mockingly self-styled 'sourcerer's apprentices,' thereafter contributed several papers to the development of the electroweak theory.[14]

We recall that Schwinger had started writing the second volume of *Particles, sources, and fields* in 1970; he completed this, and the proofs were read scrupulously by the postdocs. The volume, devoted to electrodynamics, came out in 1973 [158]. Schwinger summarized the content of the book in the publicity blurb he wrote: 'Here is the definitive statement of modern quantum electrodynamics. The issues discussed range from vacuum polarization, in a variety of applications, to the hyperfine structure of positronium and muonium, with occasional excursions into nuclear and high energy physics. Based as it is upon the conceptually and calculationally simple formulation of source theory, little in the way of formal mathematical sophistication is required, and thus most of the book is devoted to the working out of physical problems. All essential details are explictly presented in these discussions. This book should be of interest and value to graduate students in all branches of theoretical physics, to atomic, nuclear, and high energy physicists, to mathematicians interested in physical applications, and to philosphers and historians of science.'[1]

With some very impressive work on electrodynamics (including methods harking back to his 1951 'Gauge invariance and vacuum polarization' paper

[64] and other classic papers, an independent calculation of the fourth-order contribution to the electron's magnetic moment, strong-field electrodynamics, and a revisiting of the axial-vector anomaly* which he had discovered in [64]) constituting the first half of the third volume of *PSF*, he abandoned work on the book at the point where he had to face up to strong interactions. The manuscript version of *Particles, sources, and fields* leaves off with the following interchange between Harold and Schwinger:

'Harold looks worried.

'H: Somehow, I have the unhappy feeling that we are about to leave the pleasant shores of Electrodynamics for murkier waters. Surely there are still other topics in Electrodynamics that merit consideration?

'S: Yes there are. One example is the area of very high energy collisions where the application of the source description to the individual colliding particles provides an obvious initial approximation. But, it should be clear from the confrontation of this section that the physically oriented insights of Source Theory are sorely needed in the still little understood domains of strong and weak interactions. That, after all, was the major reason for introducing this new approach and at long last we are ready, or as ready as we shall ever be, for our primary task.'[19]

In fact, in the Schwinger archive at UCLA there are a few pages of a manuscript representing Schwinger's attempt to continue writing the book, on the subject of partial symmetry.[1] But this attempt was evidently quickly abandoned, after

* This largely consisted of a confrontation with the establishment. As we have noted, Stephen Adler, John S. Bell, and Roman Jackiw had rediscovered[15] the anomaly in 1968, and within a year or so, Adler and Bardeen proved the 'non-renormalization theorem,' that the anomaly was exactly given by lowest order in perturbation theory, i.e. that all radiative corrections to it vanished.[16] In 1972 Schwinger's postdocs carried out a detailed calculation in which they found a contrary result, that the anomaly was corrected in second order by the factor $1 + \alpha/2\pi$, $\alpha$ being the fine structure constant.[17] Upon discussion with Adler, they discovered that the source of 'this discrepancy is due to the fact that we have normalized the pseudoscalar form factor at zero momentum transfer squared, rather than at $2m^2/\ln(\mu^2/m^2)$, where $\mu$ is the fictitious photon mass. Independent of the choice of normalization point, there exist radiative corrections to the low-energy theorem for $\pi^0$ decay.' Now Schwinger discovered the same correction, but refused to concede the validity or utility of the conventional normalization point. Not only does this confrontation appear in *Particles, sources, and fields*, but he wrote it up into a joint paper with DeRaad and Milton, which, however, was never submitted for publication.[1] This is presumably because at about that point he gave a seminar on the subject at MIT, very confrontational in tone, which was regarded by those in attendance as 'deeply wrong.'[18, 10] Schwinger was probably not convinced by the objections, since the corresponding section of *Particles, sources, and fields* [211] was retained, but he decided it was not worth battling referees in an attempt at journal publication.

only 13 manuscript pages. The uncompleted third volume eventually came out in 1989 when Addison-Wesley repackaged the whole set [211]. As Schwinger said in the special preface to that set, 'I began work on a third volume. That activity continued from 1972 until 1974 when rapid experimental developments in high energy physics brought it to a halt.' (The developments referred to are the deep inelastic scattering experiments discussed on pp. 500–505.)

From the beginning, interactions with other members of the Theoretical Elementary Particles (TEP) Group at UCLA were rocky.* Apparently there was considerably unhappiness that Schwinger took no interest in the research of the other members of the group, even though he was merely maintaining his lifelong attempt to avoid 'conversational physics.'[7] 'Why did we bring him if he isn't going to interact,' seems to have been the reaction of some at least. Early on, Nina Byers brought a birthday cake to Julian. Clarice opened the door and said, 'I've already baked him a cake,' and [closed] the door.[20] Still, Clarice remembered the event rather fondly.[5]

## Strong-field electrodynamics revisited

In 1973 Schwinger's interest in strong-field electrodynamics was reborn, with the publication of 'Classical radiation of accelerated electrons. II. A quantum viewpoint' [156], the first paper in this series having been published in 1949 [56]. The latter, in turn, grew out of the war work at the Radiation Lab, which we discussed in Chapter 5. What rekindled Schwinger's interest?

One answer lies in the astrophysical situation in the early 1970s. Pulsars had been identified as rapidly rotating neutron stars, where a simple argument based on flux trapping suggested that the magnetic fields on the surface of the neutron star could be of the order of $10^{13}$ gauss. This is not far from the 'critical field strength' $H_0 = m^2/e = 4.41 \times 10^{13}$ gauss, that is, the magnetic field strength where the energy of interaction of a Dirac magnetic moment $e/2m$ with the magnetic field just equals the rest-energy of the electron. (Here, $e$ and $m$ are the charge and mass of the electron, respectively.) At such a point one might naively expect that quantum effects could become significant. (Schwinger showed, in fact, that they do not become important.)

Because of this astrophysical application, Schwinger had suggested to Asim Yildiz, an engineer who had become Schwinger's student and tennis instructor a few years before Schwinger left for California, that he re-examine electrodynamics in the presence of an arbitrarily strong magnetic field. Yildiz had not gotten very far, so Schwinger took matters in his own hands. The opening

* One of Saxon's legacies as chairman of the UCLA Physics Department was the formal establishment of research groups within the department. Unfortunately, this seems to have had a divisive effect.

lines of [156] set the stage: 'For a long time I have wanted to reexamine a classic situation of classical electrodynamics, that of high-energy charged particles radiating in a homogeneous magnetic field, from the modern quantum viewpoint that employs the machinery of propagation (Green's) functions. Since the electromagnetic and relativistic aspects of the problem are quite transparent, the comparison should be instructive in giving the more abstract quantum procedure a concrete interpretation in a particular instance. And, as an added bonus, the necessary ability to treat motion in magnetic fields that goes beyond the lowest orders in a perturbative expansion should be helpful in answering questions about very strong fields, to which recent astrophysical speculations have directed attention. This paper is devoted to describing one such procedure, and applying it to rederive (for a spin-0 particle) the known classical radiation result [56]. Another method is indicated in a separate paper of Yildiz. A subsequent joint paper will contain the analogous spin-$\frac{1}{2}$ calculation, and a discussion of the anomalous magnetic moment in strong field.' (Schwinger's name does not, in fact, appear on the article discussing the spin-$\frac{1}{2}$ situation.[21] However, much of the discussion does appear in the third volume of *Particles, sources, and fields*, which was not published until 1989 [211].)

Although this paper was couched in the new source theory language, the procedure followed was actually the same as that Schwinger's monumental 1951 paper, 'On gauge invariance and vacuum polarization' [64]. What he did was calculate the mass operator for a spinless electron in the magnetic field,

$$M = \mathrm{i}e^2 \int \frac{(\mathrm{d}k)}{(2\pi)^4}(2\Pi - k)\frac{1}{k^2}\frac{1}{(\Pi - k)^2 + m^2}(2\Pi - k) + \text{c.t.}, \qquad (14.2)$$

where $\Pi = p - eqA$ is the gauge-covariant momentum operator ($q$ is the charge matrix), and c.t. stands for a contact term necessary to remove terms present when space–time points overlap. He first introduced the proper-time representation for the propagators, for example,

$$\frac{1}{(\Pi - k)^2 + m^2 - \mathrm{i}\epsilon} = \mathrm{i}\int_0^\infty \mathrm{d}s_1\, \mathrm{e}^{-\mathrm{i}s_1[(\Pi-k)^2+m^2]}, \qquad (14.3)$$

and then combined the two exponentials together by introducing Schwinger–Feynman parameters, in terms of the Hamiltonian

$$H = (k - u\Pi)^2 + u(1 - u)\Pi^2. \qquad (14.4)$$

The mass operator is thus written as

$$M = -\mathrm{i}e^2 \int_0^\infty \mathrm{d}s\, s \int_0^1 \mathrm{d}u\, \mathrm{e}^{-\mathrm{i}sm^2u}\langle(2\Pi - k)\mathrm{e}^{-\mathrm{i}sH}(2\Pi - k)\rangle + \text{c.t.}, \quad (14.5)$$

where the expectation value signifies an integration over $k$. The Hamiltonian is introduced in order to represent proper time development, for example,

$$\Pi(s) = e^{isH}\Pi e^{-isH}. \tag{14.6}$$

from which follow the equations of motion, such as

$$\frac{d\Pi(s)}{ds} = 2ueqF[\Pi(s) - k], \tag{14.7}$$

which employs the commutator

$$[\Pi_\mu, \Pi_\nu] = ieqF_{\mu\nu}. \tag{14.8}$$

In Eqn (14.7) the field strength $F^{\mu\nu}$ is regarded as the $\mu\nu$ component of a matrix. 'The simplicity of the homogeneous field situation is the linearity of the equations of motion, which permits their exact solution.' This is because the problem is then equivalent to a set of harmonic oscillators. Thus, the solution to (14.7) is

$$\Pi(s) = e^{2ueqFs}\Pi + (1 - e^{2ueqFs})k. \tag{14.9}$$

Schwinger went on to collect all the ingredients necessary to construct $M$. But, without explicitly giving the latter, he immediately took the classical, high-energy limit, and obtained the result for the power spectrum given in [56] (first stated, in terms of modified Bessel functions, in [32a]),

$$P(\omega) = \frac{\alpha}{\pi}\omega\frac{m^2}{E^2}\left[\int_0^\infty dx\,(1+2x^2)\frac{\sin\frac{3}{2}\xi(x+\frac{1}{3}x^3)}{x} - \frac{1}{2}\pi\right], \tag{14.10}$$

where $\xi = \frac{2}{3}(\omega/\omega_0)(m/E)^2$, $\omega_0$ is the Larmor frequency (the frequency of revolution of the electron in its orbit), $\omega$ is the frequency of the radiation, and $E$ is the relativistic energy of the electron.

Interestingly, Schwinger concluded this quite ingenious paper with an Appendix, containing several errata to [56], which 'seems to have escaped proof-reading.' The appearance of this erratum serves to emphasize the continuity of Schwinger's work, belying the sharp break that he made publicly with conventional field theory. In fact, one of the authors of the present volume (K.A.M.) once remarked to Schwinger something to the effect that the first source theory paper was really 'Gauge invariance and vacuum polarization' [64]. Schwinger smiled and concurred. In a very real sense, Schwinger's work was always conservative and revolutionary at the same time—building on what was true and useful, yet never afraid to leap into the unknown.

This revisitation of synchrotron radiation from a quantum viewpoint sparked a whole revitalization of a subfield of physics, and spawned a cottage industry at home and abroad. In particular, Schwinger's postdoc Wu-yang Tsai became very interested in the subject, as did Tom Erber, Professor at the Illinois Institute of Technology, who frequently made extended visits to UCLA in the 1970s and later. Erber, who did both theory and experiment, had wide-ranging unconventional interests, and had proposed an experiment to see if quantum corrections to synchrotron radiation were observable.[22] So naturally, collaborative publications ensued. Tsai and Schwinger wrote a paper on 'Radiative polarization of electrons' [159], which used Schwinger's methods to rederive the fact that electrons moving in a constant magnetic field, as in a synchrotron, become polarized because the rate at which the electron radiates electromagnetic energy depends on the orientation of the electron's spin. This effect had first been discovered by Sokolov and Ternov ten years earlier.[23] Schwinger, Tsai, and Erber developed the 'Classical and quantum theory of synergic synchrotron-Čerenkov radiation' [176], which treats the radiation produced by a charged particle moving in a dielectric medium in the presence of an external magnetic field. (The Čerenkov effect refers to the radiation produced when a charged particle moves faster than the speed of light in a medium.) 'It will appear that there is actually a single emission act, synergic synchrotron-Čerenkov radiation, for which a correspondence with either Čerenkov emission or synchrotron radiation can be established only in the respective limits of vanishing field or matter density. The practical import of the synergism is that the radiation depends sensitively on both positive and negative values of $n(\omega) - 1$ [that is, on the deviation of the index of refraction from unity], and also exhibits Airy function oscillations reminiscent of the intensity fluctuations near caustics.' This paper continues to have important applications to astrophysics, and, for example, to the construction of X-ray detectors.

Controversy with Erber led to Schwinger's final publication on this subject. This history of this takes us back to the 1950s. We recall that Schwinger [78], and independently Sokolov, Klepikov, and Ternov[24], had shown that quantum corrections to synchrotron radiation were negligible; that is, more precisely, the first quantum correction could be significant only when the product of the particle energy $E$ and the applied magnetic field $H$ was very large, in units of billions of electron volts and thousands of gauss,

$$E(\text{GeV})H(\text{kG}) \sim 10^8, \tag{14.11}$$

for an electron, a condition which is never approached in practice. (Even for LEP, where $E \approx 90$ GeV and $H \approx 1$ kG, this product is never large.) However, now Latal and Erber[25] argued that the second-order quantum correction could be significant. So Schwinger and Tsai wrote a 'New approach to quantum corrections in synchrotron radiation' [186] in which they showed definitively that this was not the case, and that, in fact, the power radiated in a synchrotron is modified by a factor which is a power series in the product $EH$,

$$P = \frac{e^2}{4\pi}\frac{2}{3}\omega_0^2\left(\frac{E}{mc^2}\right)^4\left(1 - \frac{55\sqrt{3}}{24}\Upsilon + \frac{56}{3}\Upsilon^2 + \cdots\right), \tag{14.12}$$

where the prefactor is the familiar classical expression for the total power radiated by an electron in a synchroton, and

$$\Upsilon = \frac{3}{2}\frac{(e\hbar/mc)H}{mc^2}\frac{E}{mc^2}. \tag{14.13}$$

'Therefore, we conclude that there is no evidence for the second-order quantum correction to be more important than the first-order correction.'

Both Erber and Tsai, independently and jointly, continued to write papers on strong field electrodynamics using Schwinger's 'new' methods. But it would be out of place to follow those developments here.[26]

## The November revolution: the discovery of $J/\psi$

Perhaps the most exciting period in high-energy physics in the latter half of the twentieth century was born in November 1974. On the weekend of 10 November 1974, experimenters at the Stanford Linear Accelerator Center (SLAC) and at Brookhaven National Laboratory, led by Burton Richter and Sam Ting, respectively, announced the discovery of an extremely narrow spin-1 resonance coupling to the photon; it was called $\psi$ by SLAC, because of the resemblance of its decay pattern to the Greek letter, and $J$ by BNL because of its similarity to a character in Ting's name.[27] Because of a never resolved priority dispute, this first resonance is still called $J/\psi$, although its higher energy reincarnations are universally denoted by $\psi$. What was remarkable was that although this state had a high mass, $m_{J/\psi} = 3.1$ GeV, it had a very small width, only $\Gamma_{J/\psi} = 90$ keV, but 0.003% of its mass, unbelievably small for a strongly interacting particle. For example, the $\rho$ meson, also a spin-1 particle, which plays an important role in nuclear physics, is much lighter, $m_\rho = 770$ MeV, but has a width of 150 MeV, 20% of its mass. So it was immediately apparent to physicists that something new and important was happening.

Theorists immediately rushed into print with a variety of ideas, old and new. Of course, we now know the explanation: $J/\psi$ is a bound state of a charmed and an anti-charmed quark whose strong decays are greatly suppressed by the OZI mechanism.[28] But Glashow, Illiopoulos, and Maiani, who proposed the charmed quark in order, by the 'GIM' mechanism[29] (which we discussed in Chapter 12), to suppress flavor-changing neutral currents, had not anticipated that their new quark would manifest itself in such a striking way. By early 1975 it seemed clear to most physicists that this resonance had clinched the existence of the quark model, which had already been given strong support by deep-inelastic scattering experiments (See pp. 500–505). Indeed, the $\psi$ particle system soon came to be like the hydrogen atom of subnucleon physics: with a simple linear potential, the states could be calculated quantum-mechanically in good agreement with observations. This was the famous charmonium spectrum.[30] But in November 1974 the explanation was far from obvious. The first issue of *Physical Review Letters* for 1975 had nine theoretical papers on this narrow resonance. Among them was Schwinger's 'Interpretation of a narrow resonance in $e^+e^-$ annihilation' [166].

Schwinger's note was at heart an advertisement for an idea he had published a year and a half earlier. In 'How to avoid $\Delta Y = 1$ neutral currents' [157] ($Y$ stands for hypercharge, introduced by Schwinger in 1956 [81]), Schwinger decried the proliferation of quarks, necessary in the GIM mechanism to cancel strangeness-changing neutral currents: 'Unified theories of electromagnetic and weak interactions generally face a problem with hadronic neutral currents that change hypercharge. Such currents are strikingly suppressed in nature, but are usually implied by the Cabibbo rotation that introduces the $\Delta Y = 1$ charged currents. This has led to several suggestions, of varying degrees of charm, which are uniformly couched in the language of hypothetical subnuclear constituents. The number of the latter has thereby been increased, from three, to four, five, seven, . . . .'

In this 'anti-Cabibbo' paper, Schwinger proposed that hadrons couple to the electroweak gauge bosons $\gamma$, $W^\pm$, $Z$ (incidentally, recall that Schwinger had proposed his own $U(2)$ electroweak model in [155]) through vector and axial-vector intermediaries—a vector dominance model, and hardly objectionable. (In fact, symbolically, these could be thought of as currents constructed from quarks.) But rather than the usual Cabibbo idea of having the weak interactions couple to a single direction in the $SU(2)$ flavor plane he introduced two sets of spin-1 hadrons coupling to electroweak gauge bosons uniformly. Then, as a result of strong symmetry breaking, these hadronic states broke into what Schwinger called $v$, $a$, and $v'$, $a'$ ($v$ and $a$ representing vector and axial-vector fields, respectively), where the unprimed states were the familiar $1^-$ nonuplet, $\rho$, $K^*$, $\phi$, $\omega$. The final hypothesis was that 'the fields $v'$, $a'$ are only slightly coupled to the quasistable hadrons that are of interest in weak interaction measurements.' A mixing angle, effectively equivalent to the Cabibbo angle, thus emerged in the coupling of the hadronic currents to the charged weak gauge bosons $W^\pm$. No such mixing occurred in the neutral sector, that is, in the coupling to the $Z$, so the flavor-changing neutral currents are non-existent from the outset.

Schwinger's interpretation of the narrow resonance $J/\psi$ was that it was one of these weakly coupled 'primed' hadrons, say the $\rho'$. He spoke on this work at the Coral Gables conference in January 1975, opening with the words, 'Surely, in the year of '75, there is hardly a man, correctly a person, now alive, who has not heard of the new particle called psi on the West Coast, $J$ on the East Coast.'[1] Referring to the nine *Physical Review Letters* articles appearing in January 1975, he stated 'My own contribution was purely phenomenological in character. But, as I have also noticed, thanks to the cool responses of individuals and audiences, phenomenology seems not to be enough; a speculative model is considered superior, or at least more interesting, no matter how logically inconsistent it may be. Accordingly, here is my speculation.' With these words, he introduced 'Psi particles and dyons' [169], in which he applied his speculative model of subhadronic constituents made up of dyons, particles carrying both electric and magnetic charge. (See pp. 514–519.) He was able to create a scenario in which the $\psi$ and $\psi'$ particle were members of a 'noninvariant magnetic multiplet,' and hence decayed slowly.

Schwinger's two final papers on the $\psi$ system were cute bits of phenomenology. 'Resonance interpretation of the decay of $\psi'(3.7)$ into $\psi(3.1)$' [170] and 'Pion spectrum in decay of $\psi'(3.7)$ to $\psi(3.1)$' [171], written with postdocs Milton, Tsai, and DeRaad, showed that the decay $\psi' \to \psi + \pi^+\pi^-$ could be quantitatively explained in a chiral model in which the pions couple through the intermediary of a scalar particle, $\epsilon$, through the two-step process,

$$\psi' \to \psi + \epsilon, \quad \epsilon \to \pi^+\pi^-, \tag{14.14}$$

with chirally invariant couplings of the form

$$\psi'_\mu \psi^\mu \epsilon, \quad \epsilon\, \partial_\mu \pi^+ \partial^\mu \pi^-. \tag{14.15}$$

Here we have ignored the coupling constants in front of these interactions. (Here $\epsilon$ is phenomenologically identified as $\sigma$, the chiral partner of the pion, introduced in [82].) The resulting pion spectrum is peaked toward high values of the invariant $\pi^+\pi^-$ mass, in agreement with experiment. This phenomenological analysis remains relevant.

This $\psi'$ decay calculation, elegant in concept, but quite trivial in execution, sparked a lasting break with his recently graduated student Asim Yildiz, who maintained some sort of unpaid position at Harvard, as well as a professorship of engineering at the University of New Hampshire. As Schwinger and his postdocs were completing the manuscript of the first paper in the Spring of 1975, Yildiz visited UCLA for a few days, and, with his usual affable self, learned in detail what was involved in the calculation. He then returned to Harvard, and with two students wrote a paper with precisely the same ideas, but without even an acknowledgement of having spoken to the UCLA group.[31] Schwinger was incensed. He dashed off a note to his associates: 'Absolutely unconscionable that A. Y. should get away with it! Suggest you 3 write immediately to *PRL* saying you have just seen their report, and while you don't give a damn what they say about charmonium, etc., it is an absolute lie that the phenomenological part was independent, or even partly so, since A. Y. learned all this by visiting UCLA, before which he had no idea of the $\epsilon$ model with its derivative coupling.'[32] In the end, both papers were published in the *Physical Review*, but Yildiz never spoke to Schwinger again.

The UCLA archives contain interesting correspondence[1] with San Fu Tuan of the University of Hawaii bearing on this dispute. He was the referee for the *Physical Review* paper [170] on the $\epsilon$ decay model. In his referee report he apologized for sending a copy of his letter to Schwinger on this subject to Wonyong Lee at Columbia, and the difficulty with Feinberg there, who had made no reference to Schwinger's related work. He recommended publication in *Physical Review Letters*, but, evidently, was overruled. Further correspondence indicates that Tuan had also sent a letter to Shelly Glashow at Harvard and to Al Wattenburg at Illinois, which may have had a bearing on the Yildiz *et al.* paper.[31]

## Renormalization group without renormalization group

In 1974 Schwinger wrote two papers on 'renormalization group without renormalization group' [164, 165]. Unfortunately, these were published in one of his favorite journals, the *Proceedings of the U.S. National Academy of Sciences*, which had the advantage of freedom from hostile reviewer comments, but the disadvantage of a small readership among physicists. (The contents of that prestigious journal belong largely to the biological and medical sciences.) Nevertheless, they did spark some controversy at the time, and continue to have relevance to the phenomenology of QCD.

Actually, originally Schwinger wrote two papers, entitled 'Electrodynamics renormalization group—without renormalization or a group' and 'Asymptotic spectral forms of spin-$\frac{1}{2}$ and 0 Propagation Functions'. Because of hostile response, the second paper, which applied the ideas of the first to 'matter' propagators, was withdrawn, and the first, revised and retitled less confrontationally, 'Photon propagation function: spectral analysis of its asymptotic form' was published in the *Proceedings* [164]. Copies of both these preprints can be found in the Schwinger Archives at UCLA.[1] The Acknowledgment in the typescript of the revised paper reads 'This note is a modified version of a paper written in November, 1973, which had no significant preprint distribution but was the basis of a lecture delivered at Harvard in March, 1974. The original paper was submitted to a physics journal [presumably *Physical Review*], and then withdrawn, thanks to the gratuitous opinion of a referee that "this paper does not add any clarity to the subject it addresses itself to [sic].'" This comment does not appear in the published version.

What were these papers about? The 'renormalization group' constitutes one of the most important tools in the hands of theoreticians in field theory and statistical mechanics. It is a way of summing up important classes of processes, and thereby, to some extent, to transcend the limitations of perturbation theory. It originated in the work of Murray Gell-Mann and Francis Low[33], who introduced the notion of a 'running' coupling constant, $e_M$, defined in terms of processes taking place at a mass (or energy) scale $M$. For example, in electrodynamics, we usually think of the charge $e$ as defined by atomic processes occurring at essentially zero energy (in the high-energy context); but we could define the charge in terms of the annihilation process into tau leptons,

$$e^+e^- \to \tau^+\tau^-, \tag{14.16}$$

which because the mass of the $\tau$ is so great (1.8 GeV), would give a value of $e$ about one per cent bigger. The idea is simple enough: the value of the charge depends upon which physical process is used to normalize the charge. And the fact that the charge gets smaller as the energy gets lower, or, by the uncertainty principle, as the distance gets greater, reflects the fact that virtual electron–positron pairs that appear in the vacuum partially screen the charge, the virtual positrons being attracted toward the negative electron, and the virtual electrons repelled.* Of course, if one could solve QED exactly, it would be completely irrelevant which charge definition is adopted. But because we are limited to calculating in low orders in perturbation theory, we have to know how to relate one charge definition to another. Let $e_\lambda$ and $e_M$ be the charges defined at scales $\lambda$ and $M$, respectively. Assuming that both energy scales are large compared to any relevant masses, we may regard $e_M$ to be a function of $e_\lambda$ and the mass ratio $M/\lambda$, or perhaps better in terms of the squares,

$$e_M^2 = e_M^2\left(\frac{M^2}{\lambda^2}, e_\lambda^2\right). \tag{14.17}$$

The Gell-Mann–Low function $\psi$ (or in modern parlance the $\beta$ function) may be defined as the logarithmic derivative of this quantity:

$$\psi(e_M^2) = \frac{\partial}{\partial \ln(M^2/\lambda^2)} e_M^2\left(\frac{M^2}{\lambda^2}, e_\lambda^2\right) = M^2\frac{\mathrm{d}}{\mathrm{d}M^2}e_M^2. \tag{14.18}$$

* This is what is meant by the polarization of the vacuum, first discussed by Dirac and Heisenberg, with implications worked out by Serber, Uehling, and Schwinger.[34]

Although these concepts were introduced in the 1950s, it was not until 1970 that the full notion of the renormalization group, which bears an intimate tie to scale invariance, was developed by Kenneth G. Wilson, Curtis Callan, and Kurt Symanzik.[35] It became extremely important when it was discovered, three years later, independently by David Gross and Frank Wilczek, and by David Politzer, that in a non-Abelian gauge theory such as QCD, instead of the coupling increasing with energy as we mentioned above in QED, the coupling becomes weaker as the energy scale becomes greater.[36] This phenomenon was quickly dubbed 'asymptotic freedom' because it meant that at short distances (= high energies) the coupling between the quarks in a hadron becomes weak, and one can reliably use perturbation theory. Thus it became practical to apply quantum field theory to strong interactions, at least when the quarks are close together, or are hit by high-energy projectiles (see pp. 500–505). Immediately then, quantum chromodynamics, QCD, became the accepted theory of strong interactions.

It was natural that Schwinger would wish to examine such concepts from his fresh viewpoint. In particular, most, but not all, treatments of these renormalization group concepts made use of the fact that conventional field theory is divergent, and requires renormalization, in contrast to source theory, which is always finite, and only requires imposition of normalization conditions. His starting point was the photon propagation function, naturally, since it is the photon which is the intermediary for the force between electrons (Gell-Mann and Low had the same beginning). What was novel was Schwinger's fundamental* use of a spectral representation for that propagator, that is, a 'dispersion relation,' but here having its origin in the causal exchange of real particle states. There are many such representations. The one Schwinger emphasized was for the inverse of the propagator $D(k)$,

$$e^2D(k) = \frac{e^2}{k^2}\left[1 - e^2k^2\int \frac{\mathrm{d}M^2}{M^2}\frac{s(M^2)}{k^2+M^2}\right]^{-1}. \tag{14.19}$$

Here $s(M^2)$ is a positive function of the mass of the exchanged excitation under causal circumstances. (Recall our discussion of source theory in the previous chapter.) Following a simple argument based on breaking up the spectral integration into different mass regions, Schwinger was able to recover the Gell-Mann–Low equation, but with the initially mysterious function $\psi$ replaced by the spectral function,

$$\psi(x) = x^2\sigma(x), \tag{14.20}$$

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* In fact Gell-Mann and Low[33] had used such representations, first introduced by Källén[37]; however, those spectral forms did not play an essential role in their analysis.

where he defined, for $M$ large compared to any relevant mass scale,

$$s\left(M^2/m^2, e^2\right) \approx \sigma(e_M^2). \tag{14.21}$$

(Here $e$ and $m$ may be thought of as the usual charge and mass of the electron, or $m$ may be 'a typical mass of the charged particles that are dynamically significant at the particular level of excitation under consideration.') Unlike $\psi$, the spectral function is experimentally measurable, and has physical requirements, such as positivity and known analytic behavior.

Because this first published paper caused some consternation among the 'experts,'* Schwinger followed with a second paper [165] (as did Milton[38] independently—originally, they were going to write a joint paper, but ultimately Schwinger felt that their approaches were too disparate to join) showing that this relation (14.20) is exact, provided one makes a suitable change of variables: That is, the difference between the traditional and Schwinger's approach indeed just lay in the choice of how the effective charge is defined, which was an essentially arbitrary definition. He also showed how one can obtain equivalent results on the basis of other spectral representations for the photon propagator.†

Finally, in case the reader thinks that this is merely an esoteric theorist's dance, let us jump to the present. Very recently the conventional definitions of the strong QCD coupling, $\alpha_s$, which 'runs' according to the asymptotic freedom equations referred to above, have been criticized as being internally

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* Jackiw recalled that this was one of two topics Schwinger discussed in a seminar at MIT in 1974, the other being the presence of radiative corrections to the axial-vector anomaly, discussed in the footnote on p. 488. Both conclusions went against the accepted wisdom, and were generally rejected outright—so much so that Schwinger never gave a regular seminar again in Cambridge. In fact, both results were technically and mathematically correct, the difference being one of philosophy.[18, 10]

† Sometime around this point, in the spring of 1974, Clarice held a dinner party, which was actually a blind date, at which Lester DeRaad was introduced to Margarita Baños, the daughter of Alfredo Baños, a theoretical plasma physicist at UCLA, who had worked with Schwinger at the Radiation Lab during the war. (Baños had come to UCLA right after the war, and was responsible for David Saxon joining the physics department there a year or so later, so he was indirectly responsible for Schwinger's move to the West Coast.) DeRaad was not pleased at being set up in this way (unbeknownst to all, he had a girlfriend who would shortly become his wife), and the evening rapidly deteriorated into an argument between DeRaad's libertarian philosophy on one side and Margarita's liberal views on the other. Milton heard about this encounter at breakfast the next day, and was warned to expect a call from Clarice. Milton was at that point a less desirable catch, as he had been married and divorced. Indeed, a few weeks later the call came, but with no invitation, merely a description and a telephone number. Milton and Margarita Baños married four years later.

inconsistent, in that they violate physical casuality requirements, which mathematically manifest themselves in terms of analytic properties.[39] Of course, the correct physics and mathematics were built into Schwinger's approach. A correction was proposed, making a change of several per cent (and therefore easily observable) in the value of the strong coupling at intermediate energies, and, remarkably, the new $\beta$ or $\psi$ function is proportional to the spectral density, as Schwinger proposed. This work is turning out, for example, to be important in the analysis of $\tau$ lepton decay and in the analysis of deep-inelastic scattering experiments.[40] Schwinger's ideas here are, perhaps, just now beginning to bear fruit.

## Deep inelastic scattering and Schwinger's reaction to partons and quarks

Even before the 'November revolution' Schwinger was determined to keep on top of developments in high-energy physics, for in 1974 Julian continued his iconoclastic interpretation of phenomenology with an alternative viewpoint of deep inelastic scattering based on double spectral forms (the precursor was the Deser–Gilbert–Sudarshan representation,[41] due to one of his students and two of his assistants or postdoctoral fellows), work which continued until 1977 [167, 168, 173, 178, 179, 179a, 181–83], starting from the valid premise that scaling does not necessitate the existence of point-like constituents. Let us discuss this interesting development in detail.

Experiments carried out at the Stanford Linear Accelerator Center (SLAC) in the late 1960s showed a remarkable simplicity.[42] What was observed was the phenomenon referred to as scaling behavior. It had been anticipated by the work of James Bjorken,[43] and given what became the definitive explanation in terms of the so-called parton (quark) picture of Richard Feynman.[44] We can summarize the experimental situation, and Schwinger's philosophical attitude toward it, by quoting the opening paragraph of 'Source theory viewpoints in deep inelastic scattering' [167]: 'No experiment in the recent history of high energy physics has had more impact on the theoretical community at large than the deep inelastic scattering experiment of the MIT–SLAC collaboration[42]. Very high energy electrons are inelastically scattered off individual nucleons, resulting in the production of various nucleonic excited states, or resonances. It is found that, with increasing inelasticity, this resonance structure very quickly blends into a smooth pattern that shows a remarkably simple, scaling dependence on the two independent variables of the experiment, which measure the energy and invariant momentum transfer to the nucleonic system. It was the emergence of this scaling behavior that set off an orgy of speculative model building and abstract theorizing, which has raged unchecked until recent experiments on hadronic production in electron-positron collisions dealt a body blow to the confident (but discordant) predictions that accompanied the various speculative viewpoints. Perhaps the time is now propitious for a reassessment of the situation, one that focuses more on correlating experimental facts and less on the urge to speculate about the ultimate constituents of matter. The systematic evolution of particle physics that is based on the epistemological attitude of the last sentence is known as source theory. Although it arose in response to the continuing crisis in high energy physics, the major attention for some time has been given to honing its blade on the whetstone of electrodynamics. Here we begin to wield this weapon in the arena for which it was forged.' (This marked the end of Julian's writing his treatise, *Particles, sources, and fields*, which focused on QED.)

The interaction of an electron with a nucleon is through the intermediary of a photon. We are interested, first of all, in the total cross-section, which, by the optical theorem, is given by the imaginary part of the forward scattering amplitude. Let $q$ be the momentum transferred to the nucleon, that is, $q^\mu$ is the four-momentum of the photon. The other variable is the energy lost by the electron in the rest frame of the initial nucleon (the lab frame), which, invariantly, is written as

$$\nu = -\frac{qp}{m}, \tag{14.22}$$

where $p$ is the initial momentum of the nucleon, and $m$ is its mass. The forward virtual Compton scattering amplitude is given in terms of two scalar functions, $H_{1,2}(q^2, \nu)$, which are the coefficients of the two possible symmetric, conserved, tensors,

$$T_{1\mu\nu} = m^2(q_\mu q_\nu - q^2 g_{\mu\nu}), \quad q^\mu T_{1\mu\nu} = 0, \tag{14.23}$$

$$\begin{aligned} T_{2\mu\nu} &= q^2 p_\mu p_\nu - qp(q_\mu p_\nu + p_\mu q_\nu) \\ &\quad + (qp)^2 g_{\mu\nu} + m^2(q^2 g_{\mu\nu} - q_\mu q_\nu), \end{aligned} \tag{14.24}$$

$$q^\mu T_{2\mu\nu} = p^\mu T_{2\mu\nu} = 0. \tag{14.25}$$

The total cross-sections for longitudinally and transversely polarized photons, respectively, are then given in terms of the imaginary parts of the amplitudes $H_{1,2}$:

$$\sigma_L = \frac{8\pi\alpha}{m\nu}\frac{m^2 q^2}{\sqrt{1+(q/\nu)^2}}\mathrm{Im}\, H_1, \tag{14.26}$$

$$\sigma_T = \frac{8\pi\alpha}{m\nu}\frac{m^2}{\sqrt{1+(q/\nu)^2}}\left[(q^2+\nu^2)\mathrm{Im}\, H_2 - q^2 \mathrm{Im}\, H_1\right]. \tag{14.27}$$

So far this is just kinematics. Now Schwinger supposed the amplitudes $H_{1,2}$ could be written in double spectral form, that is, as a dispersion relation in the two variables $(q+p)^2$ and $(q-p)^2$. Thus, he wrote what we might refer to as a Mandelstam representation[45]

$$H_i(q^2, \nu) = \int \frac{dM_+^2}{M_+^2} \frac{dM_-^2}{M_-^2} \frac{2h_i(M_+^2, M_-^2)}{[(p+q)^2 + M_+^2][(p-q)^2 + M_-^2]}. \tag{14.28}$$

'The term "deep inelastic scattering" refers to the regime in which both $2\nu/m$ and $q^2/m^2$ are large, in such a way that the ratio

$$\omega = \frac{2m\nu}{q^2} \tag{14.29}$$

has any value in excess of unity. The essential experimental observations about this region are the following. For both the proton and the neutron, the cross section ratio, $\sigma_L/\sigma$, $\sigma = \sigma_L + \sigma_T$, is quite small, within relatively large experimental errors; the scaling behavior that is characteristic of the region is expressed by

$$\sigma = \frac{4\pi\alpha}{q^2} f(\omega), \tag{14.30}$$

where $f(\omega)$ approaches a constant $\sim 1$ for sufficiently large $\omega$, and vanishes as $\omega \to 1$ in a manner not inconsistent with that of $(\omega - 1)^3$; the ratio $f_n(\omega)/f_p(\omega)$ decreases from unity at large $\omega$ to a somewhat uncertain limit as $\omega \to 1$.' [167] (Note that the Bjorken–Feynman $x$ variable is merely $1/\omega$.) Schwinger was able to show that these features followed from the double spectral representation by making some rather general assumptions. Thus he concluded, 'We have now seen that the general characteristics of deep inelastic scattering emerge as a reasonable interpolation between the known properties of the low energy resonance region and of the high energy diffractive region. And in doing this we have shunned the widespread practice of hanging such phenomenological correlations on the scaffolding of some speculative dynamical model. It is thereby emphasized that whatever understanding has been achieved cannot be adduced as evidence in favor of a particular model.' [167]

Schwinger ended this first paper with a caution. He noted that, at least in diffractive scattering, the spectral masses squared, $M_+^2$ and $M_-^2$, might be large and *negative.* This posed 'the danger of serious conceptual problems caused by negative $M^2$.' Indeed, in a later work by five of his former students, Richard Ivanetich, Tsai, DeRaad, Milton, and Luis Urrutia, published in the Schwinger *Festschrift,* this concern was realized: 'But finally, is not the physical intuition of the source theorist disturbed by the appearance of negative spectral masses in the 'anomalous' situation? Especially so, as the causal region lies completely outside the spectral support.'[46] Schwinger was not discouraged by this result, but wrote an enthusiastic note to his junior associates: 'Your results do not disprove DSF [double spectral form] as originally used, but recover it with the spectrum problem removed.' He went on to note that the spectral restriction of the model is, however, violated.[1]

The second paper in this series was entitled 'Source theory discussion of deep inelastic scattering with polarized particles' [168], which extended the analysis to the situation in which the target nucleus was spin polarized. For a nucleon of momentum $p^\mu$, this was described by a spin vector $s^\mu$ satisfying

$$p^\mu s_\mu = 0. \tag{14.31}$$

Consequently, there were two additional tensors describing the virtual forward Compton scattering amplitude,

$$T_{3\mu\nu} = -2m^3 \mathrm{i}\epsilon_{\mu\nu\kappa\lambda} q^\kappa s^\lambda, \quad q^\mu T_{3\mu\nu} = 0, \tag{14.32}$$

and

$$T_{4\mu\nu} = m(qs)\mathrm{i}\epsilon_{\mu\nu\kappa\lambda} q^\kappa p^\lambda, \quad q^\mu T_{4\mu\nu} = p^\mu T_{4\mu\nu} = 0. \tag{14.33}$$

Correspondingly, there were a total of four amplitudes describing deep inelastic scattering off polarized nuclei. The most interesting aspect of this paper was that two new sum rules were derived. These were

$$\int_{>m^2}^{\infty} \mathrm{d}M_+^2 \frac{1}{\pi} \mathrm{Im}\, H_4 = -\frac{F_2(q^2)[F_1(q^2) + F_2(q^2)]}{4m^2}, \tag{14.34}$$

where $F_1$ and $F_2$ are the electric and magnetic form factors of the nucleon, respectively; and

$$\frac{2}{\pi} \int_{\nu'>0}^{\infty} \frac{\mathrm{d}\nu'}{\nu'} \mathrm{Im}\, H_3(0, -m\nu') = \frac{l\mu_a}{8m^4}, \tag{14.35}$$

where $l$ is the electric charge of the nucleon (in units of the electronic charge), and $\mu_a$ is the anomalous magnetic moment of the nucleon. These two sum rules together imply the known Gerasimov–Drell–Hearn sum rule.[47] They were verified to order $\alpha$ in electrodynamics, independently, by Tsai, DeRaad, and Milton,[48] and remain of some interest.[49] (Incidentally, note that the second sum rule provides another way to calculate the anomalous magnetic moment of the electron.)

In 'Source theory analysis of electron–positron annihilation experiments' [173] the same formalism was applied to the situation where the virtual photon is time-like, rather than space-like. The results were rather preliminary, and did not seem to lead to much new insight. Historically, this paper was of interest because it represented Schwinger's response to what he saw as the crisis precipitated by the unexpected $e^+e^-$ events: not just the $\psi$ particles, but the earlier observed rise in the cross-section, now associated with the crossing of new quark thresholds.

The series continued with 'Deep inelastic scattering of leptons' [178]. In this paper, Schwinger incorporated numerical improvements and showed graphs detailing quite impressive agreement between phenomenologically derived form factors (the functions $f$ referred to above) and the experimental data. This paper further treated the deep inelastic scattering of neutrinos off nucleons. (The formalism for that case was set up by Schwinger's postdocs.[50]) Then in 'Deep inelastic scattering of charged leptons' [179] the longitudinal cross-section, which previously had been neglected as small, was treated. More important, the polarization asymmetry, defined by

$$P = \frac{\sigma_+ - \sigma_-}{\sigma_+ + \sigma_-}, \tag{14.36}$$

where the subscripts $\pm$ refer to the photon helicity and the nucleon spin being parallel or antiparallel, respectively, which had first been treated in [168], was given a modified treatment.

Schwinger, in 'Adler's sum rule in source theory' [181], provided his formulation of this important sum rule referring to deep inelastic neutrino–nucleon scattering,

$$\frac{1}{2\pi}\int_1^\infty \frac{d\omega}{\omega}\left(f_2^{\nu n} - f_2^{\nu p}\right)(\omega, q^2) = 1, \tag{14.37}$$

first derived by Stephen Adler,[51] where

$$f_2(\omega, q^2) = 2m\nu q^2 \mathrm{Im}\, H_2(q^2, \nu), \tag{14.38}$$

and the superscripts refer to neutrino–neutron and neutrino–proton scattering, respectively. He first verified the sum rule using form factors phenomenologically extracted using his formalism, and then provided an independent source theory derivation.

The final paper in the sequence dealt with 'Deep inelastic sum rules in source theory' [183]. After the success in rederiving Adler's sum rule for neutrino scattering, 'now, inevitably, we must ask about the status of the other deep inelastic sum rules, notably the Gross–Llewellyn-Smith sum rule for neutrino scattering on average nucleons[52] and Bjorken's sum rule for scattering of polarized electrons on polarized nucleons.[53] Is it possible to supply new derivations that will remove the implication that these sum rules are consequences of, and constitute tests for the validity of, speculative hypotheses concerning inner hadronic structure? Our affirmative response is given below. Then, armed with these additional constraints on deep inelastic scattering structure functions, we reconsider and improve earlier source theoretic estimates for neutrino scattering and polarized electron scattering.' [183]

In the mid-1970s Schwinger had ongoing correspondence with Vernon Hughes concerning this subject.[1] On 11 January 1977 Hughes wrote to say that radiative corrections appeared to be important to understanding deep inelastic scattering, and Schwinger wrote back on the 31st: 'My ideas continue to evolve. After a long period of disbelieving in the Bj [Bjorken] sum rule, I have now derived it to my own satisfaction, which forces revision of my results. This is described in the accompanying paper [183]. In particular, I direct your attention to the crude but not misleading formula [Eqn 116] $A/D \sim \omega^{-1/2} = x^{1/2}$ which roughly fits the trend of the data in your Fig. 5.

'As for experimental suggestions, while I've always emphasized the importance of large $\omega$, I must concede that $\omega \to 1$ is also of great importance. Concerning radiative corrections, do you have someone at work on it? I have mentioned the problem to my group, but they haven't gotten around to it yet. Best wishes, J.S.'

## Schwinger's reaction to quarks and partons

As the reader may easily discern from Schwinger's remarks motivating his phenomenological study of deep inelastic scattering, as well as in his proposals for the significance of the $\psi$ resonances discussed on pp. 493–496, Schwinger consistently had a profound antipathy toward the quark model and other speculations concerning substructure of nucleons. Why was this? Certainly, a component was the deep mutual antagonism between Murray Gell-Mann, inventor of quarks, and Julian Schwinger.* On pp. 516–517 we will discuss Schwinger's speculative model of dyons constituting hadrons, which bears at least a superficial resemblance to the quark picture. In Schwinger's paper presenting this

* 'Gell-Mann simply got up and said, "Why do we need Schwinger's mathematics when we have— . . . ." With his usual bluntness. That was my first reaction to him. So I think in the feud that apparently now rages between us, although I never declared it, I was the initial victim . . . . But there were other interactions . . . . Gell-Mann was speaking and praising Weisskopf. He, in effect, was saying that sure Weisskopf was much the better lecturer than this fellow down at Harvard that everybody went to listen to, as though I couldn't identify myself.'[7] On the other hand, the Gell-Manns did stay in Schwinger's house in Belmont during a sabbatical in the 1963.[5] And Schwinger wrote a supportive letter comparing Gell-Mann and Roy Glauber who were being considered for a Harvard faculty position in the 1950s, an offer which was in fact made to Gell-Mann, who declined it.[1]

model in *Science* [169] he made the following scathing comment concerning the naming of these proposed constituents: 'Unfortunately, the field of choice is not free of prior incursions. In the interest of an obscure literary reference that celebrates the empirical aspect of triadism, an untraditional and unmellisonant term was introduced and has found favor in some circles. I prefer to respect tradition and, more important, to emphasize the theoretical basis of the otherwise mysterious empirical characteristics.'

More significant, though, was his conservative and fundamentally phenomenological bent. He always felt that the observable aspects of the quark model were the group-theoretical ones, a view that was nearly universal before the deep inelastic scattering results began to appear in the late 1960s and the narrow $\psi$ resonances were discovered in 1974. As we have seen, Schwinger, at least to his own satisfaction, was able to accommodate these phenomena without speculations about substructure. In particular, his double-spectral analysis of the deep inelastic data had a great deal of validity. Unfortunately, it must be admitted that the later papers in the series tended to contain too much 'curve fitting' with the introduction of arbitrary parameters. To be fair, however, QCD hardly does better, for it can make no predictions for low energies and momentum, so structure functions must be extracted from experiment. At the present stage of development, one might argue that QCD remains but a useful parameterization of strong interaction physics. Perhaps if Schwinger had not been so confrontational in his presentation, his approach, which certainly contained much that is valid and useful, would have had a greater impact on the field. It may be that this impact may yet be realized, for strong interaction physics must still be recognized as an immature science.

Further insight may be gleaned from a very provocative lecture Schwinger presented at a number of places beginning in 1977. The first version, entitled 'Conflicts in physics,' was given on the occasion of the 500th anniversary of the founding of the University of Tübingen. Schwinger's 'honorary student' Walter Dittrich recalled 'that we had to move to the largest hall available. People came from all over to hear him; even Res Jost from the ETH Zurich' and Sir Nevill Mott were in attendance. 'I think Julian's speech was his first public reaction to the non-acceptance of Source Theory by the high energy community.'[54] Much the same material was presented later that year (on 5 November 1977) in honor of the 50th anniversary of the Pupin Laboratories at Columbia University, and of the contributions of I. I. Rabi. (This later version was called 'Physics of the future—a view from the past,' and is the version quoted by Schweber[55]). Subsequently, it was presented as a Sigma Xi lecture at UCLA, and at a number of other places. In these lectures he recounts the little-known history of Herapath and Waterston, nineteenth century scientists whose work on kinetic theory was effectively suppressed by the establishment.* Although they were on the side of atomism, which was ultimately to prevail, and their work was eventually rehabilitated, with an apology, by Lord Rayleigh, 'one of my heroes,' he used the example as a call for freedom from dogmatism, in the 'light of the present-day situation in high energy physics.' In particular, he saw his own work on deep inelastic scattering to be in the spirit of 'J. Willard Gibbs: While not questioning the ultimate emergence of inner structure, refrain as far as possible from making specific, speculative hypotheses about that structure.'[1] Although Schwinger submitted this talk for publication in the popular journal *The Sciences*, an organ of the New York Academy of Sciences, it was rejected by editor Robert N. Ubell, who wanted an 8–12 page article on the rise of atomism and parallels with current high-energy physics. Schwinger responded unhappily. 'My first rejection slip. [sic] I am disappointed that you found my historical anecdotes of so little interest; that was not the reaction of two very different audiences who heard this talk. While I understand your preference for an extended development of the parallels between 19th century atomism and modern HEP, I simply do not have the time to go into it further.'[1]

Saxon offers yet another interpretation of his self-imposed isolation: 'The attributes that made him so powerful and strong were the same ones that led to his gradual slipping out of the mainstream. He had to do everything himself. Then it became a point of pride. Feynman did diagrams, and Julian understood them and knew how to use them, but he was not going to do any problems using Feynman diagrams. And he didn't discover a faster way. And Dick's way did give people tools that did permit them to go ahead quicker.'[6] Of course, the same applied to Feynman's partons.

## Source theory and general relativity

We recall that it was the difficulty of consistently quantizing the gravitational field that first led Schwinger to question the efficacy of operator quantum field theory. So it was no surprise that one of the first applications of source theory

* An article by Lord Zuckerman[56] was a partial source for the Herapath and Waterston story.[1] Further sources are revealed in the Rabi symposium version: 'The historical episodes discussed here were initially presented in a lecture entitled 'Conflicts in Physics' which was delivered in Tübingen on the occasion of the 500th anniversary of the founding of that University. The problem facing me then, of bolstering a general memory of those events with precise citations, was happily solved with the timely publication by Stephen G. Brush of *The Kind of Motion We Call Heat*, Book I, North-Holland Publishing Co., 1976, a copy of which I finally located in Oxford, on my way to Tübingen. Any reader who has been intrigued by my short account should delve into the wealth of detail contained in this excellent book.'[1]

was to the gravitational field. In 'Sources and gravitons' [146] he showed that the hypothesis that the gravitational force is mediated by a spin-2 (properly, helicity-2) massless particle, the graviton, inexorably implies, in the classical limit, the Einsteinian theory of gravity, general relativity.[57] In particular, with minimal formalism, Schwinger was able to derive the four classic tests of general relativity: the gravitational redshift, the deflection of light by the Sun, the time delay of radar echoes passing near the Sun, and the perihelion precession of Mercury.

Schwinger set the stage by telling a parable: 'The graviton is unknown, as yet, to experimental science. Nevertheless, we shall accept it and its conjectured properties as the proper starting point for the theory of gravitational phenomena, just as the photon with its attributes initiates the theory of electromagnetic phenomena. The evidence for the existence of the graviton is indirect, but impressive. To indicate its nature we present the following parable: "The laws of quantum mechanics and relativity have been well established, but the interaction properties of electric charges are known only under quasi-static conditions. Two physicists, Max Stone and Ichirō Ido,* point out that all such properties would follow from the postulated existence of a certain particle, on using the principles of source theory. They predict that the particle will one day be discovered. Others dismiss this suggestion as unwarranted speculation. The issue remains undecided."' [153]

The exchange of a massless, helicity-2 graviton between its sources, whatever their arrangement, is given by the vacuum persistence amplitude

$$\langle 0_+ | 0_- \rangle_T = \mathrm{e}^{\mathrm{i}W[T]}, \tag{14.39}$$

where

$$\begin{aligned} W[T] = \frac{1}{2}\int (\mathrm{d}x)(\mathrm{d}x')[&T^{\mu\nu}(x)D_+(x-x')T_{\mu\nu}(x') \\ &- \frac{1}{2}T(x)D_+(x-x')T(x')], \end{aligned} \tag{14.40}$$

where $D_+$ is the propagation function for a massless particle and $T^{\mu\nu}$ is the source tensor for the graviton, which is necessarily conserved and symmetrical,

$$\partial_\mu T^{\mu\nu} = 0, \qquad T^{\mu\nu} = T^{\nu\mu}. \tag{14.41}$$

The term involving the trace of $T^{\mu\nu}$, $T = T^{\mu}_{\mu}$, is present in the vacuum persistence amplitude so that there are only two helicity states contributing, corresponding to the spin projection along the direction of motion of $\pm 2$. The

* The words *ichirō ido* roughly translate as 'one well.'

source here is an idealization of a production process for the graviton. In the classical regime, which is all that is at present accessible, it may be identified with the mechanical energy–momentum, or stress tensor, $t^{\mu\nu}$,

$$T^{\mu\nu} = \sqrt{\kappa}\, t^{\mu\nu}, \tag{14.42}$$

where $\kappa/8\pi = G$ is Newton's gravitational constant,

$$G = 6.67 \times 10^{-8}\mathrm{cm}^3/\mathrm{g\,s}^2 = (1.62 \times 10^{-33}\ \mathrm{cm})^2, \tag{14.43}$$

the latter form referring to atomic units, where $\hbar = c = 1$.

From the above form of the vacuum-to-vacuum amplitude, we immediately deduce the form of the energy between quasistatic energy–momentum distributions,

$$E = -G\int(\mathrm{dx})(\mathrm{dx}')\left[t^{\mu\nu}(\mathbf{x}, x^0)\frac{1}{|\mathbf{x}-\mathbf{x}'|}t_{\mu\nu}(\mathbf{x}', x^0) - \frac{1}{2}t(\mathbf{x}, x^0)\frac{1}{|\mathbf{x}-\mathbf{x}'|}t(\mathbf{x}', x^0)\right]. \tag{14.44}$$

From this follows immediately Newton's law of gravitation, and with just a tiny bit of work, the four empirical tests of general relativity mentioned above. It is only the last one, the perihelion precession, that requires a slight transcending of the linear approximation. One has to include the 'contribution to the energy density $t^{00}$ that is associated with the gravitational interaction between the planet and the sun.' The result is precisely Einstein's value for the perihelion precession angle, $\Delta\phi$,

$$\Delta\phi = 24\pi^3\left(\frac{a}{T}\right)^2(1-e^2)^{-1}, \tag{14.45}$$

where $a$ is the semimajor axis of the orbit, $T$ is the planet's period of revolution, and $e$ is the eccentricity of the elliptical orbit.

Schwinger went on to derive full Einsteinian general relativity by treating the theory as a gauge theory. That is, he defined a gravitational field $h_{\mu\nu}$ from the functional $W[T]$ as the response to a small change in the source,

$$\delta W[T] = \int(\mathrm{d}x)\delta t^{\mu\nu}(x)h_{\mu\nu}(x), \tag{14.46}$$

so since the sources must remain conserved, $\partial_\mu\delta t^{\mu\nu} = 0$, he could see that the field $h_{\mu\nu}$,

$$h_{\mu\nu}(x) = \kappa\int(\mathrm{d}x')D_+(x-x')\left[t_{\mu\nu}(x') - \frac{1}{2}g_{\mu\nu}t(x')\right], \tag{14.47}$$

was ambiguous up to a redefinition, a gravitational gauge transformation,

$$h_{\mu\nu} \rightarrow h_{\mu\nu} + \partial_\mu \xi_\nu + \partial_\nu \xi_\mu, \tag{14.48}$$

where $\xi_\mu$ is an arbitrary vector function. The functional $W[T]$ could now be written in action form,

$$W = \int (\mathrm{d}x)[t^{\mu\nu} h_{\mu\nu} + \mathcal{L}(h, \Gamma)], \tag{14.49}$$

where the Lagrange density is

$$\kappa\mathcal{L}(h, \Gamma) = -\left(h^{\mu\nu} - \frac{1}{2} g^{\mu\nu} h\right)(\partial^\lambda \Gamma_{\mu\nu\lambda} - \partial_\nu \Gamma_\mu) - \frac{1}{2}(\Gamma^{\lambda\mu\nu}\Gamma_{\mu\nu\lambda} - {}^\lambda\Gamma\Gamma_\lambda). \tag{14.50}$$

Here $h_{\mu\nu}$ and $\Gamma_{\mu\nu\lambda}$ were regarded as independent fields, $h = h^\mu_\mu$, and

$$\Gamma_\mu = \Gamma_{\mu\lambda}{}^\lambda, \quad {}^\lambda\Gamma = \Gamma^\nu{}_\nu{}^\lambda, \quad \Gamma_{\mu\nu\lambda} = \Gamma_{\nu\mu\lambda}. \tag{14.51}$$

The field $h_{\mu\nu}$ is the analog of the electromagnetic vector potential, and $\Gamma_{\mu\nu\lambda}$ the analog of the field strength tensor. Under a gauge transformation the latter field transforms as

$$\Gamma_{\mu\nu\lambda} \rightarrow \Gamma_{\mu\nu\lambda} + 2\partial_\mu\partial_\nu\xi_\lambda. \tag{14.52}$$

Under the transformations (14.48) and (14.52), the free action is invariant.

When gravity is coupled to matter, by replacing the abstract source by the mechanical stress tensor $t_{\mu\nu}$, the interaction term is not gauge invariant,

$$\delta_{\text{gauge}} W = \int (\mathrm{d}x) t^{\mu\nu}(x) 2\partial_\mu \delta\xi_\nu(x), \tag{14.53}$$

but we note that this is compensated by an infinitesimal coordinate transformation (the reader may recall that energy–momentum conservation is due to the invariance of the theory under rigid space–time translations)

$$\delta_{\text{coord}} W = -\int (\mathrm{d}x) t^{\mu\nu}(x) \partial_\mu \delta x_\nu(x), \tag{14.54}$$

which, in fact, serves to define the energy–momentum tensor. Equations (14.53) and (14.54) cancel, provided we identify the infinitesimal parameters,

$$2\delta\xi_\nu(x) = \delta x_\nu(x). \tag{14.55}$$

This is precisely analogous to the identification of phase transformations and gauge transformations in electrodynamics. So the symmetry we are about to 'gauge' is that of general coordinate invariance.

It is then straightforward to show that matter Lagrangian invariant under arbitrary coordinate transformations is (a scalar field is used as an example)

$$\mathcal{L}(\phi, g) = -(-g(x))^{1/2}\frac{1}{2}[\partial_\mu\phi(x)g^{\mu\nu}(x)\partial_\nu(x) + m^2\phi^2(x)], \tag{14.56}$$

where the metric tensor has absorbed the gravitational field,

$$g^{\mu\nu}(x) = g^{\mu\nu} - 2h^{\mu\nu}(x), \quad g_{\mu\lambda}(x)g^{\lambda\nu}(x) = \delta^\nu_\mu, \quad g(x) = \det g_{\mu\nu}(x). \tag{14.57}$$

The correspondingly generalized gravitational Lagrangian is the Einstein–Hilbert one,

$$2\kappa\mathcal{L}(g, \Gamma) = (-g(x))^{1/2}g^{\mu\nu}(x)R_{\mu\nu}(x), \tag{14.58}$$

with

$$R_{\mu\nu} = \partial_\lambda\Gamma^\lambda_{\mu\nu} - \partial_\nu\Gamma^\lambda_{\mu\lambda} + \Gamma^\lambda_{\mu\nu}\Gamma^\kappa_{\lambda\kappa} - \Gamma^\lambda_{\mu\kappa}\Gamma^\kappa_{\nu\lambda}. \tag{14.59}$$

The field equation deduced from the action composed by adding the matter and the field Lagrangians, Eqns (14.56) and (14.58), is Einstein's gravitational field equation,

$$R_{\mu\nu} = \kappa\left(t_{\mu\nu} - \frac{1}{2}g_{\mu\nu}t\right), \tag{14.60}$$

This whole development was presented in detail in the last section of the first volume of *Particles, sources, and fields* [153]. Schwinger then went on to discuss conformal invariance, which led to a variant of the scalar-tensor theory, usually associated with the names of C. Brans and Robert Dicke.[58] Half-integer spin particles were coupled to gravity by introducing tetrad or vierbein fields. In short, Section 3-17 of [153] was a concise (28 pages), readable, and self-contained primer in classical gravitational theory, that is, general relativity, developed in a quantum-mechanical context, but presented without the usual geometrical picture, which was therefore seen to have no empirical content.

Schwinger submitted two pedagogical notes on this development to the *American Journal of Physics* in June 1973. (These were originally partially rejected. The referee's quibbles had to do with uniqueness, etc. Schwinger replied as follows: 'I hope these remarks indicate why I do not, in your excellent phrase, "find this referee's comments useful," and I am therefore resubmitting these two notes "as is."'[1]) They were concerned with tests of general relativity, going beyond the four classic tests, but which again could be derived simply from the source theory formalism presented above. Schwinger was motivated to pursue this investigation through conversations with Kenneth MacKenzie at UCLA, who at the time was involved with supplying heuristic derivations of relativistic phenomena.[59] 'Precession tests of general relativity—source theory derivation' [162] derives the Thirring and Schiff effects. These were derived from the quasistatic energy expression (14.44). 'It is familiar that a rotating uniformly charged spherical shell produces a constant magnetic field in its interior,'

$$\mathrm{H} = \frac{2}{3}\frac{Q}{4\pi R}\,\omega, \tag{14.61}$$

where $Q$ is the charge on the shell, $R$ is its radius, and $\omega$ is the angular velocity of rotation of the shell, which is equivalent to a frame rotating with angular velocity

$$\omega_{\text{prec}} = -\frac{1}{3}\frac{e}{m}\frac{Q}{4\pi R}\,\boldsymbol{\omega} \tag{14.62}$$

in which a particle of mass $m$ and charge $e$ moves without acceleration. Analogously, inside a shell of mass $M$ and radius $R$ rotating with angular velocity $\omega$, an inertial frame precesses with angular velocity

$$\omega_{\text{prec}} = \frac{4}{3}\frac{GM}{R}\,\boldsymbol{\omega}. \tag{14.63}$$

This is the Thirring effect.[60] 'The Schiff effect refers to the precession of a gyroscope carried aboard a satellite in orbit about a planet.' (This experiment may at last be on the verge of realization.) Schwinger combined spin–spin (that is, the effect of the spin of the Earth on the spin of the gyroscope) and spin–orbit couplings, together with the Thomas precession[61] (for which he supplied an independent derivation in the following paper [163]) to derive the Schiff precession rate,[62]

$$\boldsymbol{\Omega} = \frac{3}{2}\frac{GM}{r^3}\mathrm{r}\times\mathrm{v} + \frac{GI}{r^5}(3\mathrm{r}\,\boldsymbol{\omega}\cdot\mathrm{r} - \boldsymbol{\omega}r^2), \tag{14.64}$$

where r and v are the position vector and the velocity of the satellite relative to the center of the Earth, $M$ is the mass of the Earth, $I$ is the moment of inertia of the gyroscope (assumed spherically symmetric), and $\boldsymbol{\omega}$ is the angular velocity of rotation of the Earth. (The Lense–Thirring effect, referring to the effect of the spin of the sun on the orbital motion of a planet going around it, was analogously derived by Milton in the same journal.[63])

Schwinger's final journal contribution to graviton physics was a short paper he submitted to the annual competition sponsored by the Gravity Research Foundation, 'Gravitons and photons: the methodological unification of source theory' [177] which summarized the results in [146]. It received the second prize, and $500, from the Gravity Research Foundation for 1975.[1] (The first prize went to Roger Penrose, for an article on 'The non-linear graviton.')

Finally, we should mention Schwinger's involvement with the BBC and the Open University. The Open University, based in Milton Keynes, England, offers correspondence degrees to British subjects. Many of these courses consist in part of lectures and television and radio programs delivered by the BBC. Schwinger, through astronomer George Abell at UCLA, was invited to design a series of programs on relativity, special and general, at a fairly popular level, entitled 'Understanding space and time.' From 1976 through 1979 Schwinger spent several months, on and off, in England working on these programs, and staff from the BBC, particularly producer/director Ian Rosenbloom visited California on several occasions, for a total of 18 months. The executive producer, Andrew Crilly, also came on a number of occasions. There were something like 16 programs in all, of which Schwinger did six or eight. Programs were filmed at sites ranging from Southern California to Germany. Rosenbloom enjoyed playing tennis (in exchange for explanations of physics) 'at the crack of dawn' at UCLA with Schwinger, whom he recalled had an 'awkward stance,' but was a much more challenging player than he first appeared to be, displaying 'tenacity and absolute perfectionism.' He remembered Schwinger once played with Wimbledon champion Tony Trabert, but Schwinger was so nervous he was unable to hit the ball over the net. Rosenbloom also remembered Clarice sent £5 notes to him in England to buy Bendick's 'Sporting' and 'Military' chocolates, which could not be purchased in the US, with instructions on which were the best shops to buy them in. In return, Julian sent him home with bottles of wine from his winery.[64] (We will describe Schwinger's winery venture in Chapter 16.)

In Rosenbloom's words, Schwinger was a 'man of terrific integrity, extremely fair and considerate, with a very dry sense of humor, articulate and erudite.' When Clarice and Julian invited Rosenbloom, a young bachelor at the time, to dinner, they were 'very parental and unbelievably kind.' However, Schwinger was not the ideal 'presenter,' particularly since 'at the time there was a big successful television series presented by Bronowski.' He was 'very shy,' and although he 'brought very, very imaginative ways of dealing with extremely difficult subject areas' to the programs, he was 'not God's gift to presenting on television; he should have been himself. What TV does not do well is deal with concepts. Julian was excellent at turning concepts into bite-sized chunks that people could digest. He could do that one to one, but he could not quite achieve that on the camera.'[64]

Ultimately these programs were aired in the United Kingdom ('not at prime time, but at a good time on BBC2'[64]) , and intermittently, in the United States. (Unfortunately, they appeared about the same time as Carl Sagan's *Cosmos* series, so they were not broadcast widely.* Even in Los Angeles they were aired only in early morning— no surprise since it was the local PBS station, KCET, that produced *Cosmos*.) The films formed the basis of an extension course at UCLA, hosted by Abell and Schwinger, that reached 700 people. The programs further provided the material for Schwinger's UCLA Faculty Research Lecture, 'Relativity and the common understanding,' which he delivered in November 1979. (A later version was given in Germany, on the occasion of his receipt of a Humboldt Award in 1981, and was dedicated to Alexander von Humboldt.[1]) Eventually, Schwinger turned his six programs into a popular book, *Einstein's legacy* [207], published as part of the *Scientific American Library*. Schwinger was quite pleased with his efforts (although he had been frustrated by his presentation[64]), and was proud to display his souvenir of that episode, a man-sized robot called Robie, in his living room.† However, he was disappointed with the paucity of the outcome, in view of the tremendous effort he injected into the project.[65, 66]

## Magnetic charge and dyons

Schwinger's last papers before his source theory revolution were on magnetic charge. So, it was no surprise that immediately after dealing with gravitation in the new context, he should return to this subject. 'Sources and magnetic charge' [147] was submitted in April 1968. There he introduced sources for electric and magnetic charge on the same footing, so that Maxwell's equations became (in rationalized units)

$$\nabla \times \mathrm{H} = \frac{\partial}{\partial t}\mathrm{E} + \mathrm{J}, \qquad \nabla \cdot \mathrm{E} = \rho, \tag{14.65}$$

$$-\nabla \times \mathrm{E} = \frac{\partial}{\partial t}\mathrm{H} + {}^*\mathrm{J}, \qquad \nabla \cdot \mathrm{H} = {}^*\rho, \tag{14.66}$$

where $^*\mathrm{J}$ and $^*\rho$ are the magnetic current and charge densities. (The * signifies 'dual.') The modified Maxwell equations, the same as we wrote down in Eqns (4.3) and (4.4), (or in Eqns (11.23)–(11.26)), exhibit the duality symmetry between electric and magnetic quantities,

$$\text{electric} \to \text{magnetic}, \qquad \text{magnetic} \to -\text{electric}. \tag{14.67}$$

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* Media success is an ephemeral event. Victor Weisskopf had a quite popular series of programs on public television in Boston, aired at an early evening hour, in which he discussed doing physics, going for walks, and listening to Schubert. He later saw in an airline magazine an advertisement in the personal column from a woman looking for a cultured man, 'somewhat like Weisskopf only younger.' When Schwinger heard this story, he laughed and said , 'Ah, she doesn't understand, it takes age to get that way.'[18]

† Apparently the robot was designed for a 'future project that never materialized.'[64]

Because the divergence of H is no longer zero, it is no longer strictly possible to introduce a vector potential, which seems a necessity in formulating quantum mechanics. However, it is possible to do so almost everywhere, so that the vector potential is singular along a one-dimensional continuum—a line, or *string*. A symmetrical way to do this is to introduce electric and magnetic vector potentials as follows,

$$\mathrm{A}(\mathrm{r}, t) = \boldsymbol{\nabla}\lambda(\mathrm{r}, t) - \int (\mathrm{dr}')\mathrm{f}(\mathrm{r} - \mathrm{r}') \times \mathrm{H}(\mathrm{r}', t), \tag{14.68}$$

$$^*\mathrm{A}(\mathrm{r}, t) = \boldsymbol{\nabla}^*\lambda(\mathrm{r}, t) + \int (\mathrm{dr}')^*\mathrm{f}(\mathrm{r} - \mathrm{r}') \times \mathrm{E}(\mathrm{r}', t), \tag{14.69}$$

where

$$\lambda(\mathrm{r}, t) = \int (\mathrm{dr}')\mathrm{f}(\mathrm{r} - \mathrm{r}') \cdot \mathrm{A}(\mathrm{r}', t), \tag{14.70}$$

$$^*\lambda(\mathrm{r}, t) = \int (\mathrm{dr}')^*\mathrm{f}(\mathrm{r} - \mathrm{r}') \cdot {}^*\mathrm{A}(\mathrm{r}', t). \tag{14.71}$$

Here the functions f and *f represent the strings, and satisfy

$$\boldsymbol{\nabla} \cdot {}^{(*)}\mathrm{f}(\mathrm{r} - \mathrm{r}') = \delta(\mathrm{r} - \mathrm{r}'), \tag{14.72}$$

the solution being a line integral,

$$\mathrm{f}(\mathrm{r}) = \int_C \mathrm{dx}\, \delta(\mathrm{r} - \mathrm{x}), \tag{14.73}$$

where $C$ is any contour starting from the origin and extending to infinity. Schwinger, in [147], used an equivalent relativistic formulation, where it is easy to see that the two strings are not independent, but satisfy

$$^*\mathrm{f}(\mathrm{r}) = -\mathrm{f}(-\mathrm{r}). \tag{14.74}$$

The problem is that the string must be unobservable, since its introduction is arbitrary. Changing the orientation of the string is in a certain sense just a gauge transformation. But, in fact, in general, the action depends on $f$. Switching to the relativistic notation, we can write the electromagnetic action as

$$\begin{aligned} W &= \int (\mathrm{d}x) \left[ J^\mu A_\mu + {}^*J^{\mu *}A_\mu - \frac{1}{2}F^{\mu\nu}(\partial_\mu A_\nu - \partial_\nu A_\mu) + \frac{1}{4}F^{\mu\nu}F_{\mu\nu} \right] \\ &= \int (\mathrm{d}x) \left[ J^\mu A_\mu + {}^*J^{\mu *}A_\mu - \frac{1}{2}{}^*F^{\mu\nu}(\partial_\mu {}^*A_\nu - \partial_\nu {}^*A_\mu) + \frac{1}{4}{}^*F^{\mu\nu *}F_{\mu\nu} \right], \end{aligned} \tag{14.75}$$

where in the first form, $^*A$ is defined in terms of $f$,

$$^*A_\mu(x) = \int (\mathrm{d}x')f^\nu(x-x')^*F_{\mu\nu}(x'), \tag{14.76}$$

and in the second, $A$ is so defined,

$$A_\mu(x) = -\int (\mathrm{d}x')f^\nu(x-x')F_{\mu\nu}(x'), \tag{14.77}$$

(which are just the relativistic forms of Eqns (14.68) and (14.69)). The solution to this problem had been found by Dirac in 1931.[67] The only way quantum mechanics is consistent with the existence of a magnetic monopole, so that reorienting the string is truly an unobservable gauge transformation, is for magnetic charge to be discretely distributed, and for its value to be quantized in terms of the unit of electric charge $e$. This is the famous Dirac quantization condition, for a monopole of strength $g$,

$$\frac{eg}{4\pi} = n, \qquad n = 0, \pm 1, \pm 2, \ldots, \tag{14.78}$$

or possibly $n$ may be multiplied by $\frac{1}{2}$, see below. An elementary, semiclassical way of understanding this simple result is in terms of angular momentum. Consider a static system consisting of an electric charge $e$ and a magnetic charge $g$, separated by a distance $\mathbf{r}$. The resulting static electromagnetic field carries a classical angular momentum

$$\int (\mathrm{d}\mathbf{r})\mathbf{r} \times (\mathbf{E} \times \mathbf{H}) = \frac{eg}{4\pi}\frac{\mathbf{r}}{r}. \tag{14.79}$$

Now, appealing to the quantization condition that any component of angular momentum must be an integer, or an integer plus $\frac{1}{2}$, times $\hbar$ ($=1$ here), we get the Dirac condition (14.78).

Schwinger generalized the quantization condition to the case where both particles, labeled $a$ and $b$, carry electric and magnetic charges, $e_a$, $g_a$, and $e_b$, $g_b$, respectively:

$$e_a g_b - e_b g_a = 2\pi n, \qquad n = \text{integer}. \tag{14.80}$$

He also briefly introduced his model of hadrons as being strongly bound states of electrically and magnetically charged constituents. The simplest possibility for the smallest electric charge for the components is $e_0 = \frac{1}{3}$! This picture was elaborated in an article he submitted to *Science* the following year, 1969, entitled 'A magnetic model of matter' [150], 'based on lectures delivered over several years, most recently at Lindau, Germany,* in July 1968.' After reviewing

* This was a regular meeting of Nobel Laureates.

the theory of magnetic charge simply, he presented his model of hadrons as bound states of dyons, 'dual-charged particles,' that is, particles carrying both electric and magnetic charge. Here is his account of the origin of this name: 'As evidenced by the use of the provisional phrase 'dual-charged particle,' the basic aspect that should be commemorated in the name is the dualistic or dyadic character of the charge that the particle bears. There are various short Greek and Latin combining forms that could be applied: *bi-*, *di-*, *duo-*, *dyo-*, as well as longer words such as *dyadikos-*, of two. Dyadikon surely has a ring to it. But being mindful that mesotron became shortened to meson, I believe that dyon is a better choice.' Later Schwinger summarized his view of the role of dyons: 'To me, the quarks are dyons.'[7]

Schwinger then proceeded to make elementary, vastly oversimplified estimates of the binding energies and masses of these dyon constituents, which are no longer of much interest. He also appealed to a mechanism of very rapid magnetic charge exchange in order to suppress otherwise strong *CP* violation, which would be manifested as a large neutron electric dipole moment. Although Schwinger did not intend the details of this model to be taken too literally, given his inability to deal with the extraordinarily strong forces involved, the concept remains provocative. He bracketed this paper between famous quotations by two of his heroes, opening with Newton's reference to magnetism, 'And now we might add something concerning a most subtle Spirit, which pervades and lies hid in all gross bodies,'[68] and closing with Faraday, 'Nothing is too wonderful to be true, if it be consistent with the laws of nature, and in such things as these, experiment is the best test of such consistency.'

Schwinger had some difficulty with the copy editor at *Science.* He wrote back, 'The copy editor has caused me to lose the few hairs remaining in my head. Some of the "minor changes" are destructive of style and content, replacing lively phrases with dull flat ones, and quite distort meaning and emphasis. Here are the ones about which I feel most strongly. I urge they be restored to their original form.' He went on with seven pages of manuscript corrections, which apparently were mostly accepted.[1]

Schwinger started another paper, similar in tone, called 'Magnetic charge: binding agent of matter.' The two typed pages concluded: 'There is only one way to test this intriguing possibility. We must build high energy machines of sufficient power to penetrate that hidden world and bring to light this most fundamental and primitive material substance.'[1] We already mentioned that Schwinger resurrected this model after the November revolution, with 'Psi particles and dyons' [169].

Schwinger next revisited magnetic charge later that year (1975), in 'Magnetic charge and the charge quantization condition' [172]. The issue here was the vexing one of whether the Dirac quantization condition gave integers, or integers $+\frac{1}{2}$. (There was also another possible factor of 2.) Although he had been of different opinions in the past, now Schwinger argued that the result depended on whether the strings, and hence the solutions, obeyed the duality symmetry of Maxwell's equation (here we follow the somewhat more transparent argument in [180]),

$$\mathrm{E} \to \mathrm{E}\cos\theta + \mathrm{H}\sin\theta, \quad \mathrm{H} \to \mathrm{H}\cos\theta - \mathrm{E}\sin\theta, \tag{14.81}$$

where $\theta$ is a constant rotation angle. If we wish the duality symmetry to hold, then, in addition to Eqn (14.74), we must have another condition on the strings,

$$^{*}\mathrm{f}(\mathbf{r}) = \mathrm{f}(\mathrm{r}), \tag{14.82}$$

which implies that the $f$ function is odd. This is the symmetrical solution. If we do not impose this latter condition, we obtain an unsymmetrical string and solution. This leads to Schwinger's, and Dirac's, quantization conditions, respectively,

$$\frac{e_a g_b - e_b g_a}{4\pi} = \begin{cases} n, & \text{symmetric case,} \\ \frac{1}{2}n, & \text{unsymmetric case.} \end{cases} \tag{14.83}$$

Schwinger concluded this paper [172] with an *Added note*, in which he excitedly referred to the report by Buford Price *et al.*[69] that magnetic charge may have been found in a highly ionizing cosmic ray event. There was a certain degree of skepticism with which this report was received, of course, as Schwinger noted in a talk he gave shortly thereafter: 'I am privileged to talk to you at a time when evidence for the existence of magnetic charge has at long last appeared and before the strident voices raised in opposition have managed to effectively discredit that evidence.'[1] However, by that summer, Luis Alvarez[70] demonstrated, by appealing to Occam's razor, that the simpler hypothesis was that this event was produced by a rare event, a heavy platinum nucleus undergoing a double fragmentation. As Schwinger wistfully stated[71] a few years later, 'If only the Price had been right.'* The Schwinger archive contains a handwritten note from Alvarez, dated 12 November 1975, attached to his paper[70]: 'If it is true—as rumor has it—that you are inclined to believe there is some evidence for a "137 monopole" [that is, $g = 137e$ if $n = 1$], I believe this report might change your mind. As you probably know, I've always felt that monopoles exist, and should have a charge of 137. I'm therefore very disappointed to conclude that there is as yet no evidence for them.'[1]

However, this experiment motivated Schwinger to re-examine the mechanism of energy loss when a (magnetically) charged particle passes through matter,

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* Schwinger often had the television on while doing physics.

which he presented in class lectures are UCLA, and which appears in *Classical electrodynamics.* [231] With the same end in mind, he further carried out a joint analysis of 'dyon–dyon scattering' in 1976 [180]. This paper treated the classical and quantum-mechanical theory of non-relativistic scattering of dyons by dyons (in particular, of an electric charge by a magnetic monopole). The classical theory, which originated with Poincaré,[72] is very beautiful, and exhibits the phenomena of 'rainbows' and 'glories,' where the cross-section becomes infinite. The quantum theory of scattering, first developed by Tamm,[73] is intimately connected to the general theory of angular momentum, which Schwinger had done so much to develop [69]. The effect of a magnetic dipole moment is also included, which is crucial in the complementary theory of bound states. The cross-sections exhibit complicated interference patterns, vaguely reflecting the classical rainbows, yet there is no classical limit. The following year, Yoichi Kazama and C. N. Yang, carried out the corresponding relativistic calculations,[74] using a different, but equivalent, formalism.*

Schwinger's last words on magnetic charge (apart from the appearance of [180], whose completion was largely the work of his collaborators) were in a letter to *Physics Today*,[75] commenting on Carl Hagen's letter, who in turn was responding to a *Physics Today* statement of October 1975, in an article about the Price monopole, that Schwinger had a consistent theory of magnetic charge. Hagen had sent a letter to *Physics Today*, dated 21 November 1975 claiming that the Dirac monopole was not covariant, and that his paper on the subject was 'conveniently ignored' by most physicists.[76] Schwinger's reply said that the source theory approach is the only consistent procedure, and that *his* work must have escaped the attention of Hagen.[1]

Schwinger's work on magnetic charge and dyons is an enduring legacy. If magnetic charge is ever found—and nowadays most 'unified' models predict it somewhere—his work will be vindicated. It is also of practical importance, for example, in the monopole search being carried out at the University of Oklahoma.[77]

## Supersymmetry; the master and his disciples

When Schwinger came to UCLA in 1971, the Physics Department committed itself to support three postdoctoral researchers for him. This arrangement carried on the concept of the 'assistant' Schwinger had had at Harvard during the 1940s and 1950s. This support was partly through the NSF grant that

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* Instead of a string, they used the concept of defining the vector potential in different coordinate patches. They also referred to 'monopole harmonics' which Schwinger *et al.* recognized as the well-known Jacobi polynomials.

the Theoretical Elementary Particles (TEP) group held, and partly through the departmental teaching budget. (This was a rather common arrangement at UCLA in those days, since most postdocs there taught at least one course a year; they covered a very large fraction of the undergraduate teaching load.) So DeRaad, Milton, and Tsai had joint titles: Assistant Research Physicists and Adjunct Assistant Professors. (Academically, the longer the title, the lower the status!) This arrangement was good for everyone for a few years.

But, in time, more permanent situations had to be found. Fortunately, Schwinger had his old friend and neighbor in Belmont, Steve White, who was then on the staff of the Alfred P. Sloan Foundation, and he suggested that Schwinger apply to them for a research grant. This was done, through a hand-written request for Sloan support, noting that 'the NSF, in its wisdom, has decided to cut the support of my little group.'[1] and what was then thought a princely sum of $100,000 was awarded for a three-year period in 1975. (An additional supplement was later added.) This replaced the NSF support for the postdocs, which gave Schwinger greater independence and freedom from rising tensions within and without the TEP group. However, this was just a stopgap arrangement, and it was clear that permanent positions for the three junior members of the group had to be found at UCLA or elsewhere.

Tsai was the first to leave. (Luis Fernando Urrutia, who had just received his PhD under Schwinger, filled Tsai's slot for six months.[1]) Tsai accepted a faculty position at the University of Miami, Coral Gables, in 1977. Unfortunately, his wife and child did not react well to the Florida climate, so after a year they returned to Southern California. There he joined DeRaad at R. and D. Associates, a Department of Defense contractor, which DeRaad had joined in 1978 (and where he works to this day). Their subsequent adventures, and Schwinger's involvement, will be chronicled in the next chapter. Milton was the only one of the three to remain in academe. Schwinger strenuously attempted to secure a regular faculty appointment for him, but understandably (particularly in view of the heavy political infighting within the group and the department) this was impossible.* The best he was able to achieve was a promise[1] by the department that as long as he was at UCLA he would have a senior postdoctoral

* One of Schwinger's opponents in the department was J. J. Sakurai. In a letter written presumably to the department chair in 1977, Schwinger declined to serve in the future on any committee with Sakurai, and wrote bitterly, 'Incontestably, it is becoming known in the outside world that my competence is being challenged by one who is generally rated as a narrow individual of limited accomplishments. As a result, UCLA is indeed becoming a "laughing stock." The MIT people extended their sympathy to the point of inviting me to join them if it became unbearable.'[1]

position, and that Milton had the first right of refusal; Milton was to leave, first for Ohio State (OSU), and then for Oklahoma (OSU and OU), in 1979.

In 1977 Schwinger used some of the Sloan money to invite Stanley Deser of Brandeis to UCLA to give his group some private lessons on supersymmetry; Deser was one of the founders of the subject, and at that time was busy developing supergravity.[78] And these really were private lessons—the talks Deser gave were closed to all but the immediate group: Schwinger, his postdocs, a graduate student or two (Urrutia certainly, and perhaps Wilcox and Wilensky attended), and Robert Finkelstein. Although this privacy did not enhance the feeling of collegiality within the department, Schwinger felt it necessary for him to come up to speed on the technicalities, and not be swamped by experts in the department—again another example of his fear of domination.

Deser remembers this command performance: 'Julian kept his strong interest in gravitation after he created source theory, in terms of which he treated, e.g., relativistic corrections to Newtonian motion. Long afterwards, when supergravity was discovered, Julian became very interested, and no wonder: Here were his old friends spin-$\frac{3}{2}$, Grassmann variables, deep gauge invariances and gravity all mixed together. Julian decided on total immersion learning for himself: I was summoned across the country to the physics seminar room at UCLA to give him a private weekend tutorial, on the all-day Russian scale; it did not surprise me that Julian was a fast learner! People have wondered why he, who was so well placed for it, never discovered supersymmetry himself; I think that (apart from the fact that you can't win 'em all) it was his anchor in experiment that kept him from this type of unification at the time. Yet, as in other fields, he had uncannily laid the groundwork for these new ideas.'[79]

Schwinger submitted a paper on the 'Multispinor basis of supersymmetry' in 1978 [190], in which he kicked himself for not thinking of the idea first. He had presented already in the first source theory paper [135], and in more detail, in the first volume of *Particles, sources, and fields* [153], a unified treatment of particles of all spins, in terms of multispinors, that is, building everything up from spin-$\frac{1}{2}$. (This is the relativistic generalization of what he had done in 1951 in his general treatment of angular momentum [69].) So it would have been quite natural to have thought of transformations which interchange bosons and fermions, differing by $\frac{1}{2}$ unit of spin. But he had not. In his introductory words to the paper, 'All right, wise guy! Then why didn't you do it first?'

In a lecture which he gave in 1978, Julian noted that the work was done a year previously, even though the preprint was dated May 1978; the delay had been caused by his work with the BBC.[1]

Schwinger treated supersymmetry as a kinematical symmetry, so he began by considering non-interacting systems. Here is a simple example. Consider a system composed of a non-interacting massless vector particle (photon) and massless fermion (neutrino),

$$\mathcal{L}_{(1,\frac{1}{2})} = -\frac{1}{2}F^{\mu\nu}(\partial_\mu A_\nu - \partial_\nu A_\mu) + \frac{1}{4}F^{\mu\nu}F_{\mu\nu} + \frac{1}{2}\psi\gamma^0\gamma^\mu\frac{1}{i}\partial_\mu\psi, \quad (14.84)$$

where, as always, Schwinger used a Majorana (real) representation for the fermion spinor $\psi$ (this was a necessity for supersymmetry, but had been used by Schwinger for years—one reason why his students were in the forefront of the supersymmetry development). This Lagrangian is in first-order form—that is, the independent variables are $A$, $F$, and $\psi$. It is now easy to see that under the fermion–boson transformation,

$$\delta\psi = \frac{i}{2}\sigma^{\mu\nu}F_{\mu\nu}\xi, \quad \delta A_\mu = i\xi\gamma^0\gamma_\mu\psi, \quad (14.85)$$

where the infinitesimal parameter $\xi$ is a constant, the above Lagrangian changes by a total derivative, and hence the action is invariant. This supersymmetry implies the existence of a conserved supercurrent,

$$j^\mu = \frac{1}{2}\sigma^{\kappa\lambda}F_{\kappa\lambda}\gamma^\mu\psi, \quad (14.86)$$

and the group properties of the transformation are given by the commutator of successive infinitesimal transformations,

$$[\delta^a, \delta^b]\psi = 2i(\xi^a\gamma^0\gamma^\alpha\xi^b)\partial_\alpha\psi \quad (14.87)$$

$$[\delta^a, \delta^b]A_\mu = 2i(\xi^a\gamma^0\gamma^\alpha\xi^b)\partial_\alpha A_\mu + \text{gauge term}, \quad (14.88)$$

that is, the commutator of two supersymmetry transformations is a space–time translation. Supersymmetry evades the no-go theorem that doomed earlier attempts to unify internal and space–time symmetries.[80] He also considered unit spin transformations, that do not change the statistics, and briefly discussed interacting systems. Unfortunately, Schwinger did not pursue supersymmetry further. Those around him did. For example, Finkelstein extended this approach to supergravity, by making the supersymmetry local,[81], and together with Milton and Urrutia, showed that local supersymmetry alone was sufficient to yield full supergravity, with no appeal to general coordinate invariance.[82]

During all these years Schwinger taught brilliant graduate and undergraduate courses in field theory (source theory) and quantum mechanics, lecturing for two hours a day, twice a week, followed by lunch with Finkelstein and the three postdocs. At first they ate at various Chinese restaurants, but then, as he became more diet conscious, at the Chatam in Westwood, where he always ordered rare roast beef. Tennis with Lester DeRaad was a regular part of his weekly regime. Several skiing trips took place each winter. Of course, he almost never came down the mountain to visit UCLA except on teaching days, although one occasionally caught him late at night checking his mail.

It was in 1976 that Schwinger taught graduate classical electrodynamics, in a typically novel and very insightful way (including variational principles, of course, but especially noteworthy for the pre-eminence of physics over mathematics), and Milton suggested they turn the notes into a textbook. (Dittrich also claims credit for the proposal.[54]) Schwinger made his detailed notes available, and the three postdocs completed a first draft (more properly, version 1.5) of a manuscript, all neatly typed by Gilda Reyes of UCLA, and signed a contract with W. H. Freeman. Unfortunately, about the time Milton left UCLA in 1979 (recall that DeRaad and Tsai had departed a year or two previously) Schwinger decided the manuscript did not sound enough like himself, and started rewriting, incorporating many new features, but the result had a certain degree of turgidity. 'They came to these lectures and took notes and they wrote up these notes, did a lot of work, as a matter of fact. I did not look at the notes at all. Then the question arose, was this a publishable thing and they submitted it to some publisher and got back the response, well, yes, this seems like a very good book and it's very well written. At which point I said, Gee, that's interesting. Maybe I'll look at it. And then I began to read the notes. I realized that the notes were written—let me put it this way: when I read the notes, I did not hear myself talking. The notes were as written by the three students, who were, of course, very conscious of how I wanted to do things, but nevertheless had enough of their own mannerisms built into it with which I could not identify. I wanted to hear the words I would have used. And that has been the problem. In short, I learned that I cannot write books with other people. I must write them myself.... And so the electrodynamics book has been put on the side. Also, to do it I think requires a rather heroic or Herculean effort, because you know that Jackson[83] dominates the field.'[7] In fact, Schwinger more or less extensively revised the first half of the book, but then abandoned the project by the mid-1980s, after using the revised manuscript as a text when he taught electrodynamics again in 1983 and 1984.* At long last, the heroically revised, and greatly edited manuscript (in which, hopefully, Schwinger's lecturing voice emerges transparently) has been published. [231] The request of many, for example, 'I only wish Julian had completed the E&M textbook he started some time ago, and from which I had the chance to teach,'[79] has been answered in some small way.†

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* The Schwinger archive at UCLA[1] contains the 1984 revision, as well as earlier versions.

† Donald Clark was Schwinger's last student, defending his dissertation just a few months before Schwinger died. He was his student during much of the UCLA period, and thus had a number of observations of Schwinger's interactions there. Some of those related to his appreciation of the literature. For example, once in the late 1970s Milton

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gave a colloquium on magnetic charge scattering, with the fascinating phenomena of rainbows and glories. Someone asked, 'Where did you get all this stuff?' Clark heard Schwinger murmur, 'Books.' Clark also observed that in his interactions with students he did not push, but rather led. Once when Clark was in Schwinger's office discussing his research problem, Schwinger pulled a book off a shelf and paged through it. This message in Clark's view was: 'Read some books—I do.'

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# Taking the road less traveled

## Introduction

During his first decade at UCLA Schwinger was actively engaged in recasting high-energy physics into his own language, be it through unconventional interpretations of the $\psi$ particles [166, 169], non-speculative approaches to deep inelastic scattering [167, 168, 178, 179, 179a, 181–183], or the field theory of magnetic charge [172, 175]. But, as we have seen, the reception to this work was not so favorable. He had already abandoned writing his multivolume *Particles, sources, and fields* in 1974, precisely at the point where he was to begin dealing with strong and weak interactions. His last deep inelastic scattering paper [183] was submitted in January 1977. His sole foray into supersymmetry [190] was submitted in September 1978. His final publication on synchrotron radiation [186] appeared in 1978. So it may be fair to say that the hostility toward source theory pushed him out of the mainstream, and into projects where his still formidable strengths could make an impact. We can easily identify four well-defined themes that occupied the final two decades of his life: the Casimir effect, the statistical (or Thomas–Fermi) atom, cold fusion, and sonoluminescence.

However, external rejection was not the sole cause of this redirection of Schwinger's efforts. It was probably not coincidental that in the late 1970s his little group at UCLA unraveled: as noted before, Wu-yang Tsai left for Florida, returning shortly to industry in California, Lester DeRaad took a job at R&D Associates in Southern California, and finally, in 1979, Kimball Milton left for a temporary job at Ohio State, before finding a permanent academic post in Oklahoma. It is clear that throughout the 1970s this group had a significant influence on Schwinger's research (much more so than his assistants at Harvard, because there he was the center of theoretical physics); and if, say, Milton had stayed at UCLA, as he could have done, Schwinger most likely would have retained some contact with particle physics. But we will never know.

## The Casimir effect

In 1975 Schwinger became interested in the Casimir effect through conversations with Seth Putterman.[1] (Conversations with Walter Dittrich may have also played a role.[2]) The Casimir effect is a fundamental aspect of quantum field theory, indeed of quantum mechanics, usually expressed as an observable consequence of the zero-point fluctuations of the normal modes of the electromagnetic field, or of whatever quantum fields are relevant. From another, complementary point of view, it is the macroscopic manifestation of the van der Waals forces between the molecules that make up material bodies.* Let us begin by reviewing the history of this subtle yet fundamental phenomenon.

In 1948 H. B. G. Casimir[6] considered two perfectly conducting parallel plates in vacuum separated by a distance $a$. Although it is usually, and correctly, asserted that the zero-point oscillations of the fields in the vacuum are unobservable, the presence of the boundaries changes that situation. On a perfect conductor, the tangential component of the electric field must be zero. Casimir observed that one could measure the *difference* between the zero-point energy in vacuum and the zero-point energy in the presence of the boundaries,

$$E_c = \sum \frac{1}{2}\hbar\omega_{\text{cond}} - \sum \frac{1}{2}\hbar\omega_{\text{vac}}, \tag{15.1}$$

where the subscripts designate the normal modes, of frequency $\omega$, in the presence of the conducting boundaries, and in the vacuum of unbounded space, respectively, and the sums are over all possible normal modes in the two situations. Of course, this formula is purely symbolic, since both sums are horribly divergent. In effect, Casimir gave a proper definition to this sum, and was able to extract a finite result, the Casimir energy for this situation. Because the plates are assumed to extend indefinitely in the transverse directions, it is the energy $E_c$ per unit area of the plates which Casimir calculated,

$$E_c = -\frac{\pi^2}{720}\frac{\hbar c}{a^3}, \tag{15.2}$$

or, by differentiating with respect to the plate separation $a$, the force per unit area $f$,

$$f = -\frac{\pi^2}{240}\frac{\hbar c}{a^4} = -0.013\frac{1}{a^4}\ \text{dyn}\,(\mu\text{m})^4/\text{cm}^2. \tag{15.3}$$

* Casimir and Polder in a heroic calculation derived the retarded van der Waals force using nonrelativistic quantum electrodynamics.[3] Shortly afterwards, Niels Bohr asked Casimir what he was doing. After hearing of this work, Bohr 'mumbled something about zero-point energy. He gave me a simple approach.'[4] That new approach was reported in Paris, with a rederivation of the force between molecules, and the force between a molecule and a conducting plane.[5] We will discuss these results below. The derivation of the force between conducting planes[6] followed shortly.

Note the minus sign in these expressions. That means that the Casimir force is attractive: the two plates are pulled toward each other. To this day, the sign of this effect defies intuitive understanding.

As early as 1956 experimental results appeared more or less in agreement with the Casimir theory.[7] It is very difficult to measure the Casimir force between conductors, because any small stray electrical charge on the conductors will give rise to a much larger electrical attraction or repulsion. (Very recently, however, definitive experimental results, completely consistent with Casimir's prediction, have been published.[8]) On the other hand, it was clear from the outset that there was nothing particularly special about having conducting boundaries. Starting in 1955, E. M. Lifshitz and collaborators in Moscow developed the theory of the Casimir effect for parallel dielectrics, that is, for a situation translationally invariant in the $x$- and $y$-directions, but with different dielectric constants $\epsilon(z)$ in the three regions,

$$\epsilon(z) = \begin{cases} z < 0: & \epsilon_1, \\ 0 < z < a: & \epsilon_3, \\ a < z: & \epsilon_2. \end{cases} \tag{15.4}$$

For this geometry Lifshitz *et al.* obtained a somewhat complicated formula[9] that expressed the force per unit area between the dielectrics in terms of the dielectric constants, which were assumed to be functions of the frequency:

$$f = -\frac{1}{8\pi^2}\int_0^\infty d\zeta \int_0^\infty dk^2 2\kappa_3 \times \left\{ \frac{1}{\frac{\kappa_3+\kappa_1}{\kappa_3-\kappa_1}\frac{\kappa_3+\kappa_2}{\kappa_3-\kappa_2}e^{2\kappa_3 a}-1} + \frac{1}{\frac{\kappa_3'+\kappa_1'}{\kappa_3'-\kappa_1'}\frac{\kappa_3'+\kappa_2'}{\kappa_3'-\kappa_2'}e^{2\kappa_3 a}-1} \right\}, \tag{15.5}$$

where $\zeta = (1/\mathrm{i})\omega$ is the imaginary frequency, $k^2 = \mathrm{k}_\perp^2$ is the square of the transverse momentum, $\kappa^2 = k^2 + \zeta^2\epsilon$, and $\kappa' = \kappa/\epsilon$. In fact, this Lifshitz force was confirmed, based on knowledge of these dielectric constants, in a beautiful experiment of Sabisky and Anderson,[10] who measured the force holding a film of superfluid liquid helium to a $SrF_2$ substrate, to high precision, over distance scales differing by a factor of 1000. So by 1973 there was no doubt of the theoretical or experimental reality of the Casimir effect.*

* Schwinger noted in a talk in 1988 in honor of Herman Feshbach that Leonard Schiff had proposed that van der Waals forces were responsible for holding such a helium film to a surface, thus anticipating the Lifshitz theory.[11]

As noted, in 1975 Schwinger became interested in explaining the Casimir effect in the source theory language, which 'makes no reference to quantum oscillators and their associated zero point energy.' [174] As usual, his presentation was first to his field theory class, and only then did he write a short paper for publication [174]. Anticipating that the effect of the two polarizations of electromagnetism was merely a doubling of that for a single, massless, scalar mode, his derivation consisted, first, in obtaining the general expression for the infinitesimal change in the action for a scalar particle under an infinitesimal change in the physical parameters,

$$\delta W = \frac{\mathrm{i}}{2}\int (\mathrm{d}x)(\mathrm{d}x')D(x,x')\delta D^{-1}(x',x), \tag{15.6}$$

where $D$ is the massless propagation function or Green's function, or the equivalent change in the energy

$$\delta\mathcal{E} = -\frac{\mathrm{i}}{2}\int (\mathrm{d}\mathbf{r})(\mathrm{d}\mathbf{r}')\mathrm{d}\tau D(\mathbf{r},\mathbf{r}',\tau)\delta D^{-1}(\mathbf{r}',\mathbf{r},-\tau), \tag{15.7}$$

which ignores transient effects. Then, by inserting an appropriate Green's function that satisfies the Dirichlet boundary conditions at $z = 0, a$, written in terms of the longitudinal eigenfunctions, $\sqrt{2/a}\sin(n\pi z/a)$, he obtained the following formula for the change in the energy per unit area if the separation is changed by an amount $\delta a$, due to the Green's functions in the region $0 < z < a$:

$$\frac{\delta\mathcal{E}_a}{A} = \frac{1}{4\pi}\frac{\delta a}{a}\frac{1}{\mathrm{i}\tau}\frac{\mathrm{d}^2}{\mathrm{d}\tau^2}\frac{1}{1-\mathrm{e}^{-\mathrm{i}(\pi/a)\tau}}, \tag{15.8}$$

where the limit $\tau \to 0$ is understood. This result is divergent in that limit. But Schwinger then subtracted off the contribution from the region on the other side of the plate, $a < z < L$ (an additional conducting plate is placed at $z = L \gg a$), which may be immediately inferred from Eqn (15.8) to be

$$\frac{\delta\mathcal{E}_{L-a}}{A} = -\frac{1}{4\pi}\delta a\frac{1}{\mathrm{i}\tau}\frac{\mathrm{d}^2}{\mathrm{d}\tau^2}\frac{1}{\pi\mathrm{i}\tau}. \tag{15.9}$$

The force per unit area is then immediately found from the sum of Eqns (15.8) and (15.9) to be

$$f = -\frac{1}{A}\frac{\partial\mathcal{E}}{\partial a} = -\frac{\pi^2}{480}\frac{1}{a^4}, \tag{15.10}$$

indeed, exactly one-half Casimir's result (15.3).

Schwinger concluded this note by rederiving the effect of finite temperature, in particular, the high-temperature limit,

$$kT \gg \frac{\pi}{a}: \quad f_T = -\frac{\zeta(3)}{8\pi}\frac{kT}{a^3}, \tag{15.11}$$

which had first been obtained by F. Sauer and J. Mehra.[12] Schwinger justified this publication, apart from it giving the Casimir effect a source theory context free from an operator substructure, by quoting from C. R. Hargreaves,[13] who stated that 'it may yet be desirable that the whole general theory be reexamined and perhaps set up anew.' The context of the latter remark was a discrepancy between the temperature dependence found between conducting plates, and that found from the temperature-dependent Lifshitz formula[9] when the dielectric constant in the region outside $0 < z < a$ is set equal to infinity, a process which should correspond to a perfect conductor. Unbeknownst to Schwinger, this error had been corrected subsequently.[14] (Hargreaves had corrected another error in Lifshitz's paper having to do with the effect of imperfect conductors.)

It was partly this (non-existent) discrepancy, but primarily the challenge to understand the phenomenon in his own language, that led Schwinger, and his postdocs Milton and DeRaad, to write 'Casimir effect in dielectrics' [187], in which the Lifshitz formula for the Casimir force between parallel dielectrics was rederived in an elegant, action-principle based, Green's function technique. The key point here was that the effective product of electric fields could be represented in terms of the classical electromagnetic Green's dyadic,

$$\mathrm{E}(\mathrm{r})\mathrm{E}(\mathrm{r}')\big|_{\mathrm{eff}} = \frac{\hbar}{\mathrm{i}}\,\boldsymbol{\Gamma}(\mathrm{r}, \mathrm{r}'; \omega), \tag{15.12}$$

where, from Maxwell's equations, the Green's dyadic satisfies

$$-\boldsymbol{\nabla} \times (\boldsymbol{\nabla} \times \boldsymbol{\Gamma}) + \omega^2\epsilon\boldsymbol{\Gamma} = -\omega^2\, \mathbf{1}\delta(\mathrm{r} - \mathrm{r}'). \tag{15.13}$$

From $\boldsymbol{\Gamma}$ Schwinger, Milton, and DeRaad calculated the change in the energy, using a method similar to that sketched above, or equivalently, the force directly from the electromagnetic stress tensor,

$$T_{zz} = \frac{1}{2}[H_\perp^2 - H_z^2 + \epsilon(E_\perp^2 - E_z^2)], \tag{15.14}$$

where H is calculated from E (and hence $\boldsymbol{\Gamma}$) using Maxwell's equations. Removing constant divergent terms from the result, the so-called volume stress, which would be present if a given dielectric extended over all space, they succeeded in rederiving the Lifshitz formula. As a special case, they took the perfect conductor limit noted above ($\epsilon \to \infty$ in the external region) and obtained the Casimir result, as well as the appropriate high- and low-temperature limits found by Sauer and Mehra.[12] They also showed how, in the case of tenuous dielectrics, i.e. in the case when $\epsilon - 1 \ll 1$, the Casimir force could be thought of as the superposition of the van der Waals attractions between the individual molecules (separated by a distance $r$) that made up the media,

$$\text{large separations:} \quad V = -\frac{23}{4\pi}\frac{\alpha_1\alpha_2}{r^7}, \tag{15.15}$$

$$\text{small separations:} \quad V = -\frac{3}{\pi r^6}\int_0^\infty d\zeta\, \alpha_1(\zeta)\alpha_2(\zeta), \tag{15.16}$$

where $\alpha = (\epsilon - 1)/4\pi N$ is the electric polarizability of the molecules, with number density $N$. These are the van der Waals potentials originally derived by Casimir and Polder[3] and by Fritz London,[15] respectively.

These results were all explicitly contained in the much earlier papers by Lifshitz and collaborators,[9] to whom due acknowledgement was made. Nevertheless, Lifshitz was somewhat offended by this paper, and he wrote Schwinger a letter: 'Thank you for the preprint of your... paper .... It was gratifying to know of your interest in my earlier work.

'Of course, the method adopted in this paper is far superior than [sic] the method which was used in my first paper of 1954. But it seems to me that it is almost identical with the method developed later by I. Dzyaloshinskii, L. P. Pitajevskii, and myself. The derivation of my results by this method was published in our joint paper in *Advances of Physics*, 1961 (identical with the paper in *Soviet Physics, Uspechi*, referred to in your preprint); it was also reproduced in the book by Abrikosov, Gorkov, Dzyaloshinskii on the *Field Theoretical Methods in Statistical Physics* (English translation, Prentice Hall, 1963).

'As to the formula for the low temperature limit of the force between the two perfect metallic surfaces (formula 3.17 of your paper), the error in sign in my paper was the result merely of an unfortunate slip in rewriting the Euler summation formula, and not of a deeper origin. This error has since [been] noticed by different authors both in our country and elsewhere.'[14]

The only really new result in this paper was an attempt to derive the surface tension for an ideal liquid (liquid helium) from such considerations, by examining the effect of a change of shape of the surface on the energy. 'The second-order change in the energy ... is directly related to the surface tension.' [187] Unfortunately, a quadratically divergent result was obtained. However, with reasonable numbers inserted to provide a physical cutoff to the divergence, a value for the surface tension, and for the latent heat, could be obtained crudely in agreement with the observed values to within a factor of two or three. This idea remains provocative yet unresolved.

A few months later the same three authors wrote a second paper on Casimir phenomena, entitled 'Casimir self-stress on a perfectly conducting spherical shell.' [188] The impetus for this work went back to another paper of Casimir, this one in 1956, in which he suggested that the attractive Casimir force could balance the Coulomb repulsion of a semiclassical model of an electron.[16] More precisely, it had long been known that a purely electromagnetic classical model of an electron was impossible, that one had to add the so-called Poincaré stresses to stabilize the particle. Casimir now suggested that those stresses could arise from quantum mechanics. Indeed, if a reasonable guess extrapolated from the parallel plate calculation was used, one could calculate a value for the charge on the electron, or better, the fine structure constant, $\alpha = e^2/\hbar c$, consistent, perhaps, with the experimental value, $\alpha = (137.036\ldots)^{-1}$.

It remained for Timothy Boyer, a student of Sheldon Glashow at Harvard, to take up the challenge of a real calculation for the spherical geometry in 1965. He calculated the change in the zero-point energy due to the presence of a perfectly conducting spherical shell of radius $a$. Both modes, interior to, and exterior to the shell had to be included in order to get a finite result. This impressive calculation was difficult and subtle, and involved extensive numerical calculation. His result, obtained after three years of work, was accurate to only one significant figure, but it was of the *opposite* sign compared to the one found by Casimir in the parallel geometry:[17]

$$E_{\mathrm{B}} = +\frac{0.9}{2a}. \tag{15.17}$$

His expression was subsequently evaluated more accurately, to three significant figures, by Davies.[18]

Because this result was so surprising, and devastating to Casimir's electron model, it was an obvious target for a recalculation by Schwinger and his postdocs, now that their improved Green's function machinery had been honed. By the end of 1977 they had derived a compact formula for the Casimir energy of a conducting shell, much simpler than that of Boyer,

$$E = -\frac{1}{2\pi a}\sum_{l=1}^{\infty}(2l+1)\frac{1}{2}\int_{-\infty}^{\infty}\mathrm{d}y\,\mathrm{e}^{\mathrm{i}\epsilon y}x\frac{\mathrm{d}}{\mathrm{d}x}\log(1-\lambda_l^2), \tag{15.18}$$

where the sum is taken over the different angular momentum modes, the integral is over (imaginary) frequencies, $y = (1/\mathrm{i})\omega a$, the quantity $x = |y|$, and the logarithm depends on

$$\lambda_l(x) = (s_l e_l)'(x) \tag{15.19}$$

(where the prime denotes differentiation). The functions $e_l$ and $s_l$ are given in terms of modified Bessel functions,

$$s_l(x) = \sqrt{\frac{\pi x}{2}} I_{l+1/2}(x), \tag{15.20}$$

$$e_l(x) = \sqrt{\frac{2x}{\pi}} K_{l+1/2}(x). \tag{15.21}$$

The expression (15.18), which is formally divergent, has been regulated by evaluating the underlying Green's function at unequal times, $t = t' + \tau$, i.e. by 'time-splitting.' At the end of the calculation one is to take the limit $\epsilon = \tau/a \to 0$. Unfortunately, at this point Milton and DeRaad had a bit of difficulty in seeing how to extract a number from this formula, so a few months passed. (Schwinger had contented himself with deriving the formula.) Unfortunately, because just at that point a paper by Balian and Duplantier appeared,[19] who obtained a different formula, based on a multiple scattering formalism, and obtained a result, consistent with Boyer's number, but now accurate to three significant figures. So the postdocs worked hard, discovered how to extract a reliable answer based on the use of uniform asymptotic approximations (the first term of which was accurate to 2%, while Balian and Duplantier's first approximation was only accurate to 8%), and obtained the result accurate to *five* significant figures,

$$E = \frac{0.92353\underline{1}}{2a}. \tag{15.22}$$

The reaction from Boyer and Balian was rather unexpected. In a letter to Lester DeRaad (DeRaad and Boyer, of course, had been fellow graduate students at Harvard) Timothy Boyer wrote, 'The calculations presented seem sophisticated, and presumably are carefully done. However, the comments on my work in the text of the Casimir sphere paper are hardly generous; my colleagues would characterize them differently.'[20]

He went on to apprise DeRaad of the Davies calculation, and to give further experimental references, which were incorporated into the published papers. In addition, an appreciative comment about Boyer's work was inserted into [188].

Roger Balian wrote Schwinger to say 'I guess it would be interesting to compare our respective approaches, which have the common feature of being based on the elimination of fields and consideration of sources. Our formalism was mainly intended to deal with arbitrary geometries; it is based on an expansion which converges rapidly in cases of interest (slightly deformed conducting sheet, spherical shell, etc. ... ). However, we construct the electric Green's function in terms of fictitious monopole currents, and restrict to conductors. Your approach has the advantage of allowing the treatment of dielectrics; I do not see, however, how to use it for arbitrary geometries; on the other hand, would you obtain instabilities of the surface of a dielectric at $T \neq 0$, thus generalizing the effect which we pointed out for a conducting foil?'[21] Since this letter was dated 28 December 1977, more than five months before Schwinger's paper on the Casimir effect for a sphere was submitted [188], it seems likely that at that point Balian had only seen the dielectric paper [187]; hence the remark about geometries.

Milton responded to both of these letters graciously, and promised to look at Balian's technical points in the future, but that never occurred.

Schwinger's first papers on the Casimir effect were influential, not for their explicit results, which, as we have seen, were mostly well known, but for the development of powerful techniques of attacking such problems, which continue to be exploited. A recent example is the study of the dimensional dependence of the Casimir effect in hyperspheres by Bender and Milton.[22]

Schwinger continued his involvement with the Casimir effect for the rest of his life. As we will see below, in the 1980s he explored, but did not publish, the related connection between acceleration and thermal radiation; and in the last few years of his life he suggested that the remarkable phenomenon of sonoluminescence was due to the dynamical Casimir effect.

## Schwinger's 60th birthday

On the occasion of Schwinger's 60th birthday, in 1978, the UCLA Physics Department hosted a symposium in his honor. Schwinger gave his grudging approval, and planning started approximately a year and a half in advance. Students and friends were invited; nearly half of his 70 some doctoral students attended. The lectures, chosen to reflect Schwinger's many interests, were given by Sydney Drell, on the 'Experimental status of quantum electrodynamics,' Herman Feshbach on 'Schwinger and nuclear physics,' Sheldon Glashow on 'The unmellisonant quark,' Gerson Goldhaber on 'From the $\psi/J$ to charmed mesons—three years of $e^+e^-$ research at SPEAR,' Harold Levine 'On the theory of Kirchhoff's method for the determination of electrical conductivity,' Paul Martin on 'Schwinger and statistical physics: a spin-off success story and some challenging sequels,' Yoichiro Nambu on 'Topological problems in gauge theories,' and Bruno Zumino on 'Supersymmetry and supergravity.' These talks, as well as many solicited tributes from other students and friends, were collected in a *Festschrift* published as a special issue of *Physica*.[23] Many old friends gave wonderful historical talks during the $1\frac{1}{2}$ day symposium and after the banquet; they included Isidor Rabi, Morton Hamermesh, Bernard Feld, Victor Weisskopf, David Saxon, and Richard Feynman.* These talks were transcribed

* One old friend did not make it. John Van Vleck from Harvard was taken ill just before the Symposium, and was rushed to the emergency room in La Jolla on 15 February. He

under Milton's direction, and are available in the Archives of the American Institute of Physics. They have been quoted from extensively throughout this book. (Feynman's talk was printed in the *Festschrift* for Schwinger's 70th birthday,[25] which meeting Schwinger graciously dedicated to Feynman's memory.) After the public ceremonies, a private dinner party for a few of their old and dear friends was held at the Schwingers' house in Bel Air. For the occasion, Weisskopf played one of Schubert's piano sonatas.[2]

Schwinger was not at all happy about the 60th birthday event at the time. He thought it was intended as a retirement party, in spite of many protestations to the contrary, and so gave only reluctant approval to proceed. He did not even make a response to all the wonderful tributes after the banquet. It was only several years later, at Georgia Tech, when Schwinger received the Monie Ferst Medal at Georgia Tech, that he came to realize that that celebration had been offered in love and affection, and, in effect, apologized to his students there, who had been instrumental in organizing the event. 'I remember when Julian gave his talk there ... he introduced his talk by saying some kind words about how he was glad to be honored here and it made him think back on his celebration, the affair that was held on his 60th birthday, and that he had misinterpreted that event as being one that said you're through, you're done, and then turned to the direction of his former students [Johnson, Kivelson, and Milton] and actually apologized for having taken it less kindly than it was meant. And then he went on. And the subject of his first talk was, is there life after 60, is there science after 60.'[26]

As a further tribute to Schwinger's many contributions to physics, a volume of selected works was published in 1979.[27] This developed because Moshé Flato,

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sent a moving testimonial, however, from his hospital bed: 'Very disappointed to miss the ceremonial session and frustrated to be hospitalized so near the goal line. Abigail and I send our congratulations not only to Julian but also to Clarice. Rabi can claim Julian as a product of New York culture, but we claim Clarice as a proper Bostonian. Columbia is to be felicitated for its sagacity and liberality in giving Schwinger a traveling fellowship to go to Wisconsin in 1937 so he could get a good education prior to his doctorate. This was the golden year in theoretical physics at Madison, with Schwinger, Wigner, and Breit all on the campus at the same time. I need not elaborate on his achievements while at Harvard, except to mention that the Karplus and Schwinger paper on line breadth is a classic which has been a guideline to much of my subsequent research in this field. I congratulate Schwinger not merely on his past research but projected into the future. A few years ago I commented in the Harvard Alumni Magazine on how three members of its faculty, Kendall, Oldenburg, and Webster were still publishing at age 83. With Schwinger's sustained productivity in research displayed by his starting at 17, he should do at least as well, and a simple calculation shows that he will still be publishing in 2001.'[24]

of Dijon University, who frequently visited UCLA to work with Chris Fronsdal, was the editor of a series of books published by Reidel in Dordrecht. Flato and Fronsdal enlisted Milton's help, and they went to Schwinger to secure his blessing for the project. It ended up being a personal selection: Schwinger selected the articles he wanted reprinted (and not those which had been reprinted in earlier collections, such as [83]), and Schwinger prefaced each by a pithy phrase indicating why he had made that selection. The result is a handy overview of Schwinger's contributions, incomplete of course, but including a relatively complete list of his publications, compiled by Milton, which formed the basis of the complete publication list appearing in Appendix A of this book.

## The Thomas–Fermi atom

In 1980, after teaching a quantum mechanics course (a not-unusual sequence of events), Schwinger began a series of papers on the Thomas–Fermi model of atoms [192–196, 201–206]. He soon hired Berthold-Georg Englert, replacing Milton as a postdoc to help with the elaborate calculations. This endeavor lasted until 1985. Much of the following discussion is based on an interview with Englert in 1997.[28]

The Thomas–Fermi model, introduced by Llewellyn Hilleth Thomas and Enrico Fermi in 1927[29] is at heart a statistical approach to a large atom, based on the idea that if the atomic number $Z$ is very large, the average principal quantum numbers of the electrons are also very large. One may consider the Poisson equation satisfied by the electrostatic potential $\phi(r)$, and express the latter as a dimensionless variable $\chi(x)$ correcting the Coulomb potential according to

$$\phi(r) = \frac{Ze}{r}\chi(rZ^{1/3}me^2/0.885\hbar^2), \tag{15.23}$$

where $m$ and $e$ are the mass and charge of the electron, respectively. The new variable $\chi$ satisfies the so–called Thomas-Fermi equation,

$$x^{1/2}\frac{\mathrm{d}^2\chi}{\mathrm{d}x^2} = \chi^{3/2}, \tag{15.24}$$

subject to the boundary conditions $\chi = 1$ at $x = 0$ and $\chi = 0$ at $x = \infty$. (For an elementary discussion, the reader may consult any of a number of quantum mechanics textbooks, for example Ref. 30.) It is a bit subtle to solve the Thomas–Fermi equation numerically, because it is, in effect, an eigenvalue equation for the initial slope, $\chi'(0)$.

It would be difficult to do better in tracing the origin of Schwinger's work on this subject than to quote from the opening paragraph in the first paper in this series, 'Thomas–Fermi model: the leading correction,' [192]: 'The Thomas–Fermi (TF) model was one of the topics I selected for an undergraduate course in quantum mechanics [in fall 1979]. My level of knowledge of its applications was that of textbooks of the 1950s, along with the monograph cited in Ref. 31; I had no reason to suspect that the particular subject of the binding energies had attracted any more recent attention. Once again I was struck by the qualitative agreement of the model with empirically estimated total binding energies, for a wide range of [atomic number] $Z$. The slowly varying nature of the quantitative discrepancy suggested that a simple leading correction could be found. A qualitative argument indicated that it would vary as $Z^{-1/3}$, and a proper numerical factor was obtained by an elementary physical derivation. The resulting remarkable agreement with experiment seemed to merit a small publication. The referee* of that paper kindly drew my attention to a communication by Scott[32] in which the same result appeared, identified as a "boundary effect." Of course, the underlying physical ideas are the same—the error of the TF model in giving the electrons an infinite density at the nucleus had always been recognized. But, as to the reliability of the quantitative statement, as derived by Scott (I quote from another reference[33] supplied by the referee), "it seems difficult to give a completely clearcut demonstration of the case." Accordingly, I feel justified in resubmitting my "clearcut demonstration." It will be followed by a discussion of the different approaches, and then by a partial treatment of relativistic effects, which topic does not appear to have received its definitive study (again, there may be publications more recent than the citations [of Ref. 33]).' Thus, the response to the referee was a substantially enlarged paper, including new sections entitled 'Discussion' and 'Relativistic Corrections.'[11]

The following year Schwinger published a second paper† in *Physical Review A* on the 'Second correction' [193] (which originally had been submitted to *Physical Review Letters*, but was too long[11]), and then enlisted Lester DeRaad, Jr, his former postdoc, and current tennis partner, then and now at R&D Associates in Los Angeles, to help with the numerical calculations in [194]. In 1981 Schwinger spent the summer term at the University of Tübingen with a Humboldt Award, arranged by Walter Dittrich,[2] and gave a seminar on this work. Tübingen had long been a favorite place for Schwinger to visit, largely through his friendship with Walter Dittrich there. (Recall he had given his famous 'Conflicts in physics' lecture there a few years earlier.) Later Dittrich recalled fondly,

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* That referee was Larry Spruch, who in a letter to Schwinger dated 30 May 1980, acknowledged himself as referee and called it 'a lovely paper.'[11]

† Eugen Merzbacher recalled that Schwinger spoke on this topic at a special session of the Washington APS meeting, probably in 1981, which Merzbacher had helped organize, in honor of the work on many subjects by L. H. Thomas. Schwinger took the 'red-eye' to Washington; among the other speakers was Larry Spruch, who spoke on classical atomic collisions, Thomas's first work as an undergraduate.[34]

'Julian and Clarice spent a whole semester in Tübingen, during which they occupied a small house a few kilometers away from the city. Julian rented a piano, drove a used Audi (Clarice bought a used bike), took German lessons and often met Ginny [Dittrich's wife] and me at the tennis courts, where he proved to be very harsh on himself. This may have had to do with the early hour, since Julian became active rather late in the day. It was only after the "good time hour" (gin-and-tonic time around 6 p.m.) that Julian relaxed and became most charming. On one of the photos I took of Julian he wrote: "To Walter with appreciation for letting me lose at tennis and win at the Museum [our favorite restaurant in Tübingen]."'[2] However, Dittrich had been a little disappointed by how private and non-interactive Schwinger was on that trip, as was always his custom.[35]

Schwinger also had an unhappy experience with the local police authorities. In Germany, one must register with the police if one stays in a city any length of time. When Dittrich took Schwinger to register, Julian was taken aback by the question about religion, and refused to answer. Only because of Schwinger's exalted stature were they able to bend the rules and avoid supplying a response.[35]

Of course, Schwinger gave a seminar on his work on the Thomas–Fermi atom, and in the audience was Berthold-Georg Englert, who had just received his PhD under Dittrich's direction, in the process receiving a distinguished dissertation prize. Dittrich suggested Englert look at some of the problems. 'I think it took only a day or two for Englert to come up with some beautiful graphs that made my special guest very happy.'[2] Schwinger invited Englert to join the project, so Englert came to UCLA in November 1981 as a postdoc.* (For the first few months he shared the position with Gregg Wilensky, who had just received his PhD from Schwinger.) Englert was immediately involved in work on 'the outer regions of the atom' [195], a paper already begun by Schwinger before their collaboration.

There were two very substantial groups of three papers each that resulted from the Englert–Schwinger collaboration on the 'statistical atom.' The first set, entitled just that [201–203], had to do with treating the strongly bound electrons in the atom. The work was explicitly analytical, essentially a 'very much refined WKB approximation.'[28] Central to the calculation was averaging over Airy functions, which was completed in 1981. Once the strong binding problem

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* In a letter to Ernie Abers, who was then the Chairman of the UCLA Physics Department, Schwinger wrote in September 1981 from Germany: 'When I left, and it already seemed Kim Milton's position would be vacated, I thought we agreed that Wilensky and Wilcox would share it. Do I now gather correctly that Wilcox will join Milton in Oklahoma, presumably freeing that fraction of the job?' He went on to suggest Englert (who had already applied to UCLA) for that job, and suggested Abers call Walter Dittrich in Tübingen.[11]

was solved, only programming remained; the work was mostly completed in 1982, although the papers were not submitted until late in 1983. Part of the reason for the delay was that the second paper of the set [202] was written first by Schwinger; Englert wrote the first drafts of [201] and [203] later, and then all had to be smoothly joined together.

The second set of three papers [204–206] had to do with shell corrections. Remarkably, one can push the semiclassical picture to see atomic shell effects, manifested in the most weakly bound electrons. A hint of this was seen already in Appendix 2 of [195]. The Thomas–Fermi statistical picture incorporates the deeply bound electrons, while the weakly bound electrons are dealt with perturbatively. This work, which was largely analytical, was already started while the first set of papers was being written. Here the crucial paper was the first one, which explained the systematics of the periodic table, and the characteristic oscillations in the binding energy with atomic number. It is useful to quote the opening paragraph of that paper, for it reveals that the 'semiclassical atom' was a new approximation. 'In theoretical atomic physics two main approaches have been pursued. One is the Hartree–Fock (HF) method and its refinements; it can be viewed as a generalization of Schrödinger's description of the hydrogen atom to many electron systems. It is, by construction, more reliable the smaller the number of electrons. The other one is the statistical Thomas–Fermi (TF) treatment and its improvements. This one uses the picture of an electronic atmosphere surrounding the nucleus; it is better the larger the number of electrons. Somewhere between the HF and TF treatments is the semiclassical approach that we want to study here. It borrows the idea of a common average potential for all electrons from the TF method while using the concept of angular and radial quantum numbers much as the HF method does.' [204] And the conclusion of the third paper in the series reveals that, in a real sense, they had only reached the point of significant physical applications when the collaboration concluded. 'We labored mightily to produce a semiclassical result that is not essentially different from the previously known HF prediction. Why did we choose to do so? Because now we have an understanding of the physical origin of the nonrelativistic binding-energy oscillations. . . . The HF method is designed for the investigation of individual atoms with given nuclear charge and number of electrons. In contrast, the semiclassical approach is meant to deal as a whole with the Periodic Table. . . . This remarkable achievement of the semiclassical method demonstrates that it is sufficiently refined to describe atomic properties that are attributable not only to the central bulk of electrons, but also to the relatively few outer electrons. This points to possible future applications. For example, the calculation of atomic electric polarizabilities requires a good description of the loosely bound electrons at the edge of the atom. So far, it has been notoriously difficult to handle these electrons with sufficient accuracy in

TF-type statistical models, although certain improvements have already been made. [202] The new semiclassical method is likely to take the next step in this direction.' [206]

In 1982 Schwinger, during a visit to Tübingen that was again arranged by Dittrich,[2] gave a talk at the annual meeting of Nobel Laureates, organized by Count and Countess Bernadotte, sponsored by German industry, which was a masterful summary of all this program. Eventually, in 1985, he and Englert wrote up an ensuing article to be published in the *Physikalische Blätter*, the German analog of *Physics Today*, upon the invitation of its editor, Ernst Dreisigacker. Unfortunately, when it was sent by Englert to Dreisigacker in 1986, the editor found it unacceptable: it was too long, had too many equations, and was not written in German. The following year, Wolfgang Witschel, who was editing a new journal, *Comments in Chemical Physics*, wished to include the paper as the first article in the new publication. Again, fortune did not smile, and that journal never came into being. Consequently, this interesting article, which by that point Englert had decided was obsolete, especially because he had written a monograph on the subject,[36] was never published.[37]

Englert left UCLA in March 1985 for a position at the University of Munich, where he is today. This marked the end* of the Thomas–Fermi papers, because, as Schwinger noted in a letter of recommendation for Englert, 'It was not long before he took the lead in our joint efforts.'[11] They carried on their collaboration, however. Schwinger continued thinking about the statistical atom, as witness a jury duty notebook dated 1987.[11] In 1987 Schwinger spent the summer in Munich, where they did some work on molecules, but nothing got written up because 'the real applications were missing.'[28] Then, as a result of conversations in Marlan Scully's office in Garching, they were dragged into the 'Humpty Dumpty' papers [208–210], questioning whether one can reunite beams of atoms which have been separated by a Stern–Gerlach apparatus—and indeed one cannot. It probably did not take too much coaxing to get Schwinger involved in this work, for those who took his quantum mechanics courses knew how central the Stern–Gerlach experiment was to his formulation of quantum mechanics.† We have discussed these papers in Chapter 10. In the course of this collaboration Schwinger and Englert got together

* However, a joint contribution did appear in 1989: [210a], presented at a conference in Yugoslavia. Schwinger had been invited to this conference, but Englert went in his place.[37]

† Indeed, in a talk in honor of Rabi in 1988, Schwinger began with, 'My first encounter with I. I. Rabi occurred in 1935, during a conversation about the quantum theory of measurement. Indeed, it concerned the famous Einstein, Podolsky, and Rosen paper of that year.'[11]

in Garching and Albuquerque periodically. Schwinger continued to be interested in quantum-mechanical interference, and in 1988 or so gave lectures on MAGIC and FOCUSED MAGIC, which are acronyms, the second one standing for 'Fiber Optic Counterpart Using Spin Energy Deflection of Magnetic Atoms for a Gyroscopic Interferometric Counter.' The MAGIC lecture begins with 'thoughts on a neutral atom magnetic interferometer used as a gyroscope, that is sensitive to low rotation rates, contrasted with a passive ring laser gyro.'[11] In 1989, as we shall discuss below, the phenomenon of cold fusion appeared; Englert was not involved in Schwinger's work on cold fusion but was largely responsible for getting the two papers [213] and [214] published.

Schwinger and Englert last met in 1993. Schwinger was supposed to talk about quantum mechanics—essentially the same talk as that given in [208a]—at a meeting in Crested Butte, Colorado in August 1994, and Englert intended to meet him in Salt Lake City, and drive him to the mountain resort; when Schwinger was taken ill that Spring, he asked Englert to give the talk for him.

It is interesting that this work not only is regarded as important in its own right by atomic physicists, but has led to some significant results in mathematics. A long series of substantial papers by C. Fefferman and L. Seco[38] has been devoted to proving his conjecture about the $Z$ dependence of the ground state energy of large atoms [193]. As Seth Putterman has remarked, it is likely that, of all the work that Schwinger accomplished at UCLA, his work on the statistical atom will prove the most important.[1]

## A foray into industry

In 1982 or so, Wu-yang Tsai, followed by Lester DeRaad, Schwinger's former students, and postdocs, together with others at R&D Associates founded their own company to tap into the large increase in federal defense R&D expenditures anticipated because of President Reagan's crusade to develop a defense against intercontinental ballistic missiles, the Strategic Defense Initiative, popularly called Star Wars. This small company, mostly consisting of theoretical physicists, was named Research and Development Laboratories (RDL), which set up shop first in Torrance and then in Culver City, California. DeRaad's and Tsai's immediate intention was to secure Schwinger's involvement. Such involvement might seem rather uncharacteristic, given Schwinger's deliberate distance from the weapons laboratories, Los Alamos, and later Livermore, during and after the war.* (Of course, he had worked at the Radiation Lab at MIT on radar during the Second World War, which was not a purely defensive technology.

* However, recall in the mid-1950s that he held a contract with the Signal Corp to study millimeter microwave generation, for which he put in hundreds of hours of work. He also held an AEC Q clearance until 1956.[11]

His old friend and colleague, David Saxon, said that 'he never talked to me about why he didn't go to Los Alamos' during the war.[39]) But friendship proved persuasive. In 1984 Schwinger accepted an offer to be on the board of RDL; he was already involved in consultations in 1983. The UCLA Archives have an extensive collection of his technical reports on the Orion Project, detection of stealth aircraft, and electromagnetic scattering.[11]

However, the company was not very successful. There never really was that much new money put into Star Wars; most of the large funding reported in the press was simply the result of repackaging existing projects and R&D programs. And securing such contracts required contacts in Washington, which the new company did not possess in sufficient degree. But most damaging was mismanagement at the top. Eventually, with the potential of significant personal loss (they had invested their retirement savings in the enterprise) first DeRaad and then Tsai left, DeRaad returning to R&D Associates, later to be subsumed by Logicon, and Tsai going to JPL in Pasadena. (Tsai stayed on the board long enough to extricate his friends and Schwinger from financial loss and responsibility.) Schwinger resigned from the board of RDL in August 1986.[11,40]

**Books, homages, and talks**

In May 1980 an International Symposium on the History of Particle Physics was held at Fermilab. Schwinger gave a talk entitled 'Quantum electrodynamics—an individual view,' [197] recounting his development of quantum electrodynamics. With slight changes, this talk was reproduced in the Brown and Hoddeson collection [199].* Because Ben Nefkins of UCLA had not been able to attend the Fermilab meeting, Schwinger gave an essentially identical public lecture at UCLA. The talks end with a remark distancing himself from his earlier work: 'And so, if I were asked to respond to criticism of these events I have recounted prior to the beginning of the sixth decade, I would answer, I don't do it that way anymore.'[11]

Sometime in the early 1980s, on receiving an award in Los Angeles (possibly from Sigma Xi), Schwinger gave a moving retrospective account of crucial early influences: 'If I were a different person, attending a somewhat similar celebration in the neighborhood of Hollywood, California, I would name, and thank, a host of people, beginning with my mother and ending with my agent. Tonight, however, I shall only refer to a few, essentially *anonymous* people, who had a significant impact on me. I think it happened in the third grade, or was it

* For that and the accompanying paper on Tomonaga [200], Schwinger was concerned that he would not be allowed to correct the proofs. He was particularly concerned with the appearance of his name: 'I do not use the middle initial S. I should appreciate your removing it.'[11]

the fourth. My teacher was telling her class how the moon manages to present an eternally unchanging countenance. Noticing that I was using my fingers to illustrate her lesson, she smiled at me approvingly. I trace my pleasure in moon-viewing—and my scientific career—back to that moment.

'I took my first physics course in High School. That instructor* showed unlimited patience in answering my endless questions about atomic physics, after the class period was over. Although I try, I myself cannot live up to that lofty standard.

'These examples refer to people whose names have vanished in the mists of time. On a lighter and final note, I come to a truly anonymous person. I had exhausted the easily available library facilities and turned to the reference room of the main public library. The name of the journal I wanted was known to me only in abbreviated form. I strode up to the desk and asked the clerk for the *Proceeds* of the Royal Society. He looked a little surprised, and then said, Ah, you want the *Proceedings* of the Royal Society.

'And so, allow me to wish that the *young* students among us (we are all *students*) have had, and will have, their own rewarding encounters of the anonymous kind.'[11]

We recall that in 1985 Schwinger's popular book on relativity, *Einstein's legacy*, [207] appeared, based on a series of television programs he developed for the Open University in the UK some years earlier. He spent a great deal of time on several book projects; first he attempted to complete the textbook on classical electrodynamics, discussed in Chapter 14, and then he once more tried to write the book on quantum mechanics,† which he had been working on since the early 1950s; but nothing was completed. We discussed this lifetime saga in Chapter 10. He also wrote three very interesting homages in the 1980s: 'Two shakers of physics' [200], the pun in the title referring to himself and Tomonaga, 'Hermann Weyl and quantum kinematics' [208a], in which he acknowledged his debt to one of his 'gods,' whose ways 'are mysterious, inscrutable, and beyond the comprehension of ordinary mortals,' and 'A path to electrodynamics' [212], dedicated to Richard Feynman. We will discuss the Tomonaga and Feynman tributes in the next chapter.

In the 1980s Schwinger gave many historical lectures, and not always about his own contributions to electrodynamics and field theory. As we recall from the previous chapter, one of the most interesting was 'Conflicts in physics,' a

* Possibly Alfred Bender, who taught at Townsend Harris at the time.[41]

† Now he had a new viewpoint; in the late 1970s and early 1980s he taught the undergraduate quantum mechanics course several times, using the same measurement algebra formalism abstracted from the Stern–Gerlach experiment that he used in his graduate course; these courses were quite exciting, although perhaps not completely successful.

recounting of the little-known history of Herapath and Waterston, with applications to the present, as Schwinger saw it. He also published an interesting note concerning a fundamental classical problem, 'Electromagnetic mass revisited' [198], which was dedicated to Dirac on the occasion of his 80th birthday in 1982.

It may seem surprising that Schwinger was attracted to UCLA in the first place as an ordinary professor. After all, at Harvard he had a chair, the Higgins Professorship, and the University of California did possess a significant honor, the University Professorship, which allowed the holder to teach at any campus of the UC system. Edward Teller, for example, was a University Professor. But apparently by the 1970s that program had been frozen. When a thaw appeared in the middle of that decade, the Physics Department at UCLA nominated Schwinger for the professorship, but the position went nowhere. It did not help that Feynman's secretary, who screened all of his mail, sent back a form letter stating Feynman's policy was never to write letters of recommendation for people who had been at an institution more than a few years. It is hard to imagine that Feynman, had he known of the request, would not have scribbled a brief note of support for his old friend. In any case, the effort was eventually successful, and Schwinger was named a University Professor in 1980. Saxon has a different perspective on this honor. Although he did not recall that Schwinger had received the award (Saxon was President of the entire University of California system until 1983), he remarked that 'the criteria were not distinction. It had to do with teaching, the willingness to teach on various campuses. Alvarez and all those guys, none of them had it. Lots of University Professors were not Nobel Laureates and vice versa.'[39]

This, perhaps, is the point at which to recall another honor received by Schwinger, the Monie Ferst Medal given by the Georgia Institute of Technology chapter of Sigma Xi. This was an 'award for someone who had distinguished himself through his teaching of science. It's generally given for people who have done a lot in graduate education. The first recipient was E. Bright Wilson, Jr.'[26] The associated Symposium, held on 20 May 1986, consisted of technical talks by three of his former students, Milton, Kenneth Johnson, and Margaret Kivelson, and a provocative talk by Schwinger on 'Accelerated observers and the thermal power spectrum of the vacuum.' This talk reported his work on the so-called Unruh effect,[42] to which we will return later in this chapter.* Although he had spent considerable time working on this project, and given a most interesting presentation on the subject, he did not then, or later, ever publish this work. All that was printed is his abstract for the symposium: 'Source theory, with

* This interest may have been sparked by a 1983 letter from Kirk McDonald of Princeton, on the Unruh effect.[11]

its foundation in idealizations of particle emitters and absorbers (detectors), provides a natural, self-contained approach that is intermediate between the mathematical attitudes of quantum field theorists and the physical consideration of specific detection mechanisms. The periodicity inherent in the circular coordinate form of the Euclidean Green's function, as transformed into hyperbolic (Rindler) coordinates, immediately yields the characteristic property of a thermal Green's function. The explicitness with which this can be done assists in recognizing that, despite the thermal nature of the spectrum, there are definite phase relations that would show up in other experiments.'[43]

Schwinger also gave a talk on the statistical atom there, which included stories about Maxwell and Einstein. 'Every newly appointed professor at Cambridge University was expected to give an inaugural lecture on his latest ideas, which his distinguished colleagues would attend as a sign of respect. Now Maxwell had a mischievous sense of humor, so somehow his inaugural lecture was so obscurely announced that only a few students attended. And then when an elementary course on heat was announced in the usual way, the truly great men of Cambridge, thinking this was the inaugural lecture, dutifully came to the first lecture, and were doubtless puzzled to hear Maxwell, with a twinkle in his eye, carefully explain the difference between the Fahrenheit and the Centigrade temperature scales.' And this was the story about Einstein: 'In 1921 Einstein traveled to the US with Chaim Weizmann—the future first president of Israel—who was a chemist. Concerning their Atlantic voyage, Weizmann said "Einstein explained his theory to me every day, and on my arrival I was fully convinced that he understood it." ' This story was to be included in *Einstein's legacy*: Schwinger found it 'delectable,' but the editor thought it was 'counterproductive' so it did not appear.[11] (*Scientific American* editors are notorious for their heavy-handed style.)

The Schwinger Collection at UCLA does possess a few manuscripts on the subject of acceleration and radiation which Schwinger started but did not complete.[11] There is also a draft of a paper with Manuel Villasante, dated 1994, entitled 'Acceleration, black holes, and temperature'; this paper was never completed nor submitted. Villasante received his PhD under Robert Finkelstein's direction, and became Schwinger's postdoc after Englert left. However, they never completed a paper together. The first part of the extant 56-page manuscript is essentially equivalent to the earlier solo effort of Schwinger on accelerated detectors; Villasante's contribution largely consisted of extending the ideas to a Schwarzschild space, hoping thereby to make the connection with Hawking radiation.[44] In fact, Villasante recalled they only had one brief conversation about the work, late at night, and Schwinger admitted that he, like Villasante, was confused about some points. Schwinger never responded to his earlier or subsequent messages.[45,*] Schwinger apparently never felt this work was complete enough.[48] After spending a year and a half in Italy, Villasante returned and asked Finkelstein to intercede, but evidently Schwinger did not want to be bothered about it and suggested that Villasante write it up on his own, with due acknowledgement; but Villasante never got around to it.[45] (This again illustrates the rule that for Schwinger's collaborative efforts, the burden of completing the paper fell on the collaborator.)

In March 1988 another symposium in honor of Schwinger's birthday, this time his 70th, was held at UCLA. It was rather more modest than the 60th birthday event, but there were still six talks by students and friends: John S. Bell on 'Toward an exact quantum mechanics,' Lowell S. Brown on 'Experiments on the electron magnetic moment: a barrier at twelve significant figures?,' Sheldon Lee Glashow on 'Benzene as jet fuel' (somewhat facetious, but anti-string theory), Yoichiro Nambu on 'The BCS theory and the sigma model revisited,' John H. Schwarz on 'Some hot topics in superstring theory,' and Gerard 't Hooft 'On the quantization of space and time.' These were collected in a slim volume published by World Scientific.[25] This birthday also marked Schwinger's retirement, which meant only the end of his teaching, which, however, we have seen was the source of much of his research stimulation. We may speculate that the decline in the quality of Schwinger's work which followed may have been, in part, attributable to the removal of contact with the real world of students and postdocs.[†] He no longer had any reason to come down the hill to UCLA.

## Cold fusion

In March 1989 began one of the most curious episodes in physical science in this century, one that initially attracted great interest among the scientific as well as the lay community, but which rapidly appeared to be a characteristic example of 'pathological science.'[‡] The effect to which we refer was the announcement

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* Schwinger also had a Greek student in his last years, Evangelos Karagiannis, who did his PhD on a related topic,[46] but 'he also found it impossible to get hold of Schwinger for anything.'[45] What these late collaborators failed to appreciate was that 'Schwinger was easy to work with if you wanted inspiration,' but not if you wanted guidance.[47]

† Sometime around this point Schwinger attempted to hire Elizabeth Simmons, then of Harvard and an SSC fellow, and now of Boston University, as a postdoc. Recall that Schwinger had been granted a perpetual postdoctoral position, a continuation of the position of 'assistant' he had at Harvard.[11]

‡ This term was coined by Irving Langmuir in 1953 who gave a celebrated lecture at General Electric's Knolls Atomic Power Laboratory (transcribed from a disk recording by Robert Hall) on the phenomenon wherein reputable scientists are led to believe that an effect, just at the edge of visibility, is real, even though, as precision increases, the

by B. S. Pons and M. Fleischman[50] of the discovery of cold fusion. That is, they claimed that nuclear energy, in the form of heat, was released in a table-top experiment, involving a palladium cathode electrolyzing heavy water.

Of course, the scientific community reacted with considerable skepticism. After all, it had required prodigious effort to build a hydrogen bomb, in which hydrogen is fused to form helium, with a tremendous release of energy, but only under conditions of extreme temperature and pressure, conditions rivaling those at the center of the Sun, where similar reactions take place. How could the same reaction occur, albeit at a much slower pace, at room temperature, in a glass of water? Vast sums have been spent to control nuclear fusion by containing hot ions by magnetic and inertial confinement, with only modest success, and with commercial fusion reactors, even in the eyes of the most optimistic, always many decades away in the future. Here at no cost, a solution to the energy crisis was promised. It was too good to be true.

And indeed it was. But Pons and Fleischman were respectable chemists, and people initially took their work seriously. Informal networks spreading information about new calculations and experiments related to cold fusion sprang up quickly on the still relatively primitive Internet. Bright nuclear physicists spent hours re-examining calculations and searching out new ideas to see if somehow such a thing could work. How could the nuclei of the deuterium atoms, the deuterons, get close enough together for fusion to have an appreciable chance to take place? The problem is that the nuclei are positively charged, so that Coulomb repulsion presents a very high barrier to overcome. It seemed impossible, yet just maybe ... ?

But rather quickly, the experimental situation began to unravel. Immediately, people looked for neutrons, which would be produced in the expected reaction, $d + d \rightarrow n + {}^3He$, but none were found, nor were $\gamma$-rays, if the reaction was $d + d \rightarrow \gamma + {}^4He$. Some other experimenters claimed to see similar energies released, but most did not. Then there was the famous alteration of the figure in the original paper, between the preprint and the publication. So, although the subject continued to receive serious attention for the rest of 1989, by the summer of that year most believed it was pathological at best, fraudulent at worst. This became confirmed a year later, when the cold fusion conferences

effect remains marginal. The scientist becomes self-deluded, going to great lengths to convince one and all that the remarkable effect is there just on the margins of what can be measured. Great accuracy is claimed nevertheless, and fantastic, *ad hoc*, theories are invented to explain the effect. Examples include N-rays, the Allison effect, flying saucers, and ESP. It was not a coincidence that *Physics Today* published the article, without comment, in the fall of 1989.[49]

were effectively closed to all but true believers. (For a detailed history of this whole affair, see Ref. 51.*)

So it was a shock to most physicists† when Schwinger began speaking and writing about cold fusion, suggesting that the experiments of Pons and Fleischman were valid, and that the palladium lattice played a crucial role. In one of his later lectures on the subject in Salt Lake City, Schwinger recalled, 'Apart from a brief period of apostasy, when I echoed the conventional wisdom that atomic and nuclear energy scales are much too disparate, I have retained my belief in the importance of the lattice.'[11] His first publication on the subject was submitted to *Physical Review Letters*, but was roundly rejected, in a manner that Schwinger considered deeply insulting. In protest, he resigned as a member (he was, of course, a fellow) of the American Physical Society, of which *Physical Review Letters* was the the most prestigious journal. (At first he intended merely to withdraw the paper from *PRL*, and his fellowship, but then he felt compelled to respond to the referees' comments: one comment was something to the effect that no nuclear physicist could believe such an effect, to which Julian angrily retorted, 'I am a nuclear physicist!'[11]) In this letter to the editor (G. Wells) in which he withdrew the paper and resigned from the American Physical Society, he also called for the removal of the source theory index category the APS journals used: 'Incidentally, the PACS entry (1987) 11.10.mn can be deleted. There will be no further occasion to use it.'[37, 11] A rather striking act of hubris: if he couldn't publish source theory, neither could anybody else. But the *Physical Review* obliged.

Not wishing to use any other APS venue, he turned to his friend and colleague, Berthold Englert, who arranged that 'Cold fusion: a hypothesis' be published in the *Zeitschrift für Naturforschung*, where it appeared in October of that year [213]. In that brief note, he advanced the hypothesis that the claim of cold fusion was valid, but instead of a deuteron–deuteron reaction, it was a hydrogen–deuteron reaction, 'which feeds on the small contamination of $D_2O$ by $H_2O$.' The reaction therefore was $p + d \rightarrow {}^3He$. The HD reaction is much more

* J. Huizenga, a physical chemist, and Norman Ramsey were co-chairmen of a DOE panel set up to investigate the cold fusion claims. Huizenga was, in fact, far more negative than was Ramsey, who attempted to keep an open mind on the subject. He felt that although the experiments were probably wrong, they could not judge that every claim was false. He therefore retained more sympathy for Schwinger's efforts to understand the phenomenon, although he believed that Schwinger may have been fooled. (For example, the experimenters found helium in the cell, but it turned out they had used a helium leak detector.) It is easy to be misled by coherent effects: 'nature does not like coherence.'[52]

† However, a few other eminent physicists spoke favorably of the possibility of cold fusion, notably Edward Teller and Willis Lamb, who published three articles in the *Proceedings of the US National Academy of Sciences* on the subject.

probable than the DD reaction, which in turn is much more likely to occur than HH. Thus, the absence of neutrons was no surprise. (However, a photon is liberated, which Schwinger assumed was absorbed by the lattice.)

This point of Schwinger's was very well taken, and is referred to appreciatively by Douglas R. O. Morrison in a letter in *Physics Today*. He pointed out that he suggested at the Fourth International Conference on Cold Fusion that experimenters test Schwinger's hypothesis by varying the ratio of $D_2O$ to $H_2O$. 'But surprisingly, no one has followed Schwinger's advice even though it is based on well-known rates. On the contrary, several experiments claim to have observed cold fusion with the HH reaction—which was used by Fleischman and Pons as a control giving no fusion. Thus the experimental claims for cold fusion are in contradiction to the hierarchy of rates . . . .'[53]

The second, crucial, aspect of Schwinger's theory was that excess energy of the HD reaction is carried off 'by the metallic lattice of Pd alloyed with D.' [213] This lattice somehow also acts to suppress the electrostatic repulsion between the positively charged nuclei, 'and, indeed to overcome it with an energy of attraction that significantly ameliorates the effect of the Coulomb barrier penetration.' [213]

The problem with the lattice idea, as Morrison pointed out, was one of incommensurate time-scales: 'the fusion reaction takes place in less than $10^{-20}$ seconds while the time for the energy to spread among $10^7$ nuclei of the lattice is greater than $10^{-15}$ seconds.'[53] Thus, the scheme was completely unworkable. This meant that there was no way to avoid the problem of the photon produced in the HD reaction, which Schwinger did not emphasize, merely stating 'after the *pd* fusion begins, the liberated energy is transferred to the multiphonon degrees of freedom of the lattice, rather than to a single high energy photon.' [214] And where was the $^3$He?[51]

Schwinger then went on to write three substantial papers, entitled 'Nuclear Energy in an Atomic Lattice I, II, III,' to flesh out these ideas.[28, 11] The first was published in the *Zeitschrift für Physik D* [214], where it was accepted in spite of negative reviews,[11] but directly preceded by an editorial note written, presumably, by the editor, V. Hertel: 'Reports on cold fusion have stirred up a lot of activity and emotions in the whole scientific community as well as in political and financial circles. Enthusiasm about its potential usefulness was felt but also severe criticism has been raised. If in such a situation one of the pioneers of modern physics starts to attack the problem in a profound theoretical way we feel that it is our duty to give him the opportunity to explain his ideas and to present his case to a broad and critical audience. We do, however, emphasize that we can take no responsibility for the correctness of either the basic assumptions and the validity of the conclusions nor of the details of the calculations. We leave the final judgment to our readers.'[54] According to Englert,[28] Schwinger was not too bothered by this preface, and assumed that they would similarly publish the two following papers. But they would not. Englert retained the response of the referee to the second paper, which stated, in part, 'The paper "Nuclear Energy in an Atomic Lattice. 2" is interesting and seems technically, mathematically in order. However, from the physical point of view [the need to transfer 5.5 MeV of heat to the lattice] this paper (as well as the preceeding one) is unsatisfactory and unclear.'[37]

The third paper was rejected with 'all due respect to Prof. Schwinger, I do *not trust* the physical implications of this paper. The mathematics is alright, but the physical picture is fantastic. ... '[37] Julian responded heatedly: 'I am bewildered. I thought we had an agreement: in exchange for your Editorial Note, in which you disclaim all responsibility and leave matters to the judgement of the readers, you will accept your duty to give me an opportunity to present my case. Indeed, Nuclear energy in an atomic lattice. 1 has been published. But, as indicated by the number, that is only the beginning of the presentation. Why, then, were NEAL 2 and NEAL 3 sent to referees with (at least for 2) the predictable rejections?

'And such rejections! After conceding that no flaw in the development could be found, some past experience is adduced to declair [sic] that it has to be all wrong. That is their intuition. Why is it automatically better than mine? As you said, let a broad audience decide. Peer review based on rigid preconception is censorship.

'I invite you to reread your Editorial Note, and publish NEAL 2 and NEAL 3. Be assured that, after that, I shall not trouble you again.

'I do not want to overburden you; no futher papers in the NEAL series will be sent to [*Z. Phys.*].'[37]

Another note to an editor, presumably about the same time, reveals his increasing frustration with referees. 'I am getting too old to put up with nonsensical flack from wet behind the ears referees who know and admit nothing beyond the latest bandwagon. I am well aware that charges of discrimination and prejudice also come from the lunatic fringe. It is up to you to have the common sense to know the difference.'[11]

The published NEAL I paper was substantially shortened in proof. In a letter to Englert, asking him to delete the last page of proof, Schwinger pointed out his error: '$r \ll \Lambda$ does not mean that the proton is near the neutron.' This had potentially serious implications for his estimate. 'Whether the initial HD hypothesis can be maintained, I don't know.'[11]

Although the second and third 'Nuclear energy in an atomic lattice' papers exist only in the archive,[11] there were three short follow-up publications, in the *Proceedings of the U.S. National Academy of Sciences* [216, 217, 220], all concerned with the lattice dynamics and phonons, although cold fusion is not explicitly mentioned in any of these. A letter in *Progress of Theoretical Physics* is entitled 'Nuclear energy in an atomic lattice' [219]. In this very short note, with no equations, and but a single reference, to a talk he gave at the Yoshio Nishina Centennial Symposium [218a], he suggested that the usual casual order, which would seem to preclude penetration of the Coulomb barrier, could be altered by the presence of the lattice! He concluded by stating that 'It is not my intent—nor would I be qualified—to declare the reality of the evidence offered for what has been called cold fusion. Rather, I only point out that the argument that has produced contemptuous dismissal of the possibility could be based on a false premise. The subject requires research, not fiat.' [219]

The UCLA archives contain lectures that Schwinger gave in the 1990s on cold fusion and related topics. These include 'A progress report: energy transfer in cold fusion and sonoluminescence,' given in honor of birthday celebrations at MIT in 1991 (we will discuss this lecture in connection with sonoluminescence in the following section), and 'Cold fusion: a brief history of mine,' which is a good history of the subject from Schwinger's perspective, given in 1993, which again made the connection between cold fusion and sonoluminescence.

Englert stated that by the early 1990s Schwinger had decided that his cold fusion papers could not be completely right.* By then, even he must have seen that the evidence for cold fusion could no longer be taken seriously. We must ask why he was so eager to jump on the cold fusion bandwagon, when most physicists approached the matter cautiously. The answer, in large part, may be found in his own experience. The rather 'contemptuous dismissal' of his source-theory program by most of the field theory community made him sensitive to the plight of the underdog. He always insisted on understanding things his own way. His way was often called conservative,[55] yet this, as we have seen, is a great oversimplification. His openness to the cold fusion hypothesis may have been unfortunate, but was completely consistent with his attitude to science. As he concluded his Sigma Xi lecture on 'Conflicts in physics' with a quotation from Boltzmann: 'Who has seen the future? Let us have free scope for all directions in research; away with all dogmatism.'[11]

Schwinger continued to hold his open-minded view of cold fusion to the end. His files contain contain an article in the *New Scientist*, dated 8 January 1994, stating that long ago Reifenschweller and Casimir had observed a decrease in radioactivity in electrolysis, but, of course, without any cold fusion claim.

* Possible objective evidence for this lies in the fact he had his invited paper read by Eugene Mallove at the International Conference on Cold Fusion IV, held in Hawaii in December 1993, begging off attending because of his new-found intolerance for jet lag—which, on the other hand, may well have be a completely legitimate excuse.[11] Schwinger was diagnosed with pancreatic cancer just two months later.

Schwinger scribbled on the margin that this would be explainable by the fusion reaction $^3H + {}^1H \rightarrow {}^4He$ (in the presence of titanium).[11]

Robert Finkelstein, who had lunch with Schwinger regularly during this period, emphasized that Schwinger thought that 'conditions in cold fusion were very different from those holding in hot fusion,' and that therefore physicists should keep an open mind about unexpected possibilities.[48] Schwinger put it well in a lecture he gave at the Université de Bourgogne in February 1990: 'The usual viewpoint, derived from the hot fusion experience, is that the rate of the fusion reaction is the product of the penetration factor for the repulsive Coulomb barrier—with the intrinsic rate of fusion. The two factors: penetration probability and intrinsic fusion rate are independent.

'But in the very low energy cold fusion, one deals essentially with a single state, described by a single wave function, all parts of which are *coherent*. A separation into two independent, incoherent factors is not possible, and all considerations based on such a factorization are not relevant.'[11]

Morrison concluded his letter by complaining that Schwinger would not accept criticism of his cold fusion work. That may well be true, yet his example was not persuasive: 'During a visit to the University of California, Los Angeles, I tried to contact him, but was told that he was virtually unseen on campus. When he was phoned at home, a charming lady [Clarice] explained it was not possible just then, and what can you do when such a person says, "he had a special glint in his eye this morning and I am sure he has a new idea, so I could not possibly disturb him"? Letters remained unanswered.'[53] As we have seen, this was Schwinger at work throughout his life, not just during this, perhaps, unfortunate period.

## The Casimir effect and sonoluminescence

Schwinger's last physics endeavor marked a return to the Casimir effect, of which he had been enamored nearly two decades earlier. It was sparked by the remarkable discovery of single-bubble sonoluminescence. It was not coincidental that the leading laboratory investigating this phenomenon was, and is, at UCLA, led by erstwhile theorist Seth Putterman, a longtime friend and confidant. Putterman and Schwinger shared many interests in common, including appreciation of fine wines, and they shared a similar iconoclastic view of the decline of physics. So, of course, Schwinger heard about this remarkable phenomenon from the horse's mouth, and was greatly intrigued.

What is sonoluminescence? The word means the conversion of sound into light. As such, it had been observed since the 1930s,[56] but this so-called multiple bubble sonoluminescence was hardly investigated, and was nearly completely forgotten by the last decade of this century. Not completely, because Tom Erber, on one of his many visits to UCLA told Putterman about this old effect,[1] and in short order, a much more remarkable version of the effect was discovered. If a single bubble of air is injected into a beaker of water, and held in a node of a standing acoustic wave set up in the water, the bubble will begin to expand and contract in concert with the frequency of the standing wave. If the ultrasonic wave has a frequency of about 20 000 Hz, and a pressure amplitude of about one atmosphere, a small suspended bubble of air will expand and collapse 20 000 times a second, undergoing a change in radius of a factor of 10 or more (and hence, in volume, of at least a factor of 1000), from, say $4 \times 10^{-3}$ cm to $4 \times 10^{-4}$ cm. If the parameters are chosen just right (a small percentage of noble gas, for example, the amount of argon in our atmosphere, seems essential), exactly at minimum radius a bright flash of light is released from the bubble. This flash of light consists of approximately one million optical photons, so that about 10 MeV of energy is converted into light on each collapse. This flash of light, integrated over many cycles, is bright enough to be visible to the naked eye if the water is observed in a darkened room. (The authors have seen this effect for themselves.) Whatever produces the flash of light is sufficiently non-catastrophic that it does not in any way disrupt the bubble, and the periodic collapse and re-expansion continues for many minutes, perhaps months. For a review of the experimental situation, see Ref. 57.

The hydrodynamics of the bubble collapse and re-expansion appears to be quite well understood. What is not understood at all is how some of the energy in the bubble, extracted from the sound field, is converted into the intense flash of light. The duration of the flash has not been determined, but it is less than $10^{-11}$ seconds, much smaller than the period of the bubble collapse, but apparently long compared with the period of optical photons (about $10^{-15}$ seconds). Although there have been various classical and quantum hypotheses put forward, they tend not to be, in the words of Putterman, 'falsifiable.'[57]

Of course, Putterman told Schwinger about the phenomenon right away. He called Schwinger at home, and immediately Schwinger drove down to see it. At first Schwinger had difficulty in seeing the faintly glowing bubble. Putterman told him to 'look at $r = 0$,' and soon he saw the bubble at the center of the spherical vessel. Schwinger's reaction was 'I'm shaken.' He at once started work on the problem of understanding what was happening.

Schwinger immediately had the idea that a dynamical version of the Casimir effect might play a key role. In a letter to Putterman 'Re: nanosecond sonoluminescence' wherein he proposed the Casimir effect mechanism, presumably written on Martin Luther King Day, Schwinger opened with a quotation: 'MLK: "I have a dream." JS: "I have a feeling."'[11] The idea was that the virtual photons present, due to the Casimir effect, or in conventional language, vacuum fluctuations, in a bubble in a dielectric medium could be converted into real photons because the radius of the bubble is rapidly changing. This was, in fact, closely related to the so-called Unruh effect[42] in which an accelerated mirror radiates a blackbody spectrum of photons, or in which an accelerated observer sees such a thermal spectrum—in turn closely allied with Hawking radiation from a black hole.[44] (Recall that Schwinger had worked for a while on the Unruh effect in the mid-1980s into the 1990s, although he never completed a paper on the subject.*) So there were two challenges for Schwinger. One was to develop the 'dynamical Casimir effect' for the spherical geometry of a bubble, and the second was to apply that effect to the hydrodynamic situation of a collapsing bubble in sonoluminescence.

Early in the process he gave the talk 'A progress report: energy transfer in cold fusion and sonoluminescence.' As we recall, this was a talk given at MIT in 1991 'to celebrate the birthdays of former students.' He opened by recalling that he had first arrived in Berkeley on the day World War II began in Europe, and shortly thereafter Oppenheimer gave a talk entitled 'A progress report,' signifying no great research breakthroughs, but simply the passage of time. Schwinger modestly began by quoting Mort Sahl: 'The future lies ahead.' He devoted most of the talk to cold fusion, but in the last two pages of the typescript turned to another phenomenon in which there was a 'focusing or amplification of about eleven orders of magnitude,' namely sonoluminescence. He recounted the history, beginning with the sea trials of the British destroyer *HMS Daring* in 1894, where serious propeller vibrations led to the discovery of bubbles forming and collapsing, that is, cavitation. Lord Rayleigh, who was brought in for consultation years later during World War I, indeed identified cavitation as the culprit, but he made a serious error in the theory, in assuming that the process was isothermal (constant temperature) rather than adiabatic, in which the entropy is constant. Still later, in 1927, when high-intensity sound waves produced cavitation in water, it was found that hydrogen peroxide was formed, leading to the suggestion that light could be formed also, as was found in 1934. However, only in 1970 was it found that such sonoluminescence could occur without cavitation noise, and, as noted above, single-bubble sonoluminescence (SL) was not discovered until 1990. 'When I first heard about coherent SL, some months ago, my immediate reaction was: This is the dynamical Casimir effect.

* In 1990, just before single-bubble sonoluminescence was discovered, Schwinger wrote a manuscript entitled 'Superluminal light' (a later version was called 'Tachyonic light') which was a reaction to the claim by K. Scharnhorst and G. Barton that light speeds greater than the speed of light in vacuum are possible in a parallel plate capacitor, the original Casimir effect geometry, indeed as an induced consequence of the Casimir effect.[58] Unlike them, Schwinger found the effect was non-uniform, dispersive (that is, frequency-dependent), and that the effect persisted if only a single plate was present.[11]

... The mechanisms that have been suggested for cold fusion and sonoluminescence are quite different. But they both depend significantly on nonlinear effects. Put in that light, the failures of naive intuition are understandable. So ends my Progress Report.'[11]

The first substantive step in the process was documented in two papers Schwinger published in *Letters in Mathematical Physics*, edited by the frequent UCLA visitor Moshé Flato, as sequels to his first, 1975, Casimir paper [174], also published in the same journal. In the first [221], he derived the original Casimir effect for parallel conducting plates by an elegant proper-time approach, while in the second [222], he reconsidered dielectric slabs. In both cases, the emphasis was on energy rather than force. He followed* this by two somewhat longer articles in the *Proceedings of the U.S. National Academy of Sciences.* (Unlike Feynman,[59] Schwinger continued throughout his career to find the Academy a useful scientific venue.) In the first, he rederived, for the third time, the Lifshitz theory for the Casimir effect between parallel dielectric slabs [223], in an efficient way making use of an explicit break-up into Transverse Electric (TE) and Transverse Magnetic (TM) modes. As had been done in his earlier collaborative work [187], he explicitly removed volume and surface energies: 'One finds contributions to $E$ [the energy] that, for example, are proportional ... to the volume enclosed between the slabs. The implied constant energy density—independent of the separation of the slabs—violates the normalization of the vacuum energy density to zero. Accordingly, the additive constant has a piece that maintains the vacuum energy normalization. There is also a contribution to $E$ that is proportional to [the area], energy associated with individual slabs. The normalization to zero of the energy of an isolated slab is maintained by another part of the additive constant.' [223] Then he turned to the case of interest for sonoluminescence: spherical dielectrics. In 'Casimir energy for dielectrics: spherical geometry' [224] he began an elegant treatment of the Casimir effect in that situation. Unfortunately, he only treated the TE modes, and went only far enough to see that the parallel geometry result was recovered if a careful limit of the radius of the sphere going to infinity is taken. Explicitly, he left the details to Harold!

But Harold, or Sagredo,† had been over this ground already. Fifteen years earlier, while still at UCLA, Milton had computed the Casimir effect for a dielectric ball.[60] Perhaps Schwinger can be forgiven his ignorance of his former student and postdoc's work by the fact that this paper was completed and

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* Actually [223] was submitted essentially simultaneously with [222].

† In *Particles, sources, and fields* [153], p. 241, S[chwinger] explicates this Galilean confusion; see p. 472.

published after Milton had gone to Ohio State.* In any case, Schwinger did not get far enough with this calculation to apply it to sonoluminescence. Instead, when he started to develop his theory of sonoluminescence in a series of five papers† in the *Proceedings of the U.S. National Academy of Sciences* [225–228, 230] he simply wrote down a naive approximation for the Casimir energy obtained, in effect, by subtracting the zero-point energy of the vacuum from that for the medium, giving the quartically divergent formula,

$$E_{\text{bulk}} = \frac{4\pi a^3}{3} \int \frac{(\mathrm{d}k)}{(2\pi)^3} \frac{1}{2} k \left(1 - \frac{1}{n}\right), \tag{15.25}$$

where $n$ is the index of refraction of the medium. Schwinger had forgotten his own requirement of subtracting from the volume energy that term which 'would be present if either medium filled all space.' Since this expression is very divergent, it is extraordinarily sensitive to the cutoff which must be used on physical grounds to give a finite result. However, if a plausible ultraviolet cutoff were used, Schwinger obtained a sufficiently large Casimir energy, $E_{\text{bulk}} \sim 10$ MeV.

The problem is that the bulk energy Schwinger considered is not relevant to sonoluminescence.[60] It is, in fact, a kind of self-energy, one that contributes to the density of the water, and of the gas, that is already phenomenologically described. As further noted above, the same is true of the surface energy, it being subsumed into the definition of the surface tension. The correct conclusion from the calculation of Ref. 60 and Ref. 61 is that the Casimir energy is very small (in the simple approximation of dilute media),

$$E_C = \frac{23(n-1)^2}{384a}, \tag{15.26}$$

which amounts to only about $10^{-3}$ eV in the case of sonoluminescing bubbles, and is therefore completely irrelevant to sonoluminescence.

### The dynamical Casimir effect

But these considerations are static, which are appropriate only if the time scale of the flash is long compared with the time-scale of optical photons, about $10^{-15}$ s. A simple argument suggests that this is a reasonable assumption if one wishes to avoid the walls of the bubble having speeds in excess of the speed of

* However, in Milton's last meeting with Schwinger, in December 1993, Schwinger did not wish to be reminded of this earlier work.

† There are notes for at least three further papers in Schwinger's files on 'Casimir light,' the last being subtitled 'A study in green.' These must represent his last scientific work.[11]

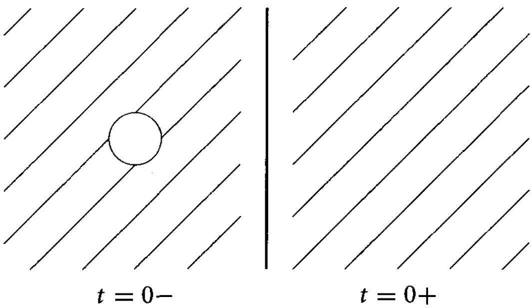


Fig. 15.1 The sudden collapse of an otherwise static bubble.

light. However, we must remain open to the possibility that discontinuities, as in a shock, could allow changes on such short time-scales without requiring superluminal speeds. Indeed, Schwinger suggested [225, 227] that the bubble collapsed on an extremely short time scale, so that rather than the slowly varying (adiabatic) approximation discussed above being valid, a sudden approximation is more appropriate. We therefore turn to an analysis of that situation.

The picture offered is that of the abrupt disappearance of the bubble at $t = 0$, as shown in Fig. 15.1. On the face of it, this picture seems preposterous—the bubble simply disappears and water is created out of nothing. It may be no surprise that a large energy release would occur in such a case. As in Schwinger's papers, let us confine our attention to the electric (TM) modes. They are governed by the time-dependent Green's function satisfying

$$(\partial_0 \epsilon(x) \partial_0 - \nabla^2) G(x, x') = \delta(x - x'), \tag{15.27}$$

where $\epsilon(x)$ is the space-time varying dielectric constant, related to the index of refraction by $n = \sqrt{\epsilon}$. The photon production is given by the effective two-photon source

$$\delta(JJ) = \mathrm{i}\delta G^{-1} = \mathrm{i}\partial_0 \delta\epsilon(x) \partial_0. \tag{15.28}$$

A straightforward calculation leads to the probability of emitting a pair of photons with momenta k and $-$k (this now includes the equal contribution from the magnetic modes):

$$P_{\gamma\gamma} = 2v \frac{(\mathrm{dk})}{(2\pi)^3} \ln \frac{\epsilon^{1/4} + \epsilon^{-1/4}}{2}, \tag{15.29}$$

where $v$ is the volume of the bubble. If $|\epsilon - 1|$ is small, this reduces to

$$P_{\gamma\gamma} = v \frac{(\mathrm{dk})}{(2\pi)^3} \left( \frac{\epsilon - 1}{4} \right)^2, \quad |\epsilon - 1| \ll 1. \tag{15.30}$$

Numerically, this approximation is good enough for a first estimate. The total number of photon pairs emitted is then, if dispersion is ignored,

$$N = \left(\frac{4\pi}{3}\right)^2 \left(\frac{a}{\Lambda}\right)^3 \left(\frac{\epsilon - 1}{4}\right)^2, \tag{15.31}$$

where the cutoff wavelength is $\Lambda$. Such a divergent result should be regarded as suspect.* It was Claudia Eberlein's laudable goal[62] to put this type of argument on a sounder footing. Nevertheless, if we put in plausible numbers, $\sqrt{\epsilon} = 4/3$, $a = 4 \times 10^{-3}$ cm, and, as in Schwinger's earlier estimate, $\Lambda = 3 \times 10^{-5}$ cm, we obtain the required $N \sim 10^6$ photons per flash.†

One problem with this estimate is one of time and length scales—for the instantaneous approximation to be valid, the flash time $\tau_f$ must be much less than the period of optical photons, $\tau_o \sim 10^{-15}$ s. This would seem to imply superluminal velocities. On the other hand, the collapse time $\tau_c \sim 10^{-5}$ s is vastly longer than $\tau_f$, and is therefore totally irrelevant to the photon production mechanism. The flash occurs near minimum radius, and thus the appropriate value of $a$ in Eqn (15.31) would seem to be at least an order of magnitude smaller, $a \sim 10^{-4}$ em. This would lead to $N < 10^3$ photon pairs, totally insufficient.

Schwinger's final paper,‡ on sonoluminescence, [230] was published in the month of his death. As we noted he was typically unaware of some of his own colleagues' papers relevant to the subject, but, atypically, he was very explicitly seeking Milton's collaboration in the last year of his life (Milton talked to him at some length in December 1993, at the annual Christmas party given by the Bañoses', which he and Clarice often attended,§ and at a subsequent lunch). He also arranged to have the earlier papers, on Casimir energy, but not the later

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* Although it is not clear how this is to be related to the divergent energy (15.25), Schwinger obtained both in [227] as the imaginary and real parts, respectively, of a complex action.

† An equivalent argument has recently been given by Liberati *et al.*[63]

‡ However, as we have noted, he had several other projects in the works. In addition to further papers on sonoluminescence, and on the Unruh effect, he wrote three letters to semi-popular science journals on quantum mechanics during the last few months of his life. Two were published in *Science*: 'Quantum mechanics: not mysterious' (originally the subtitle was 'Different? Yes. Mysterious? No.'), which attacked a 'Research news' article by David Freedman describing a 'protective' Stern–Gerlach experiment proposed by Aharonov, Anandan, and Vaidman;[64] and 'Quantum uncertainty principle: no loopholes,' attacking another 'Research news' article, this time by Gary Taubes.[65] He also wrote a letter to *Scientific American* responding to David Albert's article in the May 1994 issue on the Stern–Gerlach experiment, but he did not live long enough to see that through to publication.[11]

§ His role was to hide the three kings in the Christmas tree.

ones, on sonoluminescence, sent to Milton. Schwinger felt that 'carrying out that program is—as one television advertiser puts it—job one' [229]. It seems apparent that he was aware of the inadequacies of his treatment of the Casimir effect, and was looking for additional expertise and strength. The subject is not completely closed, because there are serious subtleties in these Casimir calculations, the adiabatic approximation (that is, treating the bubble radius as slowly varying on an electromagnetic time scale) may be invalid, and most likely a shock forms, which allows for discontinuities on very short time-scales. So, as noted, Schwinger's ideas here are still being explored. Perhaps Julian Schwinger will ultimately have the last laugh!

As we have noted, Schwinger explicitly and implicitly drew parallels between cold fusion and sonoluminescence. At first blush this seems implausible. After all, sonoluminescence without doubt exists, while cold fusion does not. But Schwinger's point was one of overcoming seemingly impossibly different scales. In the case of cold fusion, how can the Coulomb barrier be overcome at very low energies; in the case of sonoluminescence, how could hydrodynamics, characterized by acoustic phonons, couple to quantum electrodynamics, characterized by much higher energy photons? It is natural that he would find the attempt to solve these conundrums challenging. And, as it became increasingly untenable to pursue cold fusion, he shifted his efforts toward the experimentally confirmed sonoluminescence.

Seth Putterman recounted his final meeting with Schwinger two days before his death. Schwinger did not want to talk about history, but about physics, and wanted to know what was new in sonoluminescence. Putterman told him of the puzzling fact that water was the 'friendliest' liquid for the phenomenon, and that the effect only appeared if about 1% noble gas was present. Schwinger thought for a bit, and said, 'It probably has something to do with evolution.'[1] Heady stuff indeed!

## Conclusions

How do we place the last 25 years of Schwinger's career in context? It seems that a number of general conclusions may be drawn.

We have argued, in Chapter 13, that source theory was not so abrupt a break with the past as Schwinger presented it. It becomes increasingly clear as one reads his treatise *Particles, sources, and fields* [211], or his general *oeuvre*, that he returned to techniques which he had invented in the 1940s and 1950s. Examples are 'non-causal methods' which can be found in his famous 1951 'Gauge invariance and vacuum polarization' (GIVP) paper [64], strong field methods, which go back to his early work on synchrotron radiation [56, 78] (and also GIVP), and even the theory of sources, which he introduced also in 1951 [66].

He, of course, was aware of this continuity; but he felt the need to emphasize a rather complete break. He saw a great improvement in conceptual clarity, for when he did operator field theory he carried around a great deal of baggage (which *really* is essential) which most people had dispensed with or ignored. Source theory enabled *Schwinger* to dispense with the 'physical remoteness' [153] of renormalization and confront the physics directly. Undoubtedly, with hindsight, we can say that his later work would have had much greater impact if he had not drawn such an exclusive distinction.

Of course, probably a bigger impediment to the reception of his ideas was a change in the times. Dispersion relations had died as a fundamental contender to field theory before he mounted his attack, and field theory was reborn with the discovery by 't Hooft that gauge theories of weak and strong interactions were renormalizable and hence made sense. Simultaneously, Feynman's path-integral formulation of field theory,[59] particularly of non-Abelian gauge theories, took over nearly completely, and Schwinger's earlier field theoretical developments, such as the quantum action principle, were nearly forgotten by the high energy physics community, although interestingly not so much in other branches of physics. (However, there is increasing evidence that his methods are coming back into vogue.) Schwinger could accept the electroweak synthesis (to which he had contributed so much), but not quarks and QCD. The notion of 'particles' which were not asymptotic states was too distasteful. (Yet his idea of magnetically charged dyons was not so different—maybe it was just the 'unmellisonant' name [150].)

In many of his later projects, the first paper in the series was far and away the strongest. He had a very useful idea in the first deep inelastic scattering paper [167], but thereafter the work increasingly, although by no means entirely, reduced to fitting data with many parameters. To a somewhat lesser extent, a similar characteristic is true of the Thomas–Fermi papers (although here it is the first two papers that stand out). And in the 'dynamical Casimir effect' work there is enough in these many short papers for about one substantial article; the essential calculations to incorporate dynamically changing boundary conditions have yet to be carried out. Moreover, because Schwinger's approximation of keeping the bulk energy contribution is apparently erroneous, the relevance to sonoluminescence remains to be established. Finally, even though his appeal for tolerance is understandable, the whole cold fusion episode did not enhance his reputation.

The last 30 years of his life were not Schwinger's strongest scientifically. Certainly not for lack of ability: he remained an awesome calculator and a brilliant expositor of unconventional and clever ideas. But the times had changed, and Schwinger was no longer the moulder of ideas for theoretical physics. He is sometimes criticized for venturing into phenomenology—but in fact his first, and quite substantial, papers on nuclear physics were phenomenological. [The unfortunate distinction between theory and phenomenology (not one that Schwinger ever made) is a product of the last two decades, as 'theoretical physics' has become increasing disconnected from the real world.] In fact, his approach to physics was always profoundly phenomenological. Much of his criticism of QCD is quite valid—the theory remains on very tenuous ground, and is more of a parameterization of strong interaction physics than the theory of first principles it pretends to be. GUTs and strings he found outrageous not because of their theoretical failings but because he, quite rightly, found the notion of a 'desert' between 1 TeV and the Planck scale completely unbelievable—this was, after all, his reason for inventing source theory, to separate high-energy speculations from models of low-energy phenomena; his view of the world was 'anabatic,' from the bottom up, not 'trickle-down.'

As the continuing influence of Schwinger's work on the 'statistical atom' demonstrates, we should not underestimate the power of his work to have a long-range impact. We can confidently expect future surprises. This may be true as well of the many papers to which we have referred only in passing, because they do not fit into a well-defined pigeonhole, or have tended to be dismissed as mistaken or irrelevant. We can only urge the reader to read his papers, for unmined riches are contained therein.

Eight months before his death, Schwinger made his first appearance on the Internet with his July 1993 Nottingham lecture, 'The Greening of quantum field theory: George and I' (hep-ph/9310283) [229]. This lecture,* delivered on the occasion of an honorary degree, provides a remarkable overview of Schwinger's work from his own perspective. His whole career, nearly, was framed in the language of Green's functions, so it is a natural story. We quoted from it extensively in Chapter 9. He concluded with an 'advertisement' for his theory of sonoluminescence. We conclude this chapter, and our account of the final third of Julian Schwinger's life, by quoting his final words from this lecture: Like George Green, 'he is, in a manner of speaking, alive, well, and living among us.'

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2. Walter Dittrich, letter to K. A. Milton, September 1998.
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16

# The diversions of a gentle genius

## Confessions of a nature worshipper

In November 1973 the *UCLA Monthly*, a periodical for faculty and students of UCLA, published an interview with their newly acquired Nobelist.[1] Since this reveals many of Julian Schwinger's views about science and society, which he seldom shared with the public, it seems appropriate to begin this final chapter by quoting the interview. The questioner was Mark Davidson.

*Do you think scientists are sufficiently concerned about the moral implications of their work?* 'The establishment of the American Federation of Scientists and other such groups is a direct reflection of the scientists' moral and practical concern with the application of their developments. All of this followed the development of the atomic bomb, which was of course the major traumatic experience.

'But at the same time scientists tend to feel that society should not impose any restrictions on the purely research aspects of their work. We feel the scientists should be free to extend the boundaries of man's knowledge no matter where that search leads. How the results of our work are applied then becomes a serious moral question in which all of society should be involved.

'If a chemist, for example, were to discover a gas which might be used for chemical warfare, I do not believe he should then destroy the knowledge of that gas. He should try to make sure it is never used for that purpose. But that's not a question of suppressing the research. It's a matter of changing the whole way of thinking of the people involved in deciding how scientific discoveries are applied.'

*Einstein once wrote that the atomic bomb had changed everything but man's way of thinking, and that must change. In what ways do you think man's thinking must change?* 'Einstein meant that war as a means of deciding questions must be abandoned. And I fully believe, in spite of everything one sees in the newspapers, that man is moving in that direction.

'For example, there was the widespread revulsion against the Vietnam episode. And the gradual inchings toward East–West detente is a hopeful sign. Let's hope the atomic bomb, if it prompted this global trend, will turn out to have been a blessing in disguise.'

*Einstein also wrote that the abolition of war would require the evolution of a patriotism toward the entire human race. Is that your thinking too?* 'Yes. And that's one of the reasons I'm very partial toward the space program. Some scientists rail against the supposed wasting of money on space. But the space program has helped to remove the parochialism of the human race. When man can go up there and look down and see how tiny our sphere is and how we really are all just one people, I think the national barriers and narrow ways of thinking tend to disappear. In that sense I think science may have contributed an enormous step toward making us one people.'

*Do the scientists you've met in various countries tend to think like citizens of the world?* 'Yes, and that's very understandable. The scientific attitude toward things is a dispassionate weighing of facts and the removal as far as possible of emotionalism and irrational attitudes. And when you've done that, there's very little room for nationalism and the rest of the petty ideas.'

*Have you met such cosmopolitan ideas in the Communist world?* 'Yes. I spent a month or so in the Soviet Union once and I talked to people there who were hardly distinguishable from Western scientists.'

*As a member of the international scientific community, do you feel a kinship with Soviet physicist Andrei Sakharov, who has become a symbol of the fight for intellectual freedom in the Soviet Union?* 'Very much so. And I must agree with him that we in America should help open up that society by asking for intellectual freedom as a bargaining point in the negotiations for a lasting detente. As a member of the National Academy of Sciences, I was delighted with its recent resolution to that effect.'

*You don't have a reputation as a public crusader, do you?* 'Well, I'm not really a great signer of resolutions.* I'm a member of the American Civil Liberties Union and I'm on the board of sponsors of the *Bulletin of the Atomic Scientists*. But I'm a retiring sort of person who does not enjoy broadcasting his views. My views are readily available to anyone who wants to ask, as you are.'

*Do you ever have occasion to discuss your social views with students?* 'That does happen sometimes. Recently I had what I suppose was a rap session with some of my physics students. And they seemed to share many of the views I've been discussing with you.'

* He did sign some. For example, in a two-month period in 1975 he signed three petitions: one for Andrei Sakharov to receive the Nobel Peace Prize, one against astrology, and a third against the rapid growth of nuclear power.[2]

*Speaking of students, how old a student were you when you decided to become a scientist?* 'Oh, I think I was about 10 when I decided to become an engineer.'

*You're not the son of a scientist, are you?* 'No. My father was a designer of ladies' clothes. But I became very interested in science while I was in grade school in New York City. And the most immediate thing was engineering, so I said, gee, I want to be an engineer. Then, as a result of reading books in the public library, I discovered that the really interesting thing about engineering was the science behind it. And so I probably was about 12 when I decided to become a physicist.'

*And you became a Columbia Ph.D. in physics by age 21 and a full professor in your field at Harvard by age 29. What attracted you to theoretical physics?* 'I became fascinated with the structure of the universe.'

*Can your fascination be described as religious?* 'I suppose it's a form of nature worship. Perhaps it's not that much removed from the worship of a rock or a tree. Whatever directed primitive man to stand and wonder at the heavens is still at work in modern science.'

*Has your awe of the universe increased over the years?* 'Awe is indeed the word. And the feeling has increased as I've learned more about the complexity and yet simplicity of the universe.'

*Perhaps this is an unscientific question, but do you foresee an end to discoveries about the nature of matter?* 'That's actually a very good question. I think my answer is no. I don't see an end. It's hard to say why. Perhaps one reason is that to believe we're nearing the end of this discovery would be a form of unacceptable arrogance.

'It would be arrogant to assume that in roughly 300 years of modern science we could approach anything like an understanding of the mystery of what's out there.

'In recent years we've become aware of so many new facets of nature that were totally unexpected, such as quasars, neutron stars, and black holes. Perhaps when we travel to other planetary systems, we'll discover things that are simply unimaginable.

'So we're not even near the end of discovery. I think we're at the beginning.'

*What about the possibility of intelligence on other planets?* 'I'd be extremely shocked if we didn't have myriads of cousins out there somewhere. It seems very reasonable that if the laws of nature are the same almost everywhere, then what happened here would happen elsewhere. I do believe there is intelligence throughout the universe. And I would devoutly wish that in my lifetime the first meeting would take place.'

*Have you been involved in the controversy about the value of basic research as compared with applied science?* 'As a theoretical physicist, I've typically been asked by reporters, what good is your work?'

*How do you answer such an irritating question—other than by resorting to violence?* 'Well, violence is often indicated but never applied. I'm afraid I answer in the usual general terms: one does basic research because it's in the nature of man to seek understanding. Fundamental research does pay off in practical applications, but that should never be the sole justification.'

*Nevertheless, the basic research for which you and two other scientists received the Nobel Prize in 1965 did lead to practical applications...* 'The prize was awarded for research we had done many years before on quantum electrodynamics, which probably influenced the climate of the applied work that resulted in the invention of the laser. And somewhat earlier, I made direct use of my theoretical knowledge of electromagnetic waves to help develop radar.'

*Do you think the press has been making any constructive attempts to educate the public about the value of basic research?* 'There are many more science editors today than there used to be. I'm not sure they are that proficient in science, but they try.

'I think the great tragedy has been that our government has never understood the importance of fundamental research. Government in general has gone along with science only because of its practical applications. But the idea of science for the sake of science has certainly never been appreciated.'

*Is there a need, do you think, for more public understanding about the controversy over the federal cutback of funds for basic science?* 'Very much so.'

*Scientists at UCLA and elsewhere have referred to this situation as a national crisis. Do you agree?* 'I think it's a crisis. Suppose we do say yes applications are important and that government should focus primarily on applications. We'll accept that, but applications are always the consequences of fundamental research that was done years earlier.

'When you destroy the broad base of research, the repercussions will go on for many years.'

*How do you respond to the argument that research at a university tends to detract from teaching?* 'It's been my experience that research and teaching go hand in hand.

'My research has always been enormously assisted by the fact that I had a crew of warm bodies and live minds on whom to try out new ideas. Conversely, the viable parts of that research were instantly incorporated into the things I talked about in class.'

*Do you think there should be more instruction about the revelations of basic science in liberal arts curricula?* 'Absolutely. Science should be a central point of liberal arts education.

'In fact, what could be more of a liberal art than science? I hope I've conveyed to you my feeling of the tremendous excitement that goes with this endeavor.

'There's no need for liberal arts students and other laymen to view science as mechanical and uninspiring when in fact it's more artistic than art and more religious than religion. It has in it the best of everything that man has achieved.'

## 'I will be a composer by the time I'm 30!'

Music was important to Julian Schwinger throughout his life. While Julian was at Townsend Harris High School, or perhaps just after he entered City College, he fell under the influence of Hyman Goldsmith. As we have mentioned in Chapter 1, he was instrumental in kindling Schwinger's interest in classical music. Bernard Feld recalled that 'Hy was a dilettante and he was a very interesting guy because he was very lazy. But on the other hand, he had two things—one, he had an encyclopedic knowledge of what was going on, and he had really good taste. He knew what was important and what wasn't important.'[3] He had a great interest in music, and this had a profound influence on Julian.[4]

Herman Feshbach first met Julian about 1935, while he was still a student at City College, and kept in touch with him when he went to Columbia. He recalled that a group of young physicists, including Julian, would meet at the home of Artie Levinson and listen to music. A favorite opera of Julian at the late 1930s soirées was Mozart's *The Marriage of Figaro.*[5]

Joseph Weinberg, his friend at City College, recalled that Julian was exclusively interested in Mozart there, as later at Berkeley. Weinberg tried to interest him in Beethoven, particularly the F major quartet, opus 135, without much success. Julian was mystified by Weinberg's enthusiasm for Bach. Weinberg recalled looking at his record collection at Berkeley, which he did not find very exciting, except for the Mozart C major quartet, K465, 'The Dissonant.' When Weinberg picked out that piece, Julian expressed surprise, because apparently Oppenheimer had selected the same piece of music earlier.[6]

Julian Schwinger was close to Nathan Marcuvitz at the Radiation Laboratory, and in the immediate post-war period; indeed, as we have mentioned earlier, it was Mark who introduced Clarice and Julian. Marcuvitz recalled how Julian loved music, and often had a radio on in the lab. At one point he told Marcuvitz that 'at age 30 he would quit physics and devote himself to musical composition.'[7] David Saxon, another close co-worker and friend at MIT during the war remembered that 'he came to my house many times to listen to chamber music.'[8]

Clarice recalled that when they first started dating she became aware of his interest in music. Julian enjoyed music and they went to symphonies sometimes.[9] In the early years of their marriage, Julian and Clarice went to concerts and the theater. 'It did tend to diminish over the years. I'm not sure why. Maybe it just got physically harder, to find parking places, . . . . I did take up playing the piano, which I never mastered but always enjoyed. I think Clarice had a friend who sort of took me in hand and taught me the notes. I think I had a few private teachers with whom I didn't do very well because I got nervous under such close supervision. My music never amounted to anything, but I was willing to tackle anything no matter how bad my production of the sound was, because in my mind I could hear the way it was supposed to sound. I borrowed the quotation, "Anything worth doing is worth doing badly," from somebody else. That is something I subscribe to in all areas but one.'[4]

It was impossible for Julian to go to a concert the evening before he had a class, because he always needed that time to prepare. In those days there were Tuesday night concerts at Sanders Hall of the Boston Symphony to which Julian and Clarice subscribed. But Julian had a class on Wednesdays. He found it difficult to go out on Tuesdays, so Roy Glauber would often escort Clarice to these concerts.[9] And then somebody told Clarice that people were talking because Roy and Clarice were going to these concerts. So they were forced to stop attending the series.[9]

Julian noted that one of Clarice's old friends, Rhody Abrams, initiated his musical studies. It was Rhody who taught him how to read music on the piano. And it was through her that Julian got the two teachers that he had. The first one was from the New England Conservatory of Music; he started him out on scales, and he found that difficult and did not enjoy it. Julian did not really want to be a pianist; he just wanted to be able to play. So they found a teacher from the Longy School in Cambridge[10] who taught Julian to sight read. That was a great success. As a result, he could play anything, for better or worse, never counting. Sometimes he would play quite well and other times not so well. Earlier he had played every night before he went to bed, but in later years he stopped doing that.[9] Clarice recalled that people would hear Julian playing while they were conversing with her on the telephone, and would ask who was playing. When they would come for dinner, they would ask Julian to play. Clarice would tell them to make that request while on the phone, for he sounded much better on that medium. Julian would never perform in public.[10]

Not all their friends shared their interest in classical music. John Van Vleck was a very good friend. Although he thought he was typically American, Clarice felt that nobody could be less typical. His background was special, he was gentlemanly, and, of course, very bright. He was intellectual, but not in music; his taste ran to band music. Clarice recalled the first time he came to dinner, without his wife. After dinner they went to the living room and he played band music. The Schwingers almost went out of their minds, in spite of it being good band music. Clarice was more responsive than Julian.[9] Clarice, however, remembered that Julian was knowledgeable about popular music. Although he did not like jazz and rock and roll, he knew the names of all the performers.[10]

## Tennis, skiing, and swimming

An early indication of Schwinger's interest in sports was manifested by the childhood episode when Julian was at summer camp in the Adirondacks. When Julian caught the ball hit by a visiting tennis pro, Bill Tilden, he was informed that he would go far, which the boy interpreted as meaning in the tennis world. At that same camp, where his older brother Harold was a counselor, Julian learned to swim.

As a student, Schwinger was not interested in contact sports. He recalled, 'When I got to Columbia they had a requirement for physical education, and that meant several things. You had to swim the pool several times, which I managed to do. But you also had to come out for things like wrestling; and the wrestling was that you were put on a mat and somebody who was really a wrestler would come running at you. And you were supposed to do something. Well, I, instead of opposing him, just skipped nimbly out of the way and he landed outside the mat. He got up and looked at me strangely and tried it again. I was in no way interested in contact. I thought my wits were better.'[4]

Schwinger had enjoyed music with Hyman Goldsmith. We recall that one time Goldsmith took Julian along for a game of tennis: because he had not played tennis for several years, his initial attempt to hit the ball was a spectacular failure, at which point Goldsmith took the racket away from him, much to Julian's annoyance, who felt, if given a chance, he could play quite decently.[4]

Saxon also remembered being surprised at Schwinger's athletic interests and abilities at MIT during the war. When they would have a picnic, he could throw a football well, and seemed to have normal athletic interests which no one would have expected from his demeanor in the laboratory.[8]

Schwinger's secret athletic prowess continued when he became a faculty member at Harvard. Charles Zemach, who was nominally Schwinger's graduate student in the early 1950s, but really wrote his thesis under Roy Glauber's direction, recalled that the physics graduate students used to have an annual picnic at Professor Benfield's farm. Julian was always personally invited, but never came. But one year, the president of the physics club invited Clarice, so the Schwingers went. A baseball game ensued; everyone was astounded, because Julian 'was a tremendous slugger, he really powdered the ball.'[11]

Tennis became a passion with Julian in the 1960s. While still at Harvard, he took on a student Asim Yildiz, a former member of the Turkish national team, who became his tennis instructor. Once settled in California, his regular tennis partner was Lester DeRaad, Jr, who had been his student at Harvard, and had accompanied him on his move to UCLA as a postdoctoral associate. Their playing continued well into the 1990s.

A unique perspective on this interest was provided by Ian Rosenbloom, who then worked with the BBC as a producer for the Open University program *Understanding Space and Time*, which we have described in Chapter 14. He recalled that Julian was very 'keen in getting as much tennis practice as he could,' and agreed to teach Rosenbloom physics in exchange for tennis. They mostly played together in California during Rosenbloom's several trips there during the 1976–77 period. Although Schwinger had 'an awkward stance, and didn't look like a challenging' player, Rosenbloom was always surprised how challenging the matches were. Julian displayed 'tenacity, determination, and absolute perfectionism.'[12]

In the 1970s and 1980s Schwinger visited Tübingen several times, where he often played tennis with Walter Dittrich. Often these games constituted the bulk of Schwinger's interaction with Dittrich, who would have liked to have had more opportunity to discuss physics with him.[13]

The love of swimming stayed with Julian throughout his life. One of the impetuses for moving to Southern California was the possibility of daily swimming, and indeed at their house in Bel Air the Schwingers had their own pool. In fact, swimming may have been decisive in causing him to move to California: several of his Harvard colleagues believe that his doctor recommended daily swimming as exercise, a prescription nearly impossible of fulfillment in New England.[14, 15]

Julian took up skiing in 1960, on a visit to New Hampshire. We have described this experience earlier. As with music, he tended to resist lessons, so he was largely self-taught. He became quite a competent skier, and skied often in the winters, especially after they moved to California.

## A reader, a listener, and a cat lover

Schwinger was an 'omnivorous reader.' His favorite reading was 'novels of the escapist variety,' and he became particularly fond of science fiction. Among the authors he enjoyed in the 1980s were Arthur C. Clarke, Roger Zelazny, Fredrik Pohl, Ray Bradbury, and John Brunner. 'There's nobody I'm crazy about, but I will sometimes respond or sometimes I go on a run of new authors and go through a whole list. Whatever is current I will read. I insist on only one thing, there is science fiction which begins with a scientific concept and extrapolates it into the future. Total fantasy, I'm not interested. Absolutely hogwash.'[4]

At social gatherings Julian was famous for truly listening to people. He was especially adept at attending to children. Clarice recalled that one of the nicest compliments that Julian ever got was from her niece, who as a little girl was asked why she loved Julian. She replied that she loved him because he listened.[9] Another anecdote refers to a dinner party given by Alfredo and Alice Baños. Diane Anthony, Alice's daughter, recalled Julian's kindness and amazing social grace in that setting. Julian was seated next to Diane's son Lao Anthony, an adolescent jazz musician. An awkward teenager, the boy sat slouched, uncommunicative. Julian leaned over and said, 'I see you're a lefty like me.' The boy was shy, but loved to talk about his music. Julian finally got him out of his shell by asking him 'What is Thelonious Monk's middle name?' Lao then sat up and joined the conversation.[16]

Julian always loved cats. While he worked at the Radiation Lab at MIT during the war, a cat roamed the premises, and he would sometimes buy two chicken sandwiches, one for himself and one for the cat.[9] After Julian and Clarice were married, a cat quickly entered the picture. His mother would not allow him to have a cat, so on their very first Christmas, Julian bought Clarice a kitten. Clarice remembered opening the big beveled glass door to their house, and seeing Julian's face framed in the glass with this little grey kitten. It was their cat Galileo, which they had for 14 years. When his mother came to visit, they would lock the cat in Clarice's mother's bedroom; if the cat entered the room while his mother was there, she would have a fit.[9]

In California, the Schwingers no longer kept a cat. They had an outdoor cat when they first moved to California, but it got eaten by a coyote. Clarice could no longer face the idea of a litter box; and moreover, indoor cats always want to go out. She recalled that Leo (Galileo) was a house cat. When they were not at home in their duplex in Cambridge, if he heard someone open the downstairs door he would tear down the stairs to get out. He wanted out in the worst way. He did not understand that he was not supposed to want out. As a result, sometimes Julian and Clarice were scouring the streets at 3:00 in the morning with a flashlight, calling for Leo, because they were terrified that he had no idea how to take care of himself. Furthermore, he tended to eat everything in sight and then get sick when he got home. But they thought he was a marvelous cat.[9]

The cat was really Julian's. Clarice brushed him and fed him, but he would curl up on Julian's desk, so while he was working, all Julian would have to do was to put out a hand and pet him to get a purr to come. Galileo and Julian were made for each other. In spite of Julian's love of a cat, Clarice could not face the thought of losing one again.[9] In view of her own miscarriages, this fear of loss of a child-substitute is quite understandable.

Schwinger was not active in politics, yet he was passionate about the issues of the day. He was clearly of the liberal persuasion, and was appalled by the Communist witch-hunt of the McCarthy era. The Oppenheimer hearing, which ultimately cost J. Robert Oppenheimer his security clearance and his access to the highest levels of the government, troubled Julian greatly. 'Apart from thinking [the charges] absurd and outrageous, I didn't have much of an opinion.'[4] Yet, although always impressed by Oppenheimer's intellect, he saw all too clearly Oppenheimer's character flaws. At one point, due to Julian's close relationship to Oppenheimer, a television interview with Eric Severeid was arranged. A film crew came to the Schwingers' house on two occasions and interviewed Schwinger. Yet, for a variety of reasons, that interview was never aired.[2] 'Nobody ever thought, for example, of subpoenaing me. In fact, the nearest approach to it came when some television crew showed up in my house and said, "would you make a statement?" I had thirty seconds to say something and I'm sure I described [McCarthy] as quite outrageous, or whatever, and then discovered afterwards that something had gone wrong and that film never existed. So even that little contribution did not come about.'[4] Schwinger clearly saw Teller's motivation for removing Oppenheimer from a position of authority.* 'From my own point of view, after all, my reluctance to become involved in the atomic bomb project in the first place would certainly extend into the reluctance to favor the development of future nastier bombs, which of course was also Oppenheimer's presumably principal sin, to stand in the way of the hydrogen bomb. His association, second-hand or whatever, with the Communist Party, all of this was old hat and to drag it out again was really just a flimsy excuse. That he outsmarted himself by being a little too ambiguous on some occasions was of course a problem.'[4]

## Traveling in style

Julian and Clarice's first trip together was their two- or three-month honeymoon vacation to the West. One of the stops was Los Alamos. There Julian was asked to give a talk at what they considered the ungodly hour of 8:00 a.m.; remarkably, Julian agreed, to Clarice's consternation. They went down to Santa Fe to buy an alarm clock. That morning, outside La Fonda, they heard a man said, 'Hello, Julie.' Clarice did not know who 'Julie' was. It never occurred to her to call Julian 'Julie.' Of course, it turned out that those who knew Julian in the Berkeley days called him Julie. Thus, old friends like Bob and Jane Wilson and Bob and Charlotte Serber would have called him Julie; in this case it was Bob Wilson.[9] The alarm clock was a failure: the clock began in their room but it kept Julian awake so they moved it from between the beds to the doorway on a chair under a pillow but he could still hear it. Finally, they moved it out to the living room. They never slept that night. They waited all night for 7:00 a.m. to come; when he went to gave his talk at 8:00.[9]

We have already recounted the Schwingers' first trip to Europe in 1949. That was a palate-awakening trip for Julian. He also enjoyed luxury hotels there, so much so that in Florence they ran out of money and had to call Clarice's

* Schwinger and Teller had been good friends until that episode, but Julian preferred not to speak to Teller afterwards, although when he encountered him at meetings he was invariably polite.

mother to wire additional funds when the prize money from the Charles L. Mayer Nature of Light award ran out. Clarice recalled Julian's first reaction to Paris. Although she enjoyed it, Julian absolutely capitulated when he first set foot in Paris. It did not particularly mean anything to Clarice, but Julian adored Paris always.[9] He would walk everywhere in Paris, while at home he always drove.

From Paris, they went to Switzerland to see Pauli, and then to participate in the first half of the joint Swiss–Italian meeting. In Switzerland, Clarice remembered being taken on outings during the meetings; Clarice became rather bored with the routine. She recalled with amusement a picnic outing in the mountains, where a pretty young wife from MIT came overdressed in veil and high heels, 'straight out of *Vogue*'. At the time Clarice thought she was being pretentious, but in fact she had not known of this organized event for the wives when she returned from a (rather formal) errand.[9]

On returning from this trip, Clarice and Julian fell into a comfortable routine. It was a wonderful time for both of them. When Clarice married Julian she hardly knew what a physicist was and she certainly did not know there were two kinds, theoretical and experimental. She felt lucky to have married a theorist. Liza Feld was married to an experimentalist, Bernard Feld, and it seemed to Clarice that no sooner had they gotten into bed and fallen asleep than the telephone would ring with the news that the machine was broken and he'd have to go off to MIT to fix it, or they would go to sleep and his colleagues would wake him and say the machine is working and he had to go off to do the experiment. People asked Clarice how she could stand to have Julian at home all the time. In Clarice's view, it was much easier to have him home all the time than to have him go off in the middle of the night. That would have been as bad as being a medical doctor's wife, which Clarice would not have liked at all.[9]

This was the first of many trips to Europe. For example, in 1955, they went to Pisa, to the *Scoula Normale* there, where Julian attended a meeting. That was the summer they also spent three weeks at Les Houches, where Schwinger gave his famous lectures on quantum mechanics. They had to travel there first-class, on the *S.S. Flandre*, a one-class ship. His schedule at Harvard and the time they had to be at Les Houches was such that there were no other ships that would get them there on time. So they went first-class. Clarice recalled seeing Julian at the dinner table. It was indescribable. The last dinner on board ship, he ate everything in sight. He paid for his gourmet experience by having a very unhappy night.[9]

Clarice found it to be very frustrating to be at Les Houches. On their side of the mountain it was raining constantly, whereas across the way the Sun was out. They were on the wrong side of the mountain, but they had a very good time. There they discovered *palmier*, what are called elephant ears in the United

States, because, once again, they were certainly never going to get up in time for breakfast. They would go into the little town and buy them. They had a kitchen in the building where they stayed, so Clarice would fix *palmier* and coffee for breakfast and then they would slide down the hill for lunch.[9]

Following the summer school they went to Denmark. Clarice found the drive through Germany terrible. It filled her with horror. Although the weather was beautiful and they were driving down a lovely road, with beautiful trees in the sun, she imagined seeing truckloads of people being sent off to concentration camps. It was a hideous time for her.[9] The visit in Copenhagen was arranged by Stanley Deser, who was doing a stint as a postdoc there. We described that trip in some detail in Chapter 10. '1955 stands out in my memory as an absolutely gorgeous summer spent covering as much of Europe as we could.'[4]

In 1958 they again spent the summer in Europe, eventually attending the High Energy Conference at CERN, where Julian had a heated exchange with Pauli over Euclidean field theory. In 1959 they attended the same conference in Kiev, and returned from Russia via Helsinki, which they found enchanting. Julian's and Clarice's vivid memories of that remarkable trip, and those of the year before, were recounted in Chapter 11.

In the summers in the 1950s the Schwingers typically drove across the country. They would begin by going to New York to visit Julian's parents, to West Virginia to visit one of Clarice's brothers, and to Cincinnati to visit her other brother. Then they would head off to the West Coast. In 1956, for example, they spent the summer at Stanford, and in 1957 to a meeting in Banff, which we described earlier as well.* Seattle was the destination one summer; and Madison, where Clarice felt rather isolated, was the base in 1958.† Stanford, again, was home in the summer of 1961, where they had a horse ranch in Woodside, as recounted in Chapter 11. Several times UCLA was the destination, starting in 1947; we recall from Chapter 14 that Julian always liked Los Angeles, while Clarice did not.

Of course, Clarice's mother Sadie lived with them, but Julian's parents came to visit twice a year. Julian's father Benjamin died in 1953, although, revealingly, Julian could not recall the date in 1988. While Julian's father was alive, when

* David Jackson has an unusual memory of that Canadian meeting. 'In 1957 Julian, Eugene Wigner, Phil Morrison, and I, and others lectured at a Canadian Summer School in Edmonton, Alberta. The Canadian hosts were startled by Clarice's insistence that a double bed be installed in the office temporarily assigned to Julian—he just could not work without the bed!'[17] The remarkable encounter with Marshall Baker at Lake Morraine was described in Chapter 12.

† However, that summer was very productive from Julian's point of view, as we recall from Chapter 9. It also included Glashow's thesis defense, as described in Chapter 12.

his parents visited they stayed at a hotel. When he died, Julian's mother Belle would stay with them.[9]

Generally travel was work-related for Julian. 'I am rather hazy about the traveling in [the 1950s], but for the trips, the two trips that were taken to Brookhaven in the summer, because these were important stages for me, not of tourism but of sitting down and working, developing these new concepts. That was 1949 and 1950 in the summer,'[4] when he was working on the new theory of quantized fields based on the action principle.

The highlight of their travels was the trip to Yucatan in 1962, where they spent time visiting the Mayan ruins. This reflected Julian's interest in archaeology—he collected artifacts and read avidly on the subject.[10] This excursion was unique because this was a pure pleasure trip. Nearly all of Julian's travels were associated with a meeting or a professional invitation, but this trip, and a later one in the 1980s with the Puttermans to Guatemala, was one of their few pure vacations.

1962 also brought a trip to Leningrad. Julian was an exchange professor for three weeks. He had a good time, but Clarice had a *very* good time. When people entertained them at dinner and learned that her mother came from Russia they were delighted. Clarice recalled visiting somebody who served the most marvelous cherry conserve. It was so delicious, just like her grandmother used to make, that they sent her home with a jar of it and a Russian cookbook.[9] Julian had other recollections: 'We were staying at the Hotel Astoria with the St Isaac's Cathedral next to it. The university was on the other side of the river. Somebody would pick me up in the morning and I would deliver lectures, walk back, and all the students would come with me, using the excuse to ask questions on goodness knows what.'[4] After Russia, they had to return home briefly, for Julian to receive an honorary degree from Harvard. Then they returned to Europe, first visiting Trieste, followed by Yugoslavia, and then they traveled to Geneva, for the High Energy Conference. (For more on that summer, see Chapter 11.)

Their 1963 sabbatical in Paris was memorable; they had an apartment in the 16th Arrondissement while Julian worked at Bures-sur-Yvette. Brief side excursions that year were to Israel and to Greece. That was also the year Julian bought his first Italian car, a *Flavia Lancia*, nicknamed *Brigitte Bardot*. We have also recounted that year's adventures in Chapter 11. And of course they would never forget their trip to Stockholm in 1965. The saga of the Nobel Prize has also been recounted in detail. As an outcome of the Prize, Julian bought Clarice her *Volvo*. Earlier that year, Schwinger had replaced his Lancia with another blue Italian sports car, an *Iso Revolta*, bought on the assumption that its *Corvette* engine would make servicing in America easier. Even though the *Lancia* was only two years old, Julian had fallen in love with the *Iso* at a New York automobile show and ordered it on the spot. (For a description of the specifications of that remarkable automobile, see the recollection of Lowell Brown* in Ref. 18.) They visited Rome again in 1968, after which they went on to Florence, Duino, and Yugoslavia again. The key point of that summer was four weeks in Trieste,† where they stayed at Prince Raimondo's Duino Castle as described in Chapter 13.‡ After Trieste, they went to Lindau for their first attendance at the annual meeting of Nobel laureates, and then to Geneva.

Julian very rarely traveled without Clarice. One of the few times was in 1964 when he went to Dubna, followed by a short visit to Copenhagen. That, and Julian's solo trip in 1961 to the Solvay Conference in Brussels, are described in Chapter 11.

The Schwingers' favorite sabbatical was the six months they spent in Tokyo in 1970, with support from the Guggenheim Foundation.§ Schwinger spent most of his time writing his treatise on source theory, *Particles, sources, and fields*, and in the preface to the second volume of that series expressed regret at the time devoted to writing: 'Some day, when not preoccupied with the writing of a book, I shall return to Japan and fully savor its delights.'[19]

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* Clarice offered a reinterpretation of the demonstration Julian offered Brown and Mrs Teller described by Brown. Mrs Teller remarked that for the money, one would think an automatic transmission would be included. Julian was dumbfounded because he considered her remark gauche, having recently given up a Cadillac with automatic everything to purchase this sports car, and he did not know what to say.[10]

† The Schwingers first visited Trieste in 1962, and again in 1965, when they went on to the Feldafing meeting in Austria.

‡ In June and July 1968, Abdus Salam organized a six-week international symposium on *Contemporary Physics* to celebrate the official opening of the new buildings of the International Centre for Theoretical Physics in Miramare, near Trieste, Italy. Distinguished physicists from all over the world attended by invitation, including Julian Schwinger, who was one of the stars at the symposium. Prince Raimondo della Torre e Tasso invited all the Nobel laureates to stay as his guests at the Duino Castle; everyone accepted but Paul Dirac, who stayed at the Adriatico Palace Hotel. On one occasion during this symposium, Schwinger asked Jagdish Mehra, 'As a historian of physics, who do you think had the greatest sense of the architecture of physics?' Mehra replied, 'William Rowan Hamilton.' Schwinger said, 'I entirely agree. I have always been conscious of the affinity with Hamilton in my own work.' During his stay with Clarice in Trieste, Schwinger became very fond of Northern Italian cuisine.

§ They had visited Japan briefly in 1966 when Julian attended a conference. Julian first had a chance to talk to Tomonaga on that trip. (Recall that because of an accident during the celebration following the announcement of the Nobel Prize, Tomonaga had been unable to attend the Nobel Prize ceremony in 1965.) Some details of that trip are given in Chapter 13.

Clarice felt that Kazuhiko Nishijima* was responsible for their happiest sabbatical.[21] (Clarice's perspective on this trip was the subject of pp. 467–469.) However, Nishijima recalled it differently.[20] 'In 1969 I received a letter from Julian informing me of his plan to spend his sabbatical in Tokyo. This was, however, the only letter from him to me. After that I tried to contact him in order to make necessary arrangements for their stay, but he did everything by himself. Indeed, he had been very well prepared. Before coming to Japan he studied Japanese and could even read and write fairly many Chinese characters. Also he negotiated [with] a Japanese physicist from Tokyo for renting his house during his absence from home.

'In January 1970, Julian and Clarice flew to Japan and after spending some time in a hotel they moved to the promised house. Soon after that I invited them to our home with some physicists including Tomonaga and his wife. Although Tomonaga, Schwinger, and Feynman [had] shared the Nobel Prize for Physics in 1965, Tomonaga did not attend the ceremony in Stockholm because of an accident at home and it was practically the first time for them to meet.' Once they arrived in Japan, Nishijima did everything to make sure that the Schwingers had a good time. 'One of the highlights of their stay in Japan came in early April. In that year the annual meeting of the Physical Society of Japan was held in Kochi, Shikoku. Shikoku is the smallest of Japan's four main islands and is the site of the famous eighty-eight temple circuit in honor of the great Buddhist saint Kukai or Kobodaishi. Julian was going to give an invited talk on dyons at this meeting.

'I was to accompany them on this trip and we took [the] Shinkansen (super express) from Tokyo to Osaka and then local trains to Uno, a port town. We took a ferry boat from Uno to Takamatsu to cross the Inland Sea. Takamatsu is a city on the northern coast of Shikoku. It was already evening when we arrived there. We stayed overnight in this city. Next morning we visited [the] famous Ritsurin Park in this city and then set off for Kochi by a local train. During the train trips in Shikoku I had to feed them with "bento," a modest box lunch, since nothing else was available. I guess it was not to their taste, but they pretended to like it for some time.

'In Kochi he gave a lecture on dyons and his picture appeared in a local newspaper that evening. Here he experienced something characteristic of Shikoku, namely, he tasted raw whale meat. When the meeting was over we visited tourist spots along the southern coast of Shikoku by bus or taxi, and Julian was impressed by the fact that the famous name Kobodaishi is scattered everywhere in Shikoku. One scenic place was called "Minokoshi," meaning "left unseen."

* Nishijima had first met the Schwingers at Trieste, and then at the Feldafing meeting in Austria, in the summer of 1965.[20]

'Julian: "Left unseen? By whom?"

'I: "By Kobodaishi."

'Julian: "Oh, here he is again!"

'On the way back we finally arrived at Kobe, a modern port city, after crossing the Inland Sea. Julian was eager to enter a restaurant to order beef steak since Kobe is famous for her beef. He was finally released from bento in Shikoku.

'In the summer of that year the World Exposition was held in Osaka and there were many foreign visitors to this Expo. One of them was Professor Bogoliubov who was on his way to the Expo as a representative of the Soviet Union. He gave a lecture at the University of Tokyo and then we invited Professor [Nikolai] and Mrs Bogoliubov for a dinner. Julian and Clarice met them there, but I do not recall what they talked about that summer.

'The Expo site was extremely crowded and in order to visit a popular pavilion visitors had to wait many hours in a long line. Julian knew someone in charge of the American pavilion, and thanks to his introduction to this person I could visit the American pavilion without waiting.

'In general Julian was fond of Japanese dishes. Shortly before his departure from Japan I took Julian and Clarice to a famous tonkatsu restaurant. Tonkatsu is a sort of pork cutlet, but the way it is prepared is characteristically Japanese and the sauce used for tonkatsu is also unique. Nowadays soybean sauce is found everywhere in the world, but tonkatsu sauce is found probably only in Japan. Anyway he liked tonkatsu so much that he blamed me on the spot: "Why didn't you take me here earlier so that I could come here more often before I leave?" '[20]

The Schwingers continued their nearly yearly travels to Europe after they moved to Los Angeles in 1971. For example, in September 1972, Schwinger attended the international symposium on *The Physicist's Conception of Nature*, held at the International Center for Theoretical Physics, Trieste, which was organized by Jagdish Mehra to celebrate P. A. M. Dirac's 70th birthday. Mehra invited Schwinger to give a talk about his own work on quantum electrodynamics from an autobiographical point of view. However, Schwinger gave 'A report on quantum electrodynamics' [160]. (We described part of this report in Chapter 13.) When, in May 1980, at the Fermilab symposium on *The Birth of Particle Physics*, Schwinger gave his lecture on 'Quantum electrodynamics: an individual view' [197, 199], Mehra remonstrated with him that this was exactly the kind of talk he had invited him to give at the Dirac Symposium in 1972, and Schwinger replied: 'I felt too shy then to talk about my own contributions in front of Dirac!'

The old university in Tübingen was a frequent destination, because of the Schwingers' friendship with the Dittrichs. For example, they spent the summer term there in 1981, thanks to a Humboldt Prize, as described in Chapter 15.

But after the mid-1970s, Schwinger stopped attending the big conferences, particularly the International Conferences on High Energy Physics, the 'Rochester Conferences,' which through the 1960s he had participated in regularly. In 1982, for example, that meeting was held in Paris; the Schwingers were there, but Julian did not participate in that meeting, but rather the International Colloquium on the History of Particle Physics which immediately followed the big meeting, 21–23 July,[22] at which he repeated the historical talk he had given at Fermilab two years earlier [197, 199]. Milton recalled encountering Julian and Clarice at a concert at Sainte Chapelle. The Schwingers invited the Miltons to tea at their rather modest hotel near the Eiffel Tower.

The Schwingers came to Erice in Sicily, to attend the International School of Subnuclear Physics in 1986 and 1988. In the latter year Schwinger gave his talk on 'Anomalies in quantum field theory' [215], and was presented with a birthday cake to honor his 70th birthday.[23, 24]

We alluded to the adventure in Guatemala about 1980, a trip Julian and Clarice took with Seth and Karen Putterman and their two-year-old daughter Rita, 'who never complained.' On that trip, because of heavy rains, planes were not flying out of Guatemala City. The Schwingers rented a jeep, which they discovered had no brakes, and drove from Guatemala City to Flores and thence to Tikal. They stayed at a hotel near the Tikal ruins, where the man sent out to get provisions never returned; the first night they had chicken, the next skin and bones. On the return drive, they got stuck and had to be towed. It was not easy to get the jeep on a ferry boat, where they had to supply their own rope. In short, it was quite a remarkable adventure, but one well-worth enduring in view of Julian's love of pre-Columbian art and archaeology. North American Indian art was also fascinating to Julian, and of course on a number of occasions they visited Chaco Canyon, Canyon de Chelly, Mesa Verde, and other sites in the Southwestern United States.[10]

## A gourmet and his vineyard

In Schwinger's bachelor days, he was strictly a steak and chocolate ice cream man. Clarice explained that it was just so easy for him not to think of what it was he was going to eat when he was by himself at a restaurant. He would just sit down and give his usual order, continue thinking what he wanted to think about and go away. He continued with this diet even after their marriage, although he encouraged Clarice to be more adventuresome.[9]

A variation was provided once a year. They always spent New Year's Eve with Clarice's friend Rhody Abrams where they would have boiled lobster and champagne and Burgams' chocolate ice cream. She would provide a detergent bucket and the Schwingers would come with the lobsters; sometimes they would have clams first, but always there were lobsters. They continued this ritual for many years.[9]

But Julian's tastes were transformed by the summer in Europe in 1949. Visiting Europe was always an excuse for gastronomical adventures. For example, in 1957 Julian participated in a mathematical meeting in Lille, which we described in Chapter 11. He especially remembered an extraordinary dining experience at a tiny restaurant, La Pyramide, which he visited again in 1961 when he attended the Solvay Conference in Brussels.[4]

Jane Wilson recalled that she and Robert Wilson 'were close to Clarice and Julian in the fall of 1946 when they were not yet married. Julie was the youngest tenured professor at Harvard (I think), and Robert possibly the next youngest. The rest of the physics department seemed old and settled. We had a good time together—mostly eating in fashionable restaurants. We also went to the theater.

'After Robert left Cambridge we saw them frequently at conferences and we usually had dinner together. They visited us once in our Ithaca house.

'When I think of Julie I think of fancy cars, fancy food—Lobster Savannah in Boston or munching our way through the Grand Vefour in Paris—an excellent menu.'[25]

Clarice recalled an early dinner outing with Harvard faculty, where the Kembles took them and another couple out to dinner. They took them to a very nice restaurant on Charles Street in Boston. The other women talked about schools and children. Clarice had been married six months and there she was in a car with two women who were just talking about schools and children. Her life was simply not attuned to that. She found the evening very pleasant, but she felt that she just wasn't part of that life.[9]

The Schwingers did not have casual social interactions with their friends. During the Army–McCarthy hearings, the Van Vlecks, who lived across the street from their house on Fayerweather Street in Cambridge, would come over to watch, because they did not have a television set. They would come over after breakfast and watch television together and then they would go home. It was not much of a casual dropping in, because one did not drop into the Schwingers' house. They did not have that kind of easy interchange. But it was different with Victor Weisskopf. He came to Julian and Julian responded. They would go to lunch once a week; this was Vicki's doing. Julian would never have initiated such a thing, but he enjoyed doing it until a crowd started to tag along, at which point he discontinued the luncheons as we described in Chapter 5. On the other hand, the Schwingers went out with the Felds, to dinner and the movies or the theater.

A February ritual did develop early, because Julian's birthday was the 12th, Ellen Weisskopf's was the 10th, and Herman Feshbach's was the 2nd. And the Schwingers could not imagine having a dinner party without inviting the Felds;

so every year Clarice would have a dinner party for Julian's birthday, which became a memorable tradition. [9]

The first year the Schwingers became very fond of Percy and Olive Bridgman. They were still living in Boston with Clarice's mother. That winter was a grueling one, with snow literally waist high. Mrs Bridgman came on the streetcar to have tea with Clarice and her mother. Clarice regarded that as an extraordinary thing to do, because it was a long hard subway ride and a long walk. She wore the fur coat, which Percy Bridgman had bought from his Nobel Prize money. He apparently had a sweet tooth because she took half the cake that Clarice's mother baked home to him.[9] The wife of the President of Harvard, James Conant, also came to call, while Clarice and Sadie were in the middle of house-cleaning; she dropped in with her white gloves and her card.[9]

A very special friend was Ed Purcell. He was a very dear man and a wonderful teacher. Clarice always thought of the Purcells' children when people talked about bright children or bright parents, because the boys were so sweet, bright, and as enterprising as they could be. Clarice was appalled by a teacher's saying, 'I would expect better of a Nobel Prize winner's son' in front of the whole class at school when one of them made some sort of mistake.[9] Recall that it was Ed Purcell's coming to Harvard after the war that convinced Schwinger that he should choose Harvard too. 'There was mutual admiration between Ed and Julian.' In fact, although Ed was an experimentalist and Julian a theorist, there was much in common between the two men. Both owed their careers to Lark–Horowitz, the chairman of the Physics Department at Purdue in the 1930s. Lark–Horowitz gave Schwinger his first real position, and somewhat earlier he had introduced Purcell to physics: 'Ed hadn't known what physics was until Lark–Horowitz had him work on this experiment.'[26] For several years after they both arrived at Harvard, the Purcells and the Schwingers would get together for dinner once a month. Beth Purcell recalled that 'Julian was not reclusive, he liked to be with people, but he had a certain reserve. When he was with people he knew and liked he was quite outgoing. He was not the life of the party, but he participated in social occasions. He was not gregarious or outgoing, but he enjoyed a good conversation with good friends. Julian had a wry sense of humor. One had the feeling that he was aware of his own ability, and didn't underestimate it.'[26]

Things went on much the same way after the Nobel Prize. In Belmont they lived quite nearby to the Desers, the Malenkas, and the Martins and they saw them often. Because they all lived in Belmont, there was an easier interchange of visiting back and forth. Their social life consisted of visiting with Clarice's old friends, people at MIT, and the family. Nothing really changed in that respect.[9] In 1967 the Malenkas and the Martins gave the Schwingers a surprise twentieth anniversary party; Clarice was not happy about it.[9] More successful had been a couple of earlier costume parties that the Malenkas hosted, which many of

Julian's students and colleagues attended. For one of these parties, Julian came dressed as a dashing musketeer.[27]

They lived within walking distance of their friends in Belmont. Clarice found it was pleasant living there. They had a nice house with beautiful trees and a pretty garden and were off the main traffic line; yet it was only a ten-minute walk to the bus. And marketing was nearby. When she first got her *Volvo*, she disliked driving so much that she would walk down to market, leave the groceries at the market, come back and get her car and drive to pick them up because she could not carry them home. But the idea of driving to market did not seem pleasant to her.[9]

When friends moved away, the Schwingers maintained the friendship, but without much contact. For example, Madlyn and Mort Hamermesh left Cambridge after the war, but when the Schwingers, a few years later, spent a summer at Brookhaven, the two couples became rather close again. Clarice felt that they had always been close and warm, and whenever they got together they immediately took up from where they had left off. But the Schwingers did not write or call. It was just a longstanding friendship. And this was true of most of Julian's friends who went away; they maintained relationships but not contact.[9]

The Kivelsons were among the Schwingers' closest friends in California. They often had dinner together. Margaret Kivelson recalled Clarice's kindness in allowing shop talk: 'Particularly when we'd come to dinner at their house, Julian would always want to know what was going on with my space craft, what we were discovering. He was very interested. I always looked forward to telling him what was new. Clarice was always happy to see him involved. She has an amazing talent. I rarely talk science in social situations when there's somebody there who's not a scientist, but Clarice had a way of making you feel it's quite all right, she's interested too. That's the thing I would say was most exceptional. She always encouraged this part of the interaction, never making it seem we were boring her to tears.'[28]

A special event occurred early in their life in Los Angeles. On 3 October 1973, Jagdish Mehra, at the request of Yuval Ne'eman—then President of Tel Aviv University—organized a symposium on 'The Present and Future Goals of Science,' which was held at the Century Plaza Hotel in Los Angeles to celebrate the Decennial Assembly of Tel Aviv University. Los Angeles was the headquarters of The American Friends of Tel Aviv University and the home of Victor Carter, Chairman of the Board of Governors of the University. Among the speakers and attendees were Willis E. Lamb, Jr, Sir John Eccles, Robert Sinsheimer, Allan Sandage, Edwin McMillan, Owen Chamberlain, Murray Gell-Mann, Emilio Segrè, Alfred Kastler, Leon N. Cooper, and Julian Schwinger, whom Mehra had invited to be Chairman of the Symposium. Schwinger served as a most efficient and gracious Chairman who kept everything moving on a tight schedule, and the Symposium was a huge success. That evening, after the Symposium, there was a dinner–reception at the house of Mr Carter, and many wealthy persons were invited to pledge support for the advancement of education and research at Tel Aviv University. The Yom Kippur War against Israel had just been declared, and Edward Teller gave a rousing speech to all the assembled invitees and prospective donors. The pledges had been intended to be made for Tel Aviv University but, by general consent, the monies pledged that evening—millions of dollars—were made a gift to the State of Israel for its war effort. The initial plan was that the distinguished Nobel laureates would all travel to Israel as guests of Tel Aviv University and be received by the Israeli President, but this plan had to be dropped in view of the war.

On the eve of the Symposium, Julian Schwinger had made a reservation for dinner for himself and Clarice and Jagdish and Marlis Mehra at the Japanese restaurant in the Century Plaza Hotel, while—without knowing about that—Mehra had made a reservation for the four of them in the French restaurant across the hall. Mehra, who did not fully appreciate the subtleties of Japanese food, had to use great diplomacy to steer Julian and Clarice into the French restaurant as his guests. There, Julian ordered filet mignon with a spread of mango chutney, and everyone joined him. It was a most unusual and exotic dish, and Julian was very pleased with his selection, as was everyone else. It went very well with a bottle of Château Laffite-Rothschild 1957, and the whole event became memorable.

### The V. Sattui Winery

A major event in Schwinger's life occurred in 1975. He learned that the scion of an old winemaking family in California, the Sattuis, was attempting to revive the family business. Vittorio Sattui started making wine in the 1880s and became quite successful. But Prohibition came in 1920, and destroyed the business. His great grandson Darryl Sattui finally began to realize his lifelong dream to restart the winery in the mid-1970s. 'I approached famous movie directors, lawyers, renowned surgeons, a Nobel Prize winner, my friends, anyone who had a little money and might be interested.'[29] He finally managed to raise a bit over $50 000, started the business on a shoestring, and actually turned a small profit the first year. For several years the operation barely survived, but it succeeded by selling only directly to the consumers. This success was achieved with no compromise in quality: 'In 1997 V. Sattui Winery won an astounding 47 Gold Medals in major international and domestic competitive tastings against the world's best.'[29]

Julian was the Nobel Laureate referred to above. He learned of the project while touring a winery where Sattui was working as a guide.[30] In a recent letter

Sattui has described in detail his relationship with the Schwingers. 'Prior to re-establishing the winery in 1975 I courted Julian Schwinger among others to get him to invest in my project. After a series of letters between Julian and me, he invited me to his home near UCLA. During the conversation Julian offered me a glass of white wine and a moment later asked me what I thought of it and more specifically what it was. I remember answering chardonnay, and he replied, "No, it's not chardonnay, it's French colombard." I realized then that he had tested my knowledge of wine, and on that specific test I had failed. For surely Julian was trying to ascertain my level of knowledge about wine prior to making the decision to invest or not.*

'Well, Julian did invest in my new winery, V. Sattui. And I would like to think it was the best investment he ever made even if I couldn't tell the difference between chardonnay and French colombard when I was first starting out.

'Each year the stockholders of V. Sattui Winery, of which Julian was the second largest (with more than 12% of the stock), met for a stockholder's meeting. And always the night before I would have dinner with Julian and Clarice. About eight years ago (fifteen years after Julian invested) at one of these dinners I happened to thank Julian and Clarice for having enough confidence in me in the beginning to take a chance on me. By this time we were quite successful, but without Julian and Clarice believing in me from the beginning the winery might never have gotten started again.

'Before Julian could reply Clarice retorted, "Don't thank me, I never wanted to invest in the winery. Thank Julian. It was totally his idea." ' She went on to say that she had been totally against it, and was angry and hurt over the idea, and was very upset after he essentially invested their meager life savings into

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* Frances Apt wrote: 'We were all at a New Year's Eve Party at [Paul] Martin's house, on Stone Road. As a sign of goodwill, one of Charlie's [Charles Apt] friends at Arthur D. Little, presented Charlie with a large bottle of excellent Irish whiskey that was not yet marketed in this country. We decided to bring it to the New Year's Eve festivities, and I made a label for it. I pasted on the side a large piece of paper on which I'd drawn a gaudy border of shamrocks. Then I wrote, in large letters, GAELIC DEW, and down at the bottom I wrote," Bottled in the year of the martyrdom of Sir Roger Casement." It was all in fun, though Charlie suspected that not many people would get it.

'No one, in fact, did. Paul passed it around when we came in, but people just smiled. Then he put it on the living-room mantlepiece. When you and Julian came in, Julian walked over to it as I nodded. He read it and then came over to me and said, "1916, right?" Of course, it was! And then, very quietly, he told those standing around us who Sir Roger Casement was—and at last they caught on. Now, I admit, not much of a joke, really. But it seemed to me, at the moment it came to mind, a harmless way to show the true Irishness of the contents. . . ' (Frances Apt to Clarice Schwinger, 20 November 1995; Julian had died on 16 July 1994.)

the rebirth of my winery. She added, 'But Julian had a feeling about you and knew you would be successful, and he persisted despite my grave doubts.' She later said that Julian had an instinct about people and their potential and that he was almost always right.

'Julian was a very modest man. For years I never knew who the second biggest stockholder was. He would come to meetings and listen to me the whole afternoon. Occasionally he would have something to say, and it would never be something frivolous. When he did speak at our meetings, always in that soft-spoken modest way of his, it was always pertinent to real issues. But most of all he listened, never saying a word. I often wondered what he really thought.

'When Julian first invested (again I say Julian because Clarice wanted no part of V. Sattui Winery), he had to fill out a questionnaire for the Bureau of Alcohol, Tobacco, and Firearms. For this government agency wanted to be sure unsavory types or criminals were not allowed in the wine business. It must have been a legacy of Prohibition. I remember getting Julian's form back, and it didn't seem unusual to me. He listed himself as a university professor and answered the question about where he got the money to invest by answering "from books." I just assumed that he was one of many professors that were compelled to publish in order not to lose their jobs.

'About eighteen years after Julian had invested with me thinking he was just another university professor, the President of the entire nine-campus University of California system came into our tasting room (I believe his name was Heyman*) one day. He happened during our conversation to ask if I knew who Julian was, I said, "Sure I do. He is a professor of physics at UCLA." He laughed and replied, "So you really don't know who Julian is. That man is so modest!" Then President Heyman went on for five minutes regarding Julian's accomplishments, the Nobel Prize, the National Medal of Science, etc., etc. I was dumbfounded. For all these years I had had this relationship with one of the great men of the twentieth century without so much as an inkling.

'In many ways Julian and I had about as much in common as rain and sunshine. He was the physics legend, and I could hardly spell *physics*. The only physics course I took in college was a watered down one for business students. I couldn't even understand much of his book written for the lay person trying to explain Einstein's theory of relativity [207]. In fact I could only read 48 pages and comprehended almost nothing. He was disappointed that I understood so little, as he was trying to reach the average guy to foster more interest and understanding in physics. It didn't happen with me.

'Julian and I were from two different worlds. Most of our interests were worlds apart. Yet we got along well. We never argued in nearly twenty years. He always

* Ira Michael Heyman was actually Chancellor of the Berkeley campus, 1980–90.

backed how I ran the winery, even when others didn't. And he always voted the way I hoped for at meetings, and he never meddled in the daily running of the business or the minute details. But he and his wife Clarice never failed to come from Los Angeles for our annual meetings.

'I'm sure that Julian was a lot of things to different people. I didn't see his physics side, and I regret I never attended one of his lectures which were apparently marvelous. But I found all this out too late.

'The Julian I did know was a kind, humble, gentle humanitarian with a dry sense of humor, who didn't say much. But perhaps some of the physicists didn't realize that Julian appreciated a good glass of wine, going out, good food, traveling, tennis. He had a zest for life. And he had his own fantasies such as owning part of a winery.

'The fact that two people so incongruous as Julian and I ever even met and had a nearly twenty year relationship never ceases to make me think how absolutely fascinating and unpredictable life really is. I know I am better off for having had the opportunity to have known and spent time with such a great man (and not just great in the sense of physics but as a fine human being), and I truly miss him.'[31],*

## The teacher and his disciples

From his first day at Harvard, Schwinger always had one or more 'assistants,' what we would now call 'postdocs.' The first was Harold Levine, one of his close collaborators at the Radiation Lab. 'I took him with me to Harvard as sort of my assistant. I guess I was granted the privilege. He took notes of these lectures [on nuclear physics] and he had an absolutely beautiful hand and the notes were very widely circulated because I think at the time it was the one up-to-date text on the then [current] situation in nuclear physics. I think there was some competition, later, from [Robert] Serber, in a famous set of notes [entitled] "Serber says." I guess I am more famous for what I have not published than for what I have.'[4]

Immediately Schwinger acquired a host of talented PhD students. In spite of the fact some of them felt that Schwinger didn't care about them, in fact he recognized that their presence was essential. 'In 1948 and 1949 I put out at least 10 or 12 PhDs [the actual number is 13]. I had to come up with problems for all these people, which was part of the stimulation.' Unlike Feynman, Schwinger

* On 22 March 1988, when his taped interviews with Julian and Clarice Schwinger were over, Julian presented Jagdish Mehra with a bottle of Cabernet Sauvignon 'from my vineyard.' It was indeed an honor to be presented with a bottle, for he gave them only to a few close friends. It is noteworthy that after his death Sattui Winery made a special edition label with a portrait of Schwinger at the top.

always had an ongoing program, and any number of problems that he, himself, did not have time to pursue. He was, in fact, very kind to his students, never turned anyone away who wanted to work with him, and ultimately graduated 73 PhDs, many of whom became distinguished leaders in physics and other fields, including three Nobel Laureates: Sheldon Glashow, Walter Kohn, and Ben Mottleson. Schwinger 'was hard to catch, but when I was caught I gave them the attention they needed to go to the next step and no more.'[4]

Schwinger avoided the administrative duties that take up so much of the time of an ordinary professor. His one committee assignment was very revealing, if ineffective. 'I can only think of one committee I was on, which had to do with whether we [Harvard] should get involved in developing computers. This must [have been] back in 1948. Van Vleck was on that committee and I remember them turning to me, and I said, if I really wanted to, I'm sure I could keep that machine busy totally, 24 hours a day, with the problems I could dream up. I was thinking of all the quantum scattering problems one could do.* And Van Vleck's jaw sort of dropped when I said that and he later said that statement made a very deep impression on him. I think very soon thereafter they reorganized the committee and I wasn't on it anymore. I was opposed to committees.'[4] Without Schwinger's input, Harvard made a fateful, wrong, decision not to establish a computer lab, an error not rectified until the 1960s.

Robert Raphael, who received his PhD in 1957 under Schwinger's direction and went on to become a Trappist monk, offered a succinct summary of what he had gained from Schwinger: 'Close attention to phenomena coupled with a drive towards unification using powerful analytical instruments capable of revealing their inner structure—this lesson I learned at the master's feet, and it has stood me in good stead.'[32]

Schwinger's lectures, delivered around the noon hour, were the beginning of his day at Harvard. Invariably these took place on Monday, Wednesday, and Friday; Tuesday and Thursday Schwinger stayed at home. They were marvels of content and presentation, and interruptions were seldom tolerated. But not all of Schwinger's students were respectful. Larry Horwitz, who was Schwinger's student in the mid 1950s, wrote that 'I was among a group of, I think, nine students that he took simultaneously, including Glashow, Johnson, Baker, Sawyer, Sommerfield, and Garrido. I wrote down everything that Schwinger said, but sometimes Glashow read a newspaper during class, and actually asked Julian questions!'[33] Charles Zemach, who started attending Schwinger's lectures as an undergraduate around 1950, remarked that 'elegance and analytic power

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* An example is provided by a paper he wrote with Herman Feshbach in 1949, but only published in 1951, 'On a phenomenological neutron–proton interaction' [67], in which the calculations were 'done on the Harvard Mark I calculator.'

were general trademarks' of his lectures. 'He was a scientific poet,' in that he was interested in an esthetically pleasing presentation. Zemach recalled that he typically arrived 15–20 minutes late for his 11:00 lecture, driving up at the last minute in his sleek blue automobile;* of course he never tolerated, and his well-trained audience never asked, questions. Once Zemach brought an untutored friend to listen; he had the audacity to point out one of Schwinger's rare errors on the board! 'Schwinger turned around and said, "Sir?" The guy pointed out there was a slight numerical slip there, so Schwinger corrected it. That was totally unprecedented.'[11] Robert Warnock, who arrived at Harvard in 1953, recalled that the atmosphere of the lectures was 'rather formal' in that 'questions were not tolerated.' Once a questioner grew more and more persistent, which drew no response from Schwinger except for him 'getting quieter and quieter and looking down at his feet,' until the questioner gave up in embarrassment. Schwinger's lecturing style seemed almost 'automatic' to Warnock—once a French television crew came to film one of Schwinger's lectures, so the students served as extras. Of course, as with all filming, there were pauses; after each pause Schwinger would pick up from exactly where he left off in his lecture.[35]

Abe Klein recalled that while he was Julian's assistant he attended a long mathematical lecture in 1951 on quantum mechanics. The answer emerged as 'the well-known Gegenbauer polynomials.' The class laughed at Schwinger's phrase. A short while later while *en route* to the annual departmental picnic, the graduate students encountered signs reading 'this way to the well-known Gegenbauer polynomials.'[36] Klein also remarked that Schwinger's lectures were not hard to follow, because he repeated everything three times, in different words.[36] 'On the other hand, he did tend to smooth over difficulties, and it was clear that he didn't encourage questions, so none were ever asked, at least during his classroom lectures.'[37]

One of Schwinger's less-than-satisfied students was Bryce DeWitt. He arrived at Harvard in January 1946 still wearing his Navy uniform, the same term that Schwinger started teaching there. DeWitt recalled the first course was on the electromagnetic theory of light, where, as always, everything came out of Schwinger's head. Once, when he forgot a cross-section, Schwinger consulted a piece of paper which he brought out of his pocket, at which point everyone booed! Schwinger clearly wasn't used to students, and the final exam was a disaster. 'When the bluebooks were passed around, we all just sat in stunned silence for about half an hour, without raising a pencil.' Among other things he asked the students to reproduce the 1938 classical radiating electron theory

* Another student, Hiroshi Yamauchi, once got so annoyed with Schwinger's perpetual tardiness that he actually arrived after Schwinger did—Schwinger looked surprised.[34]

of Dirac,[38] something he had not touched upon, but which he felt should be regarded as a 'corollary' to what the students had learned. Schwinger gave everyone a B; years later DeWitt happened to be in Schwinger's office alone, saw a stack of blue books, pulled them down, and discovered they were those old examination papers, ungraded!* For a later course on quantum electrodynamics DeWitt took careful notes. 'At 11:20 a door in the back of the room would open, a stooped figure with a shock of black hair would poke his way around the corner, and there he would be. He would pick up the chalk, and would always say, "At the end of the last hour," and then he would just start writing,' picking up right from where he had left off the last time. Afterwards, DeWitt would recopy his notes and fill in the details. 'Every so often, I would throw down my pencil, saying, "That son of a bitch has done it again!" ' when he discovered a 'big gap in the logic.'[40]

This was hardly the universal view, however. Richard Arnowitt found his 'lectures superb. I never felt they were facile, or left out the hard parts, as some others have felt. Julian was one of the great teachers of our time.'[41]

Lowell Brown also spoke of Schwinger's marvelous lectures, the details of which, if not the broad strokes, found their way into his own book on quantum electrodynamics.[42] But he recognized that the downside of Schwinger's presentation was that he did not put the subject into context or provide the historical background.[43] Schwinger's lectures were hermetic—their self-contained nature was both their strength and their weakness.

Not only Harvard students (and faculty) attended these brilliant lectures, but faculty and students from MIT came as well. One of the latter was David Jackson. 'I was a graduate student at MIT from 1946 to 1949. We used to come up occasionally to listen to Julian lecture, always in the afternoon. The impression was one of inexorability. However unique his approach to a topic was, he made the development seem absolutely compelling, with no possible deviations or alternative allowed. His style was very polished, without hesitation or error.'[17]

Walter Kohn painted a magnificent picture of Schwinger's lectures: 'Attending one of his formal lectures was comparable to hearing a new major concert by a very great composer, flawlessly performed by the composer himself. For example, his historic graduate courses on nuclear physics and on waveguides given in the late 1940s consisted largely of exciting original material. Furthermore,

* By the fall of 1947, Schwinger had gauged the students' level more accurately. Abe Klein noted that, rather than consisting of research-level problems, the final exam for his first course on quantum mechanics was 'more than doable.'[36] It may also have been this later semester that Schwinger taught Applied Science 33, on waveguide theory, the final examination of which is interesting but quite straightforward. Actually, the final exam of the course that DeWitt refers to seems to have been entirely reasonable.[39]

both old and new material were treated from fresh points of view and organized in magnificent overall structures. The delivery was magisterial, even, carefully worded, irresistible like a mighty river. He commanded the attention of his audience entirely by the content and form of his material, and by his personal mastery of it, without a touch of dramatization. Interaction with the audience was as rare as in a formal concert. Crowds of students and more senior people from both Harvard and MIT attended and, knowing his nocturnal working habits, I found the price of having to wait 10, 20, 30 minutes for his arrival quite trivial in comparison to what he gave us. I felt privileged—and not a little daunted—to witness physics being made by one of its greatest masters. Each of these two courses had a tremendous influence on the shape of their respective fields for decades to come, as did other later Schwinger courses such as quantum mechanics and field theory.'[44]

At Harvard, a ritual developed which continued for many years. Every Wednesday afternoon (and in earlier times, Fridays also—Mondays were occupied with faculty meetings), after Julian would return from lunch, perhaps at 3:00, students would file in to see the master sometimes according to a list which they had signed earlier in the day. As he had typically more than ten research students at a time, if one's place in the queue were unfavorable, one might not get in to see him that week. Even Schwinger's long-time assistant was not given higher rights. Horwitz made this point: 'One day, waiting in line one Friday afternoon, I met Harold Levine, his close collaborator on many works. I asked Harold what he was doing there, and he said he was waiting in line like everybody else.'[33] Alternatively, a higher-ranking individual might displace all the students. In the 1950s that might be Pauli,[33] or Weisskopf and Feshbach, or his assistant Kenneth Johnson.[45] In the 1960s such a person was Bruno Zumino, who was visiting Harvard on sabbatical. Zumino, and perhaps David Boulware, then Julian's assistant, would go out for lunch together with Julian and sometimes never return. Michael Lieber remarked, 'I remember one afternoon when it had been drizzling, and Julian had gone for lunch with, perhaps, Zumino. We sat in the outer office waiting for him to come back, sure he would return because his coat was hanging on the coat hanger. But he never came back that afternoon.'[46],*

* Charles Sommerfield, who received his degree from Schwinger in 1957, stayed on for two years subsequently as his assistant. His only duty in the position was to join Schwinger for lunch on Wednesdays, after Schwinger's lecture and before his office hours. He recalled sometimes waiting a considerable time before his boss was ready to leave for lunch, which meant that on occasion the two of them ate alone. Invariably, in those days, the luncheon spot was *Chez Dreyfus*, and Schwinger would always, after studying the menu carefully, order the same steak. Once Sidney Coleman joined them for lunch, and aware of Schwinger's habit, ordered first: 'I'll have the luncheon steak and

A more dramatic story is told by more than one of his students. Norman Horing recalled entering Lyman Hall to to see a line of Schwinger students waiting outside the bathroom. Apparently he had entered, and the students were afraid that if they did not catch him there he would slip away home. After a while, someone checked the bathroom, and indeed one of the stalls was occupied. But when the occupant emerged, it was Robert Puff, another graduate student. Either Schwinger had slipped out of the bathroom before the students gathered, or he escaped by the window; but in any case he was not seen again that day.[47, 48]

As a result, students typically would see Schwinger infrequently. Richard Arnowitt recalled that he saw him maybe once a month, but that was because 'I would do everything I possibly could until I was completely stuck.' When he did see Schwinger, 'he'd think for a few seconds then rattle off five things that I'd never thought of. For me it was one of the great learning experiences.'[41] How students reacted to Schwinger's mentoring technique depended in large measure on what they were seeking. 'Julian was hard to work with if you wanted guidance, but easy to work with if you wanted inspiration.'[41] Or as Roger Newton put it, as a 'thesis supervisor he was extremely helpful when you needed help. But those who wanted a lot of interaction were in extreme difficulty.'[49] Abe Klein remarked that he had only the best feelings about Schwinger even though he may have seen him only six times while working on his thesis.[36] In his published recollections, Klein remarked that he had only three decisive interactions with Schwinger during his eight-and-half years at Harvard. 'If he thought you needed help, he did his best to provide it. Otherwise, it was *laissez faire*.'[37] We have also noted the limited, but very successful, interaction of Margaret Kivelson with Schwinger.

One of those who needed more guidance was Raphael Aronson. Admittedly, in 1949, he was the youngest in the class, and immature; he was viewed as a child prodigy. He found Schwinger's lectures disappointing; the 'big thing' was missing, 'everything came out as in a textbook, with no loose ends. Physics is about the loose ends, and I needed that lesson.' His first two thesis topics were unsuccessful; and even his thesis, on neutron–proton and proton–proton scattering, using the strong coupling theory of Kemmer and others, turned out to be incorrect because it violated isospin symmetry. The result was disillusionment; Aronson was turned off from physics. Yet even he eventually came to see the debt he owed Schwinger; his later work in reactor shielding and transport theory could not have been accomplished without the experience of Schwinger.

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the gentleman on my left will have the same.' Schwinger bristled at being pigeonholed: 'The gentleman on his left will not have the same,' and he for once ordered something completely different.

Schwinger's hallmark, in many ways, was formalism; and the fact is that often one can 'only see the physics with the formalism.'[50,*]

Schwinger's lack of availability angered some students. One of those was Walter Kohn. 'Kohn was miffed by Julian's unavailability. He completed his thesis, wrote up a paper, and submitted it to *Phys. Rev.* without ever consulting Julian.'[17] DeWitt also had less than the usual limited amount of contact with Schwinger. Part of his isolation was self-imposed. While most of the theory students shared a large office in the basement of Jefferson Lab, DeWitt was resident tutor at Kirkland House, and working largely there, did not share in the interactions of Julian's students and assistants in the physics building.[40] As Eugen Merzbacher noted, 'DeWitt was very much to himself and had no interaction with Julian.'[51] (This may have some bearing on his letter to Pauli that caused a crisis which only a personal visit by Schwinger could resolve. See Chapter 8.)

Some of the students who wanted to work for Julian were palmed off on one of his assistants. For example, Schwinger suggested that Roy Glauber give Charles Zemach a problem on neutron diffraction. Zemach found that Schwinger did not welcome students; and that the students had to come up with their own ideas for problems. At various points Zemach tried to get Schwinger interested in his problem, but to no avail. At the end, he explained his results to the master, who remarked that he had done some calculations on a related matter, but then forgot his promise to dig out his old notes. Zemach characterized Schwinger's relationship to his students as 'standoffish behavior on his side, absolute adulation on the students' side. We were willing to get whatever crumbs we could pick up from him. Everyone realized he was something special. He was thought to be the greatest physicist in the world by those of us who sat in his courses.'[11]

But it seems that the majority of Schwinger's students were satisfied with what they received. Marshall Baker, for instance, may have seen him seven times in the two years, 1955–57, but had no desire to see him more, so much material being given at each meeting.[52]

The wait might be interminable, but once admitted into the inner sanctum, time could stop. Schwinger gave the student undivided attention for as long as required. Walter Kohn remarked, 'Arranging to meet with him was devilishly hard, but when it happened—a few times a year—I found him most generous with his time and brilliant in his judgments and suggestions. It was during these

* Many years later Aronson was rebuffed by Schwinger. After the 1978 birthday celebration at UCLA Aronson tried to talk to Schwinger. His response was, 'What do you want to talk about?' After that 'I didn't bother to go to the 70th.'[50] Ruth Malenka later told Aronson, 'You know, he didn't really mean anything by it, he just did not know how to deal with people.'[50]

meetings, sometimes more than two hours long, that I learned the most from him. He had a large old-fashioned office in the old Jefferson building. In one corner, at a desk, sat Harold Levine calculating away on intricate classical wave problems, totally oblivious to what was going on around him. Drifting in and out were other students anxious to catch Julian. Frequently Herman Feshbach came over from MIT to talk about nuclear forces. A few times Freeman Dyson and Richard Feynman dropped in to talk about quantum electrodynamics. Once a letter or preprint from Tomonaga arrived and Julian said he was nervous to open it, so often had Tomonaga's thinking been almost the same as his. What great fortune for us to be there at such a time.'[44]

In the mid-1950s these audiences took place in a small room, not Schwinger's office. One could sometimes catch a glimpse of how Julian's mind worked; he was not afraid to assimilate the competition. Larry Horwitz wrote: 'Generally, there were two viewpoints dominating the methods of that time, the functional view of Schwinger, meticulously deductive, based on integral equations and functional derivatives, and the diagram methods of Feynman. One day, in this small office, I asked him a knotty question, and he used a small corner of the little blackboard to sketch some diagrams, quickly erased them, and told me the answer in full functional form.'[33] In a much later period, Alain Phares was one of Schwinger's last Harvard students, finishing his thesis after the Schwingers had moved to the West Coast in 1971. In spite of this difficult situation, Phares found his limited interaction with Schwinger extremely valuable. 'The meetings I had with Julian whenever he was in town were crucial and invaluable. At each of these meetings, which often lasted hours, he made me feel at ease and gave me a lot of confidence in myself. The insight he provided me in every topic discussed was absolutely incredible. . . . Julian was to me the greatest of all my teachers, humble, considerate, and supportive.'[53]

Robert Warnock, who may have seen Schwinger only four times during his days as a student, summarized the view of many. 'Being Schwinger's student was no picnic. He often made appointments [to see students] but then wouldn't show up until an hour or two later. He was definitely a mythic figure. However, in his office he was quite agreeable and easy to talk to.'[35] After abandoning the experimentally ruled out $K$-meson theory of Schwinger (see below), Warnock was eventually given a problem in multichannel scattering of pions and kaons off nucleons, and worked out a helicity basis for the description of matrix elements, anticipating the work of Jacob and Wick.[54] He concluded that although he first thought his thesis would have benefited from more advice from Schwinger, perhaps he was better off with the little guidance he had.[35]

K. T. Mahanthappa noted that Schwinger practiced a self-selective process. 'He never turned down anyone who wanted to work for him. Students had to find out if it [having Schwinger for an advisor] would work out by themselves.'[55]

He also recalled it might not be easy to finish. He remembered another student, 'senior to me by three or four years. He was "tired" of being a graduate student and wanted to get out. As usual with Julian, the student had to bring up the question of finishing and getting out; Julian never said voluntarily, "This is enough for your thesis," the student had to raise the question. This student brought up the question of writing up his thesis on what he had done so far. Apparently Julian thought that it was not enough. He is said to have told him, "What do you want to do after finishing up? Go and teach in a girls' high school?" It was very funny to us at that time [about 1960], especially Julian saying such a thing. Nowadays it would be viewed as a male chauvinistic remark.'[56] A less offensive version of the same attitude occurred in his remark to Roger Lazarus, who was about to leave academe for a job at Los Alamos: 'Well, I guess you have to eat.'[57]

Abe Klein seems to have hit the nail squarely on the head in the talk he gave at the Drexel memorial session: 'Why did we see so little of him once we began work? The stories of how hard it was to get to see him once you decided it was absolutely necessary translate, in practice that *sometimes* you had to wait up to a week before your turn came. But that does not explain why the average interval between audiences was three months for me, longer for some, shorter for others. My answer, which I believe represents part of the general opinion of his students, is that we were so in awe and had so much respect for the value of his time, as opposed to the value of our own, that we felt it necessary to exhaust all other resources available to us, the literature, our fellow graduate students, and our own efforts, before we went to see Julian. In thinking about those times, I have come to realize that after the short meeting during which I received my first research topic, though I remained in awe of his abilities, I was never again afraid of him, because no matter how poor the quality of the work I had done, he never tried to destroy my ego. I could see that he took my concerns seriously and did his best to come up with useful advice. Only about half the research topics he suggested to me were any good or at least any good for me. Some of his students thought everything should work out perfectly and became and remained angry when it didn't. I think that those of us who were more realistic in our expectations fared better personally. He also took seriously his basic responsibility to order us according to promise and this explains, but only in part, why bad theses sometimes led to good careers.'[37]

Schwinger rarely complimented his students. Jack Ng was the fortunate recipient of two such compliments. One was described in Ng's account of his first meeting with Schwinger in Ref. 18, where Schwinger called himself stupid for suggesting inclusion of a parity-violating term. 'I was so taken aback by his self-deprecatory remark that I thought he must have planned on it (to make me feel at ease). So I checked around to see if the other students had the same experience. Surprisingly, no one else had. I was in seventh heaven for a few days.

The other time [that he complimented me] he said that I had the right attitude to be a good physicist.'[58]

Clarice emphasized that Julian indeed gave his students as much time as he felt they needed—but not more. She well recognized how much time he spent with his students, as one who often waited until 8 p.m. for Julian to return home.[10] In assessing the degree of effort Schwinger spent on his students, it is appropriate to consider the disparity between his abilities and those of most of his students—even though they all were very bright. Oppenheimer at some time in the 1950s created the unit for physicists, the *Schwinger*. Students hoped they would be at the level of at least 1 milliSchwinger.[34]

Passing the Qualifying Examination was often traumatic for the students, but for reasons of anticipation, not for what occurred during the exam. Schwinger often came to the rescue of the student. Milton recalls that his exam largely consisted of an argument between Schwinger and Martin on the meaning of source theory. On passing, Schwinger presented him with a copy of his recently published Brandeis lectures, *Particles and sources* [149]. Ng recounted more details of his exam: 'Glashow asked me a question: why so and so? I could not even understand his question. So I just stood next to the blackboard to ponder on his question. After a minute or so, I was about to give up. Luckily, Schwinger came to my rescue. He just turned his head towards Glashow and said, "Why not?" To my surprise, Glashow replied right away, "OK." I could understand neither Glashow's question nor Schwinger's reply. But to save myself the embarrassment, I kept quiet. I passed the exam.'[58] Of course, Glashow was understanding, because *his* thesis defense involved an argument between Schwinger and Yang.[59]

Many students have remarked that their thesis topics were often not earth-shaking.* Of course, there were a large number of exceptions, such as Glashow's work on electroweak unification which we described in Chapter 12. Again Ng had a story to tell. 'In one of the gatherings we had at Schwinger's house, for some reason, we talked about PhD theses. Shortly afterwards, Julian and I were

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* Glauber had the opinion that most of the thesis topics that Schwinger assigned were 'ill-conceived and muddy. Virtually none of them were well articulated.'[60] While it is true that a number of the theses did not reach a significant conclusion, it seems to us that this harsh assessment misses the mark. These were real, and usually difficult, research problems, and most problems often do not yield a solution when they are first enunciated. Schwinger's philosophy was not to help the student solve his problem, but to provide inspiration, just enough help to get the student to solve the problem on his own. That some students wanted more help cannot be denied, nor that the thesis would have been better if Schwinger had pitched in. But it seems the outstanding results of the caliber of his students justified his technique, even though it could not be emulated by future generations.

alone. I jokingly asked why was I not lucky like Glashow in getting a nice thesis topic. Julian was not amused. He left me alone. Of course, I should have been more diplomatic even in making jokes. But I could tell that he was a little upset with himself for not continuing his work on electroweak unification.'[58]

Julian Schwinger was apprehensive of J. Robert Oppenheimer's opinion,* but Dirac was the only person whom he held in awe. After the war, when Schwinger settled at Harvard, and Oppenheimer at the Institute for Advanced Study in Princeton, Schwinger began a regular practice of sending his students to Oppenheimer for postdoctoral research. Although he had no interest in going there himself, 'the impression you got is that everybody was constantly running up and down the hall telling each other brilliant ideas, and going to one seminar after another, and I well recognize for me that would be a total disaster,' he realized it could be very useful for young physicists. 'I was very glad to be able to send some of my PhD's there. That seemed to be the next step in their evolution. For them to get another way of doing things. Somewhere along the line I invented the phrase "conversational physics," and I can't remember whether it was pre-war Oppenheimer or referred to what was going on at the Institute, or whatever.'[4] But the process of placing his students there was not always appreciated. Fritz Rohrlich, one of Schwinger's first students, getting his PhD in 1949,† recalled that after some of his contemporaries had been offered posts at the Institute, he still had not gotten Schwinger to write Oppenheimer a letter on his behalf. Eventually, Schwinger telephoned Oppenheimer, and Rohrlich obtained the offer.‡ But he was left with the unpleasant feeling that Schwinger did not care about his students.[62] Most of his graduates, however, appreciated Schwinger's help. This special relationship with the Institute for Advanced Study continued into the 1960s. York-Peng Edward Yao recalled that when he finished his PhD in 1964, Schwinger wrote Oppenheimer and secured for him a two-year postdoctoral appointment at the Institute. He remarked that Schwinger's training was good for those who got through the process, but frustrating for

* One time after Julian gave a lecture at the Institute for Advanced Study he was so concerned with Oppenheimer's reaction that afterwards he asked Richard Arnowitt how it went.[41]

† Rohrlich was already 'a little annoyed' with Schwinger. Part of his thesis was involved with the scattering of particles possessing quadrupole moments. Schwinger used Rohrlich's results in his lectures at Harvard on nuclear physics, with due attribution; but John Blatt was in the audience taking notes. Those notes eventually became the famous 'Blatt and Weisskopf' book on nuclear physics[61]; there the reference to Rohrlich's contribution was 'unclear.'[62]

‡ A similar delay was encountered by Eugen Merzbacher—only by calling Clarice who put him through to Julian was he able to get Julian to call Oppenheimer. 'Clarice was always helpful in getting jobs for students.'[51]

others.[63] Tung-Mow Yan remarked that Schwinger arranged postdoc offers for him in 1968 at both the Institute for Advanced Study and at the Stanford Linear Accelerator Center; Schwinger was pleased that Yan accepted the latter because it was the site of the ongoing deep inelastic scattering experiments that we described in Chapter 14.[64]

Norman Horing, who also received his PhD in 1964, had a rather typical story to recount. While he was a student of Schwinger's, he felt that he was not given much attention, and he saw his advisor infrequently. His first thesis topic was based on Schwinger's 'Dynamical theory of *K*-mesons,' which as we have described in Chapter 12, was a failed attempt to save parity (space-reflection) symmetry. Horing's thesis was nearly finished when Daniel Kleitman, another of Schwinger's students, proved that the scheme was inconsistent with experiment. Horing had to abandon his project, and begin another thesis topic, eventually writing a dissertation on many body physics. Yet this delay did not result in bitterness on Horing's part; he took it as a learning experience. Later, when the new thesis was nearly finished, Schwinger expressed doubt whether the result could be correct, since it seemed to violate translational invariance. Horing was shattered for a week, and spent day and night rechecking his result. When he encountered Schwinger in the hall the following week, the master admitted that he had been mistaken. But Horing recognized that even though there were these significant 'misfires,' he would never have learned proper physics anywhere else. One of his founts of inspiration was a magnificent set of notes taken and edited by Kenneth Johnson of Schwinger's lectures on quantum field theory. He remains 'eternally grateful' for the education he received at Harvard, and recognized that the 'source is Schwinger, even though it was second hand.' His personal interaction with Schwinger was 'nothing much.' Horing was, at the century's end, writing a book on quantum many-body theory, which owed much to what he had learned, directly and indirectly, from Schwinger; in his work, for example, the marvelous 'Gauge invariance and vacuum polarization' [64], which we described in Chapter 9, with its Green's function techniques, continued to play a decisive role.[47]

Horwitz had another view on Schwinger's defeat by the experiments on parity violation. 'The results of the parity violation experiments became known on the day of my PhD qualifying examination. Schwinger was apparently thunderstruck; many of his elegant formulations were based on symmetry. Fortunately it did not affect QED very much, but he was surely very quick to see that one of the apparently principal pillars on which the world stood, that of natural symmetry, had become shaky.'[33]

We have noted that certain students felt that their work had been appropriated by Schwinger without due recognition, or their inclusion as co-authors. But this was the exception. Paul Martin noted that, 'He [Schwinger] was the loser in that there were many more things he did in unique ways, without being recognized, than he took from others without attribution.'[15] An example is given by Roy Glauber. In the fall of 1946 Schwinger was teaching nuclear physics, where in understanding the two-body forces between neutrons and protons he invented a variational principle for scattering amplitudes. The question was then, what could you learn about the form of the potential well from experiments at 10 MeV? The answer was very little; only two parameters characterized the interaction—a scattering length and an effective range. John Blatt told the world about this conclusion, through notes replicated at Princeton. Schwinger's demonstration was not terribly clean, so, with these notes in hand, Hans Bethe found a neat and brief derivation, which he published without thanking Schwinger, only Blatt. Schwinger was very angry.[60] Six months later Bethe would again [initially] omit reference to Schwinger in the draft paper of his Lamb shift calculation, as we described in Chapter 7, even though Schwinger and Weisskopf had discussed the essence of the idea with him at Shelter Island.

Schwinger seemed very shy, and seldom became personally involved with his students. This could have a way of making him seem cold and unfeeling. Yet this was not really the case. One of his foreign students recalled that as he was finishing his thesis his mother became gravely ill. Unknown to the student, his father wrote to Schwinger requesting that he allow his son to return home. At the time Schwinger said nothing, but some time later, at the student's thesis defense, Schwinger asked the student how his mother was. The student was shocked because he had never breathed a word of his mother's ill health.

In the early days at Harvard, probably around 1949, a group of Schwinger's students, including Eugen Merzbacher, Ben Mottleson, Bertram Malenka, Walter Kohn, and Sidney Borowitz (the latter was not a student of Schwinger, but was a classmate of Schwinger at City College who became an instructor at Harvard; he and Kohn were Schwinger's first assistants) gave a dinner party for Julian and Clarice. It was held at the Malenkas, who provided a home for the bachelors (Merzbacher, Kohn, Borowitz).[51] Clarice recalled that party fondly, remembering that Merzbacher had to cut the onions and cried; and that they borrowed Nancy Mottleson's sterling silver. She recalled that it was a wonderful party and they had a very good time.[9] However, some of the students felt uncomfortable. 'Nobody knew what to do with small talk,' although apparently Julian was interested in old movies. 'It was a delightful evening but everybody was a little bit apprehensive.'[50] Clarice recalled that she was horrified to see Julian when they arrived go off in a corner by himself, seating himself in an empty chair. Eventually people came to him.[9] The Malenkas remembered that because they had only a three-room apartment with no bath, they had to take over the neighbor's apartment. Beef Stroganoff and pecan pie were served, and after dinner they played charades—Julian was very good at it. At the end, everyone pitched in to do the dishes in the bathroom.[27] The Schwingers did not reciprocate by inviting the students over; Schwinger always maintained a gulf between the student and himself,* which could rapidly disappear once the degree was granted.

The Schwingers were involved with Julian's former students, Roy Glauber, Paul Martin, Bertram Malenka, Kenneth Johnson, and Stanley Deser; Clarice recalled that they went out or entertained, much more than they did in California, where they did very little. They saw each other fairly often.[9] Ruth and Bert Malenka recalled that in Fayerweather Street, Clarice started giving cocktail parties for his students. She would bake a cake for the occasion. The students would stay for hours, Clarice would keep bringing out more food, and Julian was charming. Later the Schwingers gave dinner parties, but these evenings evidently involved only selected students.[27]

On the day Schwinger's Nobel Prize was announced, the Malenkas recalled organizing a party at the Schwinger's house. They brought over cases of wine, and all his students came and congratulated each other.[27]

As we have documented, eventually the atmosphere at Harvard, where he had contributed so much to the building up of the faculty, turned against Schwinger. So much so, that by the late 1960s T. T. Wu could say, 'Whatever Julian wants Julian doesn't get'[65]—so that he was not at all reluctant to leave for UCLA when David Saxon (through University of California President Kerr) again made him an offer in 1966.†

Unfortunately, things started off rather badly at UCLA, and Schwinger was far less successful, or influential, there, than he had been at Harvard. David Saxon and Alfredo Baños were there, old friends from the days of the MIT Radiation Lab, as well as Robert Finkelstein, who had known Julian from the time of the Michigan Summer School in 1948, and his student Margaret Kivelson was in Planetary Sciences, but new friends were not forthcoming. The ways of the new superstar were not ingratiating. Alice Baños recalled that after only a year Nina Byers came up the hill to bring Julian a cake on his birthday. Clarice opened the door, said, 'I've already baked him a cake,' and closed the door. (This was not intended as a personal insult, but reflected the Schwingers' intolerance of uninvited visitors. They were not used to casual California ways.)

* This changed somewhat after the Schwingers decamped for the West Coast. Ng recalled that he was invited the the Schwingers' parties three times, and they came to the Ngs' barbeques on two occasions.[18] This change probably reflected the drastic reduction in the number of Schwinger's students.

† This was the last of many offers. Saxon was very desirous of getting Schwinger to come to UCLA, and tried to persuade him many times to do so, starting in 1947.

The department became angry with Schwinger because of his reclusive ways. He wouldn't look at their problems or interact with them, merely slipping in for his classes, and escaping as quickly as possible.* 'Why did we bring him?' was the question among many of the faculty.[66] Schwinger may have asked himself the corresponding question, especially when he discovered that the caliber of the graduate students at UCLA was much lower than he had become accustomed to at Harvard. As a result, only a handful of students worked with Schwinger during the nearly two and a half decades he was at UCLA, an isolation which undoubtedly was reflected in his physics.† The bulk of his interaction at UCLA was with his assistants DeRaad, Milton, and Tsai, whom he had brought with him from Harvard. The last of these left in 1979, and was replaced for two years by Englert in the early 1980s. Correspondingly, his physics became increasingly iconoclastic.

Even those students who felt misused by Schwinger admit his lasting influence. Horwitz summarized that influence well. 'Schwinger's serious deductive style deeply influenced me and the way that I deal with my own students. There is no question that all of his students (even Shelly [Glashow]!) were very much influenced by him in this way, and that, in addition to his incomparably important works, through this he has achieved a living immortality.'[33]

Judging by the results, Schwinger's technique of educating graduate students was stupendously successful. Feynman was rather destructive toward his students, 'while in his own way Julian really cared about his students. He gave them specific problems and offered useful advice when needed. Since his time was so precious, and he had such a high standard in research, we all did our best to figure out what we did not understand before we went to bother him with any questions. As a consequence, we became very independent. Another point is that we had to learn two approaches to physics, Schwinger's way and the conventional way. This gave us an enormous advantage compared with other students.'[64]

Steven Weinberg offered some opinions as to why Schwinger was so much more effective than Feynman in educating graduate students. Like Max Born, 'when you read the list of [Schwinger's] students you realize what an impact he had. Some of them were his students because they went to Harvard, but a lot of them went after him individually.' 'Feynman even to a greater extent than Julian was unwilling to take on the ordinary burdens of academic life. Feynman

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* At first Schwinger attended committee meetings, unlike his habit at Harvard, but when he found that his advice was not heeded he stopped attending.

† Schwinger lamented, 'At Harvard the brightest students used to come to work with me. Here at UCLA, even my name does not attract them; they all want to go to Caltech *not* UCLA.'[4]

was even more of an obvious genius than Julian; with Julian it's obvious he's an intellectual, with Feynman he comes across as a longshoreman, and then you find out that he's doing this very exciting and very inspired work, and the incongruity makes him seem even more of an awesome personality.' Feynman, along with Murray Gell-Mann, projected an overpowering aura at Caltech, so much so that some people had to leave. 'Schwinger didn't have that much of an aura.' 'Julian had a strong sense of duty,' manifested, for example, in the care which he took toward his courses, and in his taking on graduate students; while Feynman 'didn't take duty that seriously,' and only took on those tasks which appealed to him.[67,*]

## Tributes to Tomonaga and Feynman

Schwinger gave two memorials to his fellow co-recipients of the Nobel Prize for the formulation of renormalized quantum electrodynamics, Sin-itiro Tomonaga, who died in 1979, and Richard Feynman, who died in 1988. Since these tributes reveal at least as much about Schwinger as they do about their subjects, we describe them in detail here. These accounts should be regarded as complementary to the descriptions of the careers of Tomonaga and Feynman given in Chapter 8.

### Two shakers of physics

Schwinger's third visit to Japan was brief.[†] It was to honor the memory of his fellow recipient of the Nobel Prize, Shin-itiro Tomonaga. As Nishijima recalled, 'In 1979 Tomonaga passed away. He had been the president of the Nishina Memorial Foundation and Ryogo Kubo succeeded [him in] the presidency. In 1980 Kubo asked Julian to deliver a lecture in memory of Tomonaga and Julian agreed. On 8 July 1980 he delivered the memorial lecture. It was impressive and touching. He emphasized various similarities in their works and careers. I knew that he worked very hard for the preparation of this lecture. Immediately after his arrival in Tokyo he stayed in his hotel and worked intensively day and night.'[20] Clarice recalled that he almost wept on reading Tomonaga's letters while preparing his address.

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* Harvard, being a better all around university, probably attracted higher caliber students than did Caltech.

† The Schwingers went to Japan a last time, again a decade later. Nishijima wrote, 'In December 1990 the Yoshio Nishina Centennial Symposium organized by the Nishina Memorial Foundation was held in Tokyo. Julian was among the invited speakers, and this time he gave a talk on cold fusion [218a]. In this visit I had a chance to take them to a tonkatsu restaurant without realizing that it was the last chance to see Julian.'[20]

Schwinger's lecture was subtitled 'Two shakers of physics' [200]. He opened his talk by explaining that title: 'Immediately provocative is the curious similarity hidden in our names. The Japanese character—the kanji—*shin* has, among other meanings those of "to wave," "to shake." The beginning of my Germanic name, *Schwing*, means "to swing," "to shake." Hence my title, "Two shakers of physics." '

Schwinger began his lecture by recounting the history of modern physics in Japan. He described how Nishina returned from Copenhagen, lectured in Kyoto, and attracted Tomonaga to a research position in Tokyo in 1932. By 1933 Tomonaga was working on the positron and on quantum electrodynamics. Tomonaga had already been deeply influenced by Dirac's 1932 paper[68] which proposed 'to demote the dynamical status of the electromagnetic field,' ultimately 'a false trail.' Tomonaga independently, and perhaps earlier, proved, but did not publish, the equivalence to the Dirac theory and the Heisenberg–Pauli theory,[69] an equivalence demonstrated by Rosenfeld[70] and by Dirac, Fock, and Podolsky.[71]

Schwinger then drew some further historical parallels. 'I graduated from a high school that was named for Townsend Harris, the first American consul to Japan. Soon after, in 1934, I wrote but did not publish my first research paper. It was on quantum electrodynamics.' Here he used the Dirac–Fock–Podolsky formulation to describe the retarded Møller interaction.[72] 'But now, since I was dealing entirely with fields, it was natural to introduce for the electron field, as well, the analogue of the unitary transformation that Tomonaga had already recognized as being applied to the electromagnetic field in Dirac's original version. Here was the first tentative use of what Tomonaga, in 1943, would correctly characterize as "a formal transformation which is almost self-evident" and I, years later, would call the interaction representation. No, neither of us, in the 1930s, had reached what would eventually be named the Tomonaga–Schwinger equation. But each of us held a piece which, in combination, would lead to that equation: Tomonaga appreciated the relativistic form of the theory, but was thinking in particle language; I used a field theory, but had not understood the need for a fully relativistic form. Had we met then, would history have been different?' [200].

In 1936 Tomonaga turned his interest to nuclear physics, and the following year went to Heisenberg's institute in Leipzig for two years. Tomonaga, like everyone else in the field then, was thoroughly confused by the misidentification of the meson observed at sea level (now called the muon) with Yukawa's meson that carried the strong force (now called the pion). He had an indirect scheme to explain the disparate properties, particularly the long lifetime of the muon, and Tomonaga became rather depressed with his slow progress, which Schwinger documented with eloquent quotations from Tomonaga's diary. Toward the end of his stay in Leipzig, Heisenberg presented him with the idea that strong field interactions might act to suppress the scattering of mesons by nucleons; Tomonaga wanted to extend his stay in Leipzig to follow this up with a quantum-mechanical calculation, but the threat of war forced him to return home.

As it turned out, Yukawa was on the same ship crossing the Atlantic, and disembarked at New York, visiting many universities in America, beginning with Columbia, where he and Schwinger first met. But Tomonaga was so homesick for Japan that he stayed on the ship all the way to Japan.

When Tomonaga got home, he started working on Heisenberg's proposal, and then became aware that Wentzel had also attacked the problem of strong coupling.[73] Remarkably, Schwinger was thinking along the same lines at the time in Berkeley. Schwinger found an error in Wentzel's calculation. 'In the short note that Oppenheimer and I eventually published [26], this work of mine is referred to as "to be published soon." And it was published, 29 years later, in a collection of essays dedicated to Wentzel [28a]. Recently, while surveying Tomonaga's papers, I came upon his delayed publication of what he had done along the same lines. I then scribbled a note: "It is as though I were looking at my own long-unpublished paper." I believe that both Tomonaga and I gained from this episode added experience in using canonical-unitary-transformations to extract the physical consequence of a theory.' [200]

Schwinger then went on to describe the erroneous Dancoff calculation of 1939.[74] Dancoff calculated the electrodynamic correction to scattering both for spin-0 and for spin-$\frac{1}{2}$ charged particles. The former gave a finite correction, while the latter gave an infinite one, in contradiction with the expectation that the electromagnetic mass shift for spin-$\frac{1}{2}$ particles should be much less strongly divergent than that for spin-0 particles.

But in 1939 and 1940 Tomonaga was still dealing with mesons. He showed that negative mesons should preferentially be absorbed by matter, but later experiments showed no significant interaction for either sign of meson. For the next two years he worked on various strong and intermediate coupling meson theories. By the end of 1943, Sakata presented at the Meson Symposium his suggestion that the two mesons were not the same.[75]

But in the spring of that year Tomonaga presented a paper at the last meeting of the Riken, on the 'Relativistically invariant formulation of quantum field theory.' Tomonaga believed that he could solve both the problem of lack of relativistic covariance and the infinities of field theory simultaneously. This talk was followed by a paper published in the Bulletin of the Institute, Riken-Iho.[76] This paper was unknown outside Japan until it was translated into English and published in the second (August–September) issue of *Progress of theoretical physics*, in 1946.[77] Even that paper was unknown in America and Europe until well after the Shelter Island Conference. In this paper Tomonaga pointed out that the canonical equal-time commutation relations, and the Schrödinger equation, were not covariantly formulated. However, formulating commutation relations between fields on an arbitrary space-like surface presents no difficulty if there are no interactions. Even with interactions this can be achieved, if the interactions are removed by a unitary transformation, that is, by passing to the interaction representation. It is more complicated to generalize the Schrödinger equation. This was accomplished by Tomonaga by generalizing Dirac's many-time theory,[68] or the theory of Dirac–Fock–Podolsky,[71] in which 'each particle is assigned its own time variable.' 'The Schrödinger equation, in which time advances by a common amount everywhere in space, should be regarded as describing the normal displacement of a plane space-like surface. Its immediate generalization is to the change from one arbitrary space-like surface to an infinitesimally neighboring one, which change can be localized in the neighborhood of a given space–time point. Such is the nature of the generalized Schrödinger equation that Tomonaga constructed in 1943, and to which I came toward the end of 1947.' [200]

At this point Tomonaga's fundamental work was interrupted by the war. Like Schwinger, an ocean and a continent away, he began to work on radar. Tomonaga had to match the language of physicists—Maxwell's equations—to the language of the electrical engineers, namely notions such as impedance. A key step in Tomonaga's development was the delivery by German submarine of a Top Secret dispatch, which turned out to be Heisenberg's paper on the scattering matrix.[78] Schwinger drew strong parallels with his own development. 'I would like to think that those years of distraction for Tomonaga and myself were not without their useful lessons. The waveguide investigations showed the utility of organizing a theory to isolate those inner structural aspects that are not probed under the given experimental circumstances. That lesson was soon applied in the effective range approximation of nuclear forces. And it is this viewpoint that would lead to the quantum electrodynamics concept of self-consistent subtraction or renormalization.'

Schwinger then recounted his major lesson from his study of synchrotron radiation at the end of the war, wherein a properly covariant electromagnetic contribution to the electron's mass appeared. 'Moral: to end with an invariant result use a covariant method and maintain covariance to the end of the calculation. And, in the appearance of an invariant electromagnetic mass that simply added to the mechanical mass to form the physical mass of the electron, neither piece being separately distinguishable under ordinary physical circumstances, I was seeing again the advantage of isolating unobservable structural aspects of the theory.' [200]

After the war was over, Sakata proposed the notion of the field of a cohesive force which could cancel the infinite electromagnetic mass effect.[79] Tomonaga took this idea up as a promising development. Although it was later shown that this could not work beyond lowest order, the 'C-meson hypothesis served usefully as one of the catalysts that led to the introduction of the self-consistent subtraction method.' Tomonaga's group used this method to recalculate Dancoff's 1939 result[74] on electron scattering corrections. Employing a much improved, covariant method that used 'a unitary transformation that immediately isolated the electromagnetic mass term' Tomonaga discovered that Dancoff had, as Tomonaga's group had initially, overlooked a term; the resulting correction was finite!

That is, it was finite 'except for a divergence of the vacuum polarization type.' As Schwinger noted, this divergence could be absorbed into a redefinition of the charge. What was needed to proceed was experimental guidance. Such guidance was provided by the results on the Lamb shift, and on the anomalous magnetic moment of the electron announced at the Shelter Island Conference. This information only reached Japan through the popular science column of *Newsweek*. Tomonaga was then immediately able to use his 'covariant contact transformation' method, which had worked so well in uncovering Dancoff's error in electron scattering, to the Lamb shift, providing a relativistic calculation justifying Bethe's approximate non-relativistic estimate.[80] This result was announced by Tomonaga at the Kyoto symposium in November 1947, calling the method the 'self-consistent subtraction method.' 'And so, at the end of 1947, Tomonaga was in full possession of the concepts of charge and mass renormalization.' [200]

The date that Tomonaga communicated his results on elastic electron scattering,[81] 30 December 1947, was the same as when Schwinger sent in his paper on his calculation of the anomalous magnetic moment of the electron [43]. 'Here I held an unfair advantage over Tomonaga, for owing to the communication problems of the time, I knew that there were two kinds of experimental effects to be explained: the electric one of Lamb, and the magnetic one of Rabi.' In that note, Schwinger also pointed out that radiative corrections to electron scattering came out finite, and that the relativistic calculation of the Lamb shift was consistent with that of Bethe. The vagueness of the latter remark reflected Schwinger's awareness that his calculation was wrong, in that the relativistic analog of the anomalous magnetic moment came out incorrectly: 'relativistic invariance was violated in this non-covariant calculation.' We have recounted this error, and the impetus it provided Schwinger to develop a covariant formulation, in Chapter 8. At the January 1948 APS meeting Schwinger mentioned this difficulty, remarked that he now had a covariant formulation,* and learned

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* Apparently Feynman then remarked that he also had a covariant formulation. Schwinger later insisted, however, that at the time of the January 1948 APS meeting

from Oppenheimer that Tomonaga had discovered the same description at least four years previously.

In April 1948 Tomonaga wrote to Oppenheimer and sent him a collection of manuscripts. Oppenheimer telegraphed back encouraging him to write up a summary of his work, which he would arrange to have published in *Physical Review*. At the end of May, Oppenheimer received the summary, and sent it to the journal, along with a clarifying note.[82] That note refers to a difficulty Tomonaga had with what appeared to be a photon mass: Oppenheimer quoted Schwinger, in effect, stating that a sufficiently careful treatment should yield a zero photon mass. Tomonaga was not convinced by this argument; 'and he was right, for the real subtlety underlying the photon mass problem did not surface for another ten years, in the eventual recognition of what others would call "Schwinger terms." ' [90]

Schwinger mentioned that at this same time Tomonaga was also involved in cosmic ray research. Tomonaga published a paper with Satio Hayakawa on the deeply penetrating muon in 1949;[83] in the same year he published a book on quantum mechanics,[84] and came for a visit to the Institute for Advanced Study at Oppenheimer's invitation. There he worked on the quantum many-body problem, which Schwinger would turn to many years later.

But after Tomonaga's return to Japan, he soon had to assume Nishina's administrative duties upon the latter's death in 1951. He became President of the Tokyo University of Education in 1956, then the President of the Science Council of Japan in 1962, and in 1964 President of the Nishina Memorial Foundation. He retired in 1970 and wrote a couple of popular books on science. In this, too, Schwinger found a parallel with his own career, in this case with his BBC/Open University series on relativity.

As we see, Schwinger used much of his lecture on Tomonaga's career to advertize his own. In part, this was entirely justifiable in view of the striking parallels in their paths; Tomonaga's covariant approach to quantum electrodynamics anticipated many essential features of Schwinger's. If the experimental impetus had been available in Japan, Tomonaga's group might well have solved the problems of quantum electrodynamics first. But as we saw in detail in Chapter 8, Schwinger was deeply skeptical of that possibility, and because he did not esteem Tomonaga's contributions too highly, he found it very difficult to write this memorial lecture. That appears to be the true reason for the excessively self-referential tone.

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Feynman had done neither the Lamb shift nor the magnetic moment calculation; only months later, at the Pocono meeting, when Feynman congratulated Schwinger on 'getting it right,' had Feynman completed the latter calculation.[4]

### A path to quantum electrodynamics

On 15 February 1988 Richard Feynman died after a long and painful battle with cancer.* A month later, Julian celebrated his 70th birthday with a conference in his honor at UCLA, which he dedicated to Feynman's memory.† Although the two had never worked together, and had only intermittent contact,‡ they respected each other deeply; Julian Schwinger was greatly moved, indeed devastated, by Feynman's death.

A year later *Physics Today*, the semipopular professional magazine of the physicists, devoted a special issue to Feynman.[87] It included contributions by many of Feynman's friends and colleagues: John Wheeler, Freeman Dyson, Murray Gell-Mann, James Bjorken, David Goodstein, Daniel Hillis, and Valentine Telegdi. Of course, Julian Schwinger wrote about Feynman's contributions to quantum electrodynamics. Schwinger tried to capture Feynman's voice by quoting extensively from the latter's Nobel lecture.

Schwinger started by recalling their first meeting, when he visited Los Alamos from the MIT Radiation Laboratory to talk about waveguides and synchrotron radiation. Feynman looked glum. 'He began to lament the loss of irreplaceable time to do physics, of which I was keenly aware; we were both 27 years old. He said something like, "I haven't done anything, but you've already got your name on something." I still wonder what he was referring to.' [212]

But, as Schwinger pointed out, Feynman by that point *had* done quite a lot. He had begun to think about the problems of self-action of the electron, first, as an undergraduate at MIT, suggesting that electrons cannot act on themselves, but then, as a graduate student at Princeton, realizing that self-action was necessary to understand radiation resistance, required by energy conservation. With Wheeler, Feynman came up with the idea of replacing classical retarded electromagnetic interactions with 'an action-at-a-distance electrodynamics . . . that is half retarded, half advanced. . . . It was equivalent to the retarded description and contained the radiative resistance force, provided one assumed that any

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* For a detailed account of Feynman's long struggle, see [85].

† The symposium was actually held in honor of Julian Schwinger's 70th birthday. However, at the opening ceremony, he most graciously dedicated it as the 'Feynman Memorial Symposium.' This generous gesture was greatly appreciated by all those present.

‡ In the last week of January 1988, shortly before his death, Richard Feynman told Jagdish Mehra that he wanted to see and interact with Schwinger as much as possible, 'but here we are, within ten miles of each other, and in spite of numerous overtures by me, we don't meet. It has been a source of much regret to me.'[86] It was Schwinger's extreme shyness and difficulty in reaching out to people that kept him apart from even Feynman.

emitted radiation was totally absorbed within the complete system of charges.' [212]

They had also found that the symmetrical solution, unlike the retarded solution, admitted an action-principle formulation, as Adriaan Fokker had observed in 1929.[88] Feynman gave a lecture on this classical theory at Princeton, which was projected to be followed by Wheeler's talk on the corresponding quantum theory; Pauli correctly predicted that that talk would never be given. Also, at the classical level they suggested a modification of the electrodynamic interaction, replacing the delta function that enforced the interaction of a charged particle through a light signal traveling on the light cone by a smooth function $f$, which would make self-action finite; the ' "main effect of this self-action was a modification of the mass." '[212] Feynman had already achieved an integral approach, with an action that described the particle's entire path through space–time.

Schwinger next went on to describe Dirac's famous 1933 paper on 'The Lagrangian in quantum mechanics.'[89] Dirac advocated the use in quantum mechanics of the Lagrangian, which is expressed in terms of particle coordinates and velocities, rather than the Hamiltonian, expressed in terms of coordinates and momenta. He viewed the former as more fundamental. Dirac, of course, was the inventor of transformation theory. The transformation function from a description at time $t_2$ to a description at time $t_1$ is 'the product of all the transformation functions associated with the successive infinitesimal increments in time.' Dirac said the latter, that is, the transformation function from time $t$ to time $t + \mathrm{d}t$, *corresponds to* $\exp[(\mathrm{i}/\hbar)\mathrm{d}t\, L]$, where $L$ is the Lagrangian expressed in terms of the coordinates at the two times. For the transformation function between $t_2$ and $t_1$ 'the integrand is $\exp[(\mathrm{i}/\hbar) W]$, where $W = \int_{t_2}^{t_1} \mathrm{d}t\, L$.'

'Now we know, and Dirac surely knew, that to within a constant factor the "correspondence," for infinitesimal $\mathrm{d}t$, is an equality when we deal with a system of nonrelativistic particles possessing a coordinate-dependent potential energy $V$. . . . Why, then, did Dirac not make a more precise, if less general, statement? Because he was interested in a general question: What, in quantum mechanics, corresponds to the classical principle of stationary action?'

'Why, in the decade that followed, didn't someone pick up the computational possibilities offered by this integral approach to the time transformation function? To answer this question bluntly, perhaps no one needed it—until Feynman came along. He has described how, at a Princeton beer party, he was accosted by Herbert Jehle, newly arrived from Europe, who wanted to know what Feynman was working on. After telling Jehle about his struggles with electrodynamics, Feynman turned to Jehle and asked, "Listen, do you know any way of doing quantum mechanics starting with action?" As it happened, Jehle was aware of Dirac's early paper, and so Feynman found what he wanted, a formulation of quantum mechanics that could be applied to his classical action-at-a-distance electrodynamics—if one took for granted that Dirac's construction still worked when a Lagrangian did not exist. Feynman called this approach to quantum mechanics the path integral formulation because a value of the action $W$ is assigned to any sequence of intermediate coordinate values—to any path between the initial and the final coordinates—and all such values of $\exp[(i/\hbar)W]$ are added together.'

At this point Feynman could only describe electrons nonrelativistically. While he was at Los Alamos during the war, Feynman continued to think about these matters at odd moments, and discovered that his scheme did not conserve probability. He also made no attempt to do any actual calculations: '"I hadn't even calculated the self-energy of an electron up to that point [the Shelter Island Conference of 1947], and was studying the difficulties with the conservation of probability, and so on, without actually doing anything, except discussing the general properties of the theory."' [212]

After the experimental results were announced at Shelter Island, Feynman realized he had to learn how to do calculations. He still did not have a relativistic theory of electrons. He had to guess the form of the description of spin-$\frac{1}{2}$ electrons, and determine the relative signs empirically. '"I have tried to explain that all the improvements of relativistic theory were at first more or less straightforward, semi-empirical shenanigans. Each time I would discover something, however, I would go back and I would check it so many ways ... until I was absolutely convinced of the truth of the various rules and regulations which I concocted to simplify all the work."'

Feynman then published his results,[90] even though he could not justify his procedure. '"Often, even in a physicist's sense, I did not have a demonstration of how to get all of these rules and equations, from conventional electrodynamics. But, I did know from experience, from fooling around, that everything was, in fact, equivalent to the regular electrodynamics and had partial proofs of many pieces, although, I never really sat down, like Euclid did for the geometers of Greece, and made sure that you could get it all from a single simple set of axioms. As a result, the work was criticized, I don't know whether favorably or unfavorably, and the 'method' was called the 'intuitive method.' For those who do not realize it, however, I should like to emphasize that there is a lot of work involved in using this 'intuitive method' successfully. Because no simple clear proof of the formula or idea presents itself, it is necessary to do an unusually great amount of checking and rechecking for consistency and correctness in terms of what is known. ... In the face of the lack of direct mathematical demonstration ... one should make a perpetual attempt to demonstrate as much of the formula as possible. Nevertheless, a very great deal more truth can become known than can be proven."'

Feynman was still concerned, at the time of his Nobel lecture, with the failure of unitarity. 'With $\delta$ replaced by $f$ the calculations would give results ... for which the sum of the probabilities of all alternatives was not unity.... I believe there is really no satisfactory electrodynamics, but I'm not sure. Therefore, I think that renormalization theory is simply a way to sweep the difficulties of the divergences of electrodynamics under the rug. I am, of course, not sure of that." ' Feynman retained this skepticism about renormalization to the end of his life, a doubt that Schwinger did not share.

The concluding paragraphs of Schwinger's article consisted almost entirely of quotations from Feynman's Nobel lecture: 'It is most striking that most of the ideas developed in the course of this research were not ultimately used in the final result. For example, the half-advanced and half-retarded potential was not finally used, the action expression [for action at a distance] was not used, the idea that charges do not act on themselves was abandoned. The path integral formulation of quantum mechanics was useful for guessing at final expressions and at formulating the general theory of electrodynamics in new ways—although, strictly, it was not absolutely necessary. The same goes for the idea of the positron being a backward moving electron; it was very convenient but not strictly necessary.

'On looking back over the work, I can only feel a kind of regret for the enormous amount of physical reasoning and mathematical re-expression which ends by merely re-expressing what was previously known, although in a form which is much more efficient for the calculation of specific problems.... It must be remarked that although the problem actually solved was only such a reformulation, the problem originally tackled was the (possibly still unsolved) problem of avoidance of the infinities of the usual theory. Therefore a new theory was sought, not just a modification of the old. Although the quest was unsuccessful, we should look at the question of the value of physical ideas in developing a *new* theory.

'Theories of the known, which are described by different physical ideas, may be equivalent in all their predictions and are hence scientifically indistinguishable. However, they are not psychologically identical when trying to move from that base into the unknown.... If every individual student follows the same current fashion in expressing and thinking about [the generally understood areas], then the variety of hypotheses being generated to understand [the still open problems] is limited. Perhaps rightly so, for possibly the chance is high that the truth lies in the fashionable direction. But [if] it is in another direction, who will find it?'

Schwinger concluded this tribute with his own words: 'So spoke an honest man, the outstanding intuitionist of our age and a prime example of what may lie in store for anyone who dares to follow the beat of a different drum.' [212]

Although moving, these tributes to Tomonaga and Feynman appear rather stiff and formal. And indeed, when he gave the memorial lectures in Tokyo and San Francisco, respectively, his presentation was quite properly formal, consisting of standing at a lectern and reading from a prepared text. Thus he conveyed none of the excitement of his classroom lectures on physics, which were well rehearsed, but delivered without notes. Part of Schwinger's perceived coldness was based on his view of physics. His lectures on Tomonaga and Feynman, as his other historical lectures, largely consisted of quotations. Eugen Merzbacher, who was President of the American Physical Society when the Feynman tribute was organized,* remarked that 'he didn't want to involve the human being in physics, for that would spoil the esthetics.'[51]

## Celebration of his life

### Historical and philosophical talks

In addition to the tributes to Feynman and Tomonaga we have outlined above, Schwinger gave a number of historical and philosophical talks in the last two decades of his life. In Chapter 14 we have described the talk on 'Conflicts in physics', given at several places and referring to the role played by two relatively unknown scientists in the development of the atomic theory and the suppression of their ideas by the scientific establishment.† In the early 1960s he gave a nontechnical lecture on his philosophy of quantum mechanics and quantum field theory.[39] There, he expounded his current view of microscopic phenomena, with the distinction between particles (phenomenological) and fields (fundamental), a view elaborated a few years later in his Nobel Lecture. This interesting talk has been analyzed in detail in Schweber's book.[92]

Harvard University regularly sponsors a major series of public lectures, the Loeb Lectures, which feature popular talks by leading scientists. One month, in the mid-1970s, Paul Dirac and Julian Schwinger were Loeb Lecturers at

* Merzbacher recalled that he wondered whether it was worthwhile inviting Schwinger to speak at the Feynman memorial session in San Francisco. He called Clarice, who said she'd call back. 'To my amazement, he wanted to do it.'[51]

† In this connection, Margaret Newmark, daughter of Rabi, offered two vignettes of Schwinger. One was about 1953 when she was in college, when she was invited by Clarice to join them for ice cream at Schrafts in Harvard Square. While she and Clarice conversed, Julian was silent and barely looked at her, like a child being brought along by his mother. Twenty-five years later, on the other hand, at Rabi's 80th birthday celebration (Rabi was born on 29 July 1898), where Schwinger gave his famous talk at the School of International Affairs at Columbia, he appeared as very well tailored and elegant, delivering an elegant paper. He looked like an English diplomat. Afterwards he was warm and outgoing. So different were his private and public persona.[91]

different ends of the month. The gist of Dirac's talk was 'always trust the mathematics, the physics is too murky,' while Schwinger said, in effect, 'never rely on mathematics, keep your eye glued to the physics.' But Schwinger, for all his strong phenomenological bent, is remembered for his formalism, while Dirac mistrusted his mathematics of the Dirac equation to believe that the negative energies solutions were the proton, not the anti-electron.[93]

One of the most remarkable talks of this kind was delivered only once, on 12 April 1973 at UCLA on the subject of Leonardo da Vinci. The handwritten manuscript exists in the UCLA archive.[39] As this brief lecture is revealing of Schwinger's attitude toward science, we shall quote it in full.

'By the XV century the fragmentary remains of mankind's intellectual heritage had largely come to light and were increasingly available in the vernacular. The concomitant renaissance of learning had one unfortunate tinge, however. Contemporary man was overawed by the accomplishments of the ancients and their works tended to be placed on such a lofty pedestal that, in effect, one Authority was replaced by another. It was Leonardo more than any other who began the transformation of this backward looking adoration of the classical period with the forward looking modern scientific viewpoint—that only through the direct observation and probing of nature can objective knowledge be acquired. He said, for example, "Whoever in discussion adduces authority uses, not his intellect, but rather memory." Nevertheless the classical texts were of great importance to him and he read widely among them. Yet it is revealing that he was particularly attached to Archimedes for, among the Greek founders of physics, he was unique in avoiding the danger of mixing philosophical (a priori) concepts with scientific reasoning. Rather, he also proceeded in the modern manner, in which a few relatively simple facts are, through the power of mathematical analysis, made the foundation of a logical structure that encompasses wide areas of experience. Observation combined with mathematical reasoning is the cornerstone. And Leonardo said, "there is no certainty where one cannot apply any of the mathematical sciences."

'It is fascinating to read among the notebooks the bits and fragments that show how far he was in advance of his age. At a time when the Ptolemaic geocentric doctrine was universally accepted, and at least 20 years before the publication of Copernicus, we read, "The sun does not move." Again, we find the memorandum "Construct glasses to see the moon large." This, one hundred years before Galileo! Let me emphasize the epistomological point here. Leonardo was saying this, I believe: Disregard the speculations of Aristotle, for example, on the structure of the moon—rather, use your own senses, amplified by the power of scientific instruments. Here is modern science indeed! In a study of bird flight: "All movement tends to be maintained." And: "Nothing whatever can be moved by itself, but its motion is affected by another. This other is force."

And finally: "An object offers as much resistance to the air as the air does to the object." The last is stated as a special case, and the language begs for scientific precision, but do we not have here the essence of the three laws of motion, 150 years before Newton?

'The great tragedy of all this, as you know, is that none of this marvelous insight and pioneering of new paths had the slightest influence on the actual evolution of science, with a possible exception that I shall mention later. It remained locked in the notebooks to be finally appreciated only several centuries after its revelation had been duplicated, and surpassed. It is idle to speculate how it might otherwise have been, if Leonardo had obeyed the modern injunction to Publish or Perish. He did neither. Would another Newton have appeared a century earlier? Or is it inexorable to wait on the fullness of time, until the roots have dug deeply enough to bear the next growth? Leonardo himself said: "Truth is the daughter of time." I wonder, incidentally, how many of you had a feeling of recognition on hearing that last phrase? Yes, part of it, the *Daughter of Time*, is the wonderfully apposite title selected by Josephine Tey for a delightful detective-historical study of Richard III and his slandered reputation.

'I have spoken of the modern character of his thinking. Nothing could be more modern than the moral conflicts he encountered in applying his technological knowledge to the engines of warfare, as Archimedes had done before him. But it was uniquely reserved to Leonardo to solve this problem in a particular way. After mentioning the possibility of constructing submarines that could stay underwater for as long as the crew could "remain without food" as he put it, he says, "this I do not publish or divulge, on account of the evil nature of men, who would practice assassinations on the bottom of the sea." And so they did, but only several centuries later. For us, unfortunately, technological censorship, whether self or externally imposed, is no longer an answer. That can only come when man has learned to hold in check his "evil nature."

'Having broached the subject of technology, let me turn to Leonardo the engineer. In his time, science and technology, principle and application, were not differentiated as they are today. Leonardo himself, starting as a gadgeteer, a trial and error empiricist, was driven to study and develop the mechanical principles that underlie and extend practical experience. I only mention his work on rolling friction, on pulleys, on gears, on the loading of structures. But let me briefly discuss his possible connection with the actual development of the steam engine. He designed and used the first steam calorimeter, in order to measure the expansion power of steam, which device incorporated a piston, driven by that power. Many years later some of these related ideas were published in similar form by Jerome Cardan (I use the English form of his name) who was notoriously light-fingered with other people's intellectual property and who, through his father, a personal friend of Leonardo, had direct access to the notebooks. There is more to the gossip, but I shall leave you with only the suggestion that, in this instance at least, Leonardo's pioneer work many not have been entirely wasted.

'The same Jerome Cardan was also not above a bit of malicious gossip, as when he wrote "Leonardo also attempted to fly but misfortune befell him from it. He was a great painter." The reference is, of course, to Leonardo's fixed preoccupation with the flight of birds and the attainment of artificial flight. Unfortunately, Leonardo's obsessive desire to have man fly preceded the scientific study of bird flight, and was based on the erroneous notion that flapping wings, driven by man's muscle power alone, would suffice. Only later, after studying the soaring flight of birds, and appreciating some of the physical principles of wing design, did he approach the ideas of fixed wings and mechanical power. But by then it was too late. Nevertheless, he did invent the parachute and produce a mechanically driven helicopter design.

'As Cardan noted, he was a great painter. But art and science were not two different cultures for Leonardo, nor should *we* accept that artificial dichotomy. Leonardo said, "Painting, the sole imitator of all the visible works of nature, is truly a science and the time-born daughter of nature." Art and science, then, are simply two different paths to the study and understanding of nature, which is the great teacher. The humanism of which we spoke had its greatest impact on literature. Very little of classical art had survived, and humanism took a more original turn when it focussed on painting and sculpture. And it was the desire of the artists to improve their command over materials and techniques that finally brought humanism to science. In Leonardo's case, the preliminary sketches and studies for paintings and sculptures led inexorably through anatomical and other investigations to the preoccupation with the universal laws of Nature that govern all things, animate and inanimate. Would that the world's loss, when he was drawn away from painting, had been recompensed by the enormous impetus to physical and anatomical knowledge that publication of the notebooks would have produced.' Thus ended Schwinger's tribute to Leonardo.

Schwinger gave a few other pronouncements on the importance of the scientific method. In a conference discussion with Glenn Seaborg, John Eccles, and other leading scientists, Schwinger had an eloquent statement for the values of science.[39] 'Too often scientists, in justifying what they do and why society as a whole should support that effort, point to all the practical benefits that come from it. It's well known that for every dollar, ten dollars, or whatever the number may be, will be returned to the community as a whole. But that is not the fundamental reason science is done. I think science should be supported basically for the same reasons that symphony orchestras are supported. There is no economic return from this, but we all know its cultural importance. So I would emphasize the other side of the coin—the spiritual, the cultural values.'

The discussion took place about the time that the Fermilab accelerator was being planned. 'I agree that it is difficult to convey to the public at large why this particle accelerator is desirable, but I do believe it is possible and in fact mandatory to convey to the public at large the thrill of this hunt, the intellectual, and to some extent, the practical importance of it. ... Surely a feeling of the mystery and the awe of nature that goes with this can be transmitted.'[39]

In a similar vein, Schwinger pleaded for the communication of the scientific attitude in a historical session with Rabi and others held in November 1985.[39] 'Science is a world view. And a world view based on a method, and a method that everyone can apply in his own life. I mean, in his life activities of skepticism, of testing, and so forth. And that's what we have to convey to the population at large, not the detailed discoveries and so forth, but the intellectual attitude. And it is communicable. And we are failing to do it.'[39]

One of Schwinger's most eloquent statements of the importance of research was published in 1964 [124a]. We quote it in full: 'The scientific level of any period is epitomized by the current attitude toward the fundamental properties of matter. The world view of the physicist sets the style of the technology and the culture of the society, and gives direction to future progress. Would mankind now stand on the threshold of the pathway to the stars without the astronomical and mechanical insights that marked the beginning of the scientific age? The quest for understanding has led outwardly to the galaxies and inwardly to the atom and then to the nucleus. Now it is the subnuclear world that is being actively explored. The goal here is not merely an organizing principle for subnuclear particles, a new periodic table of the elements, interesting and important as that may be. Rather we are groping toward a new concept of matter, one which will unify and transcend what are now understood only as separate and unrelated aspects of natural phenomena. In past triumphs, physics has unified light with electromagnetism, mass with energy, and comprehended chemistry and the mechanical–thermal properties of bulk mater in the atomic laws of quantum mechanics. But the fundamental problems remain. What is the role of gravitation in coupling the remote stars to the atom? Can one understand the magnitude of the unit of electrical charge? These are traditional queries. Recent research has provoked a whole battery of additional questions. What is the relation between the newly revealed internal degrees of freedom and space–time? How can one connect the diverse interactions, of different strengths and characteristics, that are required to account for the birth and death of subnucleonic particles?

'But perhaps the most important question concerns whether these particles must be accepted as basic and unanalyzable, to be described only in their own framework, or whether there exists a simple and more fundamental substructure, a deeper level of description and understanding. There alternatives have been presented before in the history of physics. At the close of the 19th century it was strongly argued that the properties of bulk matter should not be accounted for by the characteristics of unobservable and hypothetical microscopic entities. Owing to the continued development of experimental technique, this limited viewpoint had to be discarded and the atomic theory triumphed. A similar decision can be given again only if the tools will be at hand to continue the penetration into the totally new, totally unpredictable world of the microcosmos. And one should not overlook how fateful a decision to curtail the continued development of an essential element of the society can be. By the 15th century, the Chinese had developed a mastery of ocean voyaging far beyond anything existing in Europe. Then, in an abrupt change of intellectual climate, the insular party at court took control. The great ships were burnt and the crews disbanded. It was in those years that small Portuguese ships rounded the Cape of Good Hope.'

## Schwinger's death and tributes

After Julian was diagnosed with pancreatic cancer, Clarice told relatively few friends.* A letter to the Baños' was misaddressed, so they never knew he was ill until Clarice called Alice to tell her Julian had died that morning. But the Kivelsons knew of the illness. Margaret and Daniel visited Julian in the hospital near the end. 'Unfortunately, when he was in the hospital, he was really miserable, very, very sick, very brave. I think he was glad that we had come. I don't think Clarice let many people come. It was really so fast, nobody would believe it. We were grateful that Clarice gave us a chance to say goodbye.'[28]

Seth Putterman also visited him two days before his death. 'He did not want to talk about history but about physics.' So Seth told him of the remarkable fact that sonoluminescence requires about a 1% admixture of noble gas in the air, and that water was the most favorable liquid. Julian thought for a moment, and remarked, 'It probably has something to do with evolution.'[95]

Schwinger died on 16 July 1994 feeling unappreciated. David Saxon reflected on this. 'What was Julian hungering for? Was it more recognition or the demanding job of living up to his own standards? The "Greening of quantum field theory" [229] was a wonderful paper in many ways, but kind of sour. Recognition was a big deal.'[8]

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* Wolfgang Pauli, a physicist whom Schwinger greatly admired, even though they had several confrontations, also died of pancreatic cancer on 15 December 1958. His early death caused Schwinger to lose weight and take better care of his health and fitness. More relevant to Schwinger's death was the fact that his father and brother apparently both also died of pancreatic cancer.[94]

A month after Julian's death, in August 1994, Clarice held a memorial service in her home. A few of his old friends and colleagues were able to attend. These included David Saxon, Paul Martin, Robert Finkelstein, and others, and informal moving tributes were offered. More publicly, in the year after his death in July 1994 three special physics gatherings were held in his honor. The first was organized by Abe Klein at Drexel University in September. That symposium was noteworthy for the eloquent reading by Freeman Dyson of Schwinger's last public lecture, 'The Greening of quantum field theory: George and I' given at Nottingham on his award of an honorary degree in 1993 [229].* (This was discussed in detail in Chapter 9.) The second meeting was organized by David Saxon and held at UCLA in October, while the third was held at the joint meeting of the American Physical Society and the American Association of Physics Teachers in Washington in April 1995. The talks given in these public symposia were collected in a volume edited by Jack Ng.[18] We have quoted extensively from these tributes throughout this book.

A year or so after his death, Clarice delivered a moving summary of their life together. It seems fitting to end this book by quoting it in full, since Clarice and Julian were nearly inseparable for 47 years.

'Nostalgia was not one of Julian's strong points, but whenever the subject of Columbia came up, he spoke of it with warmth and affection.

'I came into the world of physics a complete and utter stranger.

'Shortly after our engagement, a friend lent me a novel describing extramarital affairs among members of the English and History Departments at Harvard. I remember that when I returned it, I said, "Maybe in English and History, but not in Physics."

'Then came the introduction of the Hamiltonian, Green's functions, and my favorite, magnetic moment—surely a better name for a perfume than Poison.

'Life with Julian was fun. While his very being was immersed in the wonder and beauty of physics, he was interested in, and cared deeply about, many things—the country, our constitution, the environment, people both collectively and individually, music, skiing, tennis, travel, poetry, archaeology,

* Also given at Nottingham, and read by Dyson, was Schwinger's acceptance speech. There he referred again to the threats to science, again recalling the destructive self-isolation of the Chinese government in the 15th century. He also recounted his then recent success in teaching basic quantum mechanics to high school students. In remarks that fly in the face of Schwinger's presumed elitism, he said, 'They understood it, they loved it. And I used no more than a bit of algebra, a bit of geometry. So: catch them young; educate them properly; and there are no mysteries, no priests. It all comes down to a properly educated public.'[18]

languages. Among his happiest language lessons were those in Mayan given him by a guide when we were in the Yucatan. I have always maintained that when traveling in foreign countries every family should have one person to speak the required language. In our family, it was he. (I would point, smile, and pay.)

'For some time he continued his diet of steak and chocolate ice cream, even as he encouraged me to order all manner of exotic dishes. In due time, however, he became much more adventurous than I; and to his delight developed a discerning palate for wine.

'Soon after we were married, he began piano lessons. The first year with a New England Conservatory disciplinarian, and then with a Longy School teacher who taught sight reading. While he didn't become a Weisskopf or a Francis Low, he played well enough to afford himself pleasure and caused us no suffering. I recently received a note from a woman who lived across the street during our first temporary year in LA, saying how much she had enjoyed hearing him play at one in the morning. Talk about kindness!

'On our honeymoon we drove across the country, stopping at all the schools at which he had consumed hamburgers and chocolate milk shakes. When we came to Pikes Peak we got out of the car, cameras in hand. He had to photograph his beloved mountains, I, to take a picture of a tiny wild flower on my side of the car. He, with his long vision, I with one short as my nose.

'As McCarthyism fever mounted, I immediately went into sack cloth and ashes. Not Julian, he was confident that the American people would not tolerate it, and he was right.

'He loved teaching and enjoyed interacting with his some 70 students, two [now three] of whom became Nobelists. He was really a quite wonderful lecturer. I would attend his talks at various meetings and sometimes think if only I'd pay attention I would understand what he was saying. Instead, I would sit in back of the hall, watch the audience, and be able to tell him that even though he might feel he'd not done well enough, with rare exceptions his audience had genuinely enjoyed his talk. I could hear them say as much on their way out. Someone has apparently said that Schwinger did what he did to show that only Schwinger could do it. What a donkey! Not to understand the joy of accomplishment and the passionate excitement of sharing it.

'We both took up skiing. After a few years I made a subtle statement—I purposely left my boots behind at the end of a sabbatical in Japan. They hurt. Not Julian. His boots hurt, too, but he stayed with it and became a good intermediate skier.

'It took a bit of doing to get used to physics parties. Almost as soon as we came in, some physicist would aim and pierce Julian to a wall and tell him about his own work. A pale Julian would emerge, to be revived at dinner by sitting between two women and having an interesting and amusing exchange. He did well with women and children ... and cats. A little niece was asked why she loved Uncle Julian and she said, "Because he listens."

'He changed my life in many ways. For one thing, I didn't have to know very much. I had only to ask him and I'd have the answer. Sometimes we resorted to a reference source but more often than not he would simply tell me.

'Before 8 June 1947, I was an early to bed, early to riser. After that date a note was pasted above our door bell, "Please do not ring before 11 a.m." Then he succumbed to the lure of Southern California and year round tennis. He was told that he must not eat for two hours before playing. Suddenly 25 years of training was for naught. Breakfast appeared at 8:30 to accommodate an 11 o'clock tennis date.

'He had extraordinary perception and understanding, keen wit and delightful sense of humor. He was honest, kind, and generous.

'The world that knew him will never be the same.'[96]

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# Julian Schwinger—list of publications

This incorporates and updates the publication list found in *Selected papers (1937–1976) of Julian Schwinger* edited by M. Flato, C. Fronsdal, and K. A. Milton (Reidel, Dordrecht, 1979).

Legend: A = Abstract (APS meeting indicated), L = Letter


[0] On the interaction of several electrons, unpublished (1934).

[1] On the polarization of electrons by double scattering (with O. Halpern), *Phys. Rev.* 48, 109 (1935). [L]

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[6] Depolarization by neutron–proton scattering (with I. I. Rabi), *Phys. Rev.* 51, 1003 (1937). [A (Washington 4/37)]

[7] Neutron energy levels (with J. Manley and H. Goldsmith), *Phys. Rev.* 51, 1022 (1937). [A (Washington 4/37)]

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[11] The neutron–proton scattering cross section (with V. Cohen and H. Goldsmith), *Phys. Rev.* 55, 106 (1939). [L]

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# PhD students of Julian Schwinger

| **Name** (Year of dissertation) | Title of dissertation |
|---|---|
| Richard L. Arnowitt (1952) | The hyperfine structure of hydrogen |
| Raphael Aronson (1951) | Photon isobars and high energy proton–proton scattering |
| Marshall Baker (1957) | Determinantal methods applied to meson–nucleon collisions |
| Pradip M. Bakshi (1962) | Expectation value formalism for quantum field theory and high energy processes |
| Stephen Robert Barone (1967) | Application of field theoretic methods to some topics in quantum electronics |
| Gordon Alan Baym (1960) | Field theoretic approach to the properties of the solid state |
| Jeremy Bernstein[1] (1955) | Electromagnetic properties of the deuteron |
| David George Boulware (1962) | Aspects of the foundations of quantum electrodynamics |
| Malcolm K. Brachman (1949) | Relativistic corrections to nucleon magnetic moments |
| Lowell Severt Brown (1961) | Field theory of unstable fermions and quantum electrodynamics with an intrinsic magnetic moment |
| Kenneth M. Case (1948) | The magnetic moments of the neutron and proton |
| Lai-Him Chan (1966) | Phenomenological field theory |
| Shau-jin Chang (1967) | Selected topics in quantum field theory |
| Roger E. Clapp (1949) | A variational solution of the nuclear three-body problem |

[1] Joint student with Abraham Klein

| **Name** (Year of dissertation) | Title of dissertation |
|---|---|
| Donald Clark (1994) | Bound states of magnetic charge |
| Melville Clark (1949) | On the hyperfine structure of deuterium |
| Frederic deHoffmann (1948) | Neutron–deuteron scattering at high energies |
| Lester Leroy DeRaad, Jr. (1970) | Some aspects of higher-order source theory |
| Stanley Deser (1953) | Relativistic two-body interactions |
| Carl Bryce Seligman (1949) [DeWitt] | I. The theory of gravitational interactions<br>II. The interaction of gravitation with light |
| Julian Eisenstein (1948) | Part I. An application to the deuteron problem of the variation-iteration method for solving linear differential equations<br>Part II. Neutron–proton scattering at intermediate and high energies (with Fritz Rohrlich) |
| Stanley Jay Engelsberg (1960) | Energy losses of fast particles: collective excitations |
| David S. Falk (1959) | Approximation methods in the theory of Green's functions |
| David Feldman (1949) | On the radiative corrections to the Klein–Nishina formula |
| Sheldon Glashow (1958) | The vector meson in elementary particle decays |
| Roy J. Glauber (1949) | The relativistic theory of meson fields |
| Norman Jacob Horing (1963) | Quantum theory of electron gas plasma oscillations in a magnetic field |
| Lawrence Paul Horwitz (1957) | Low energy applications of meson field theory |
| Richard John Ivanetich (1969) | Source theory and double spectral forms |
| Kenneth Alan Johnson (1955) | Quantum electrodynamics of the scalar field |
| Evangelos Karagiannis (1990) | Radiative polarization of a rotating charge and construction of Green's functions |
| Julian Keilson (1950) | The splitting of $L$-shells in heavy nuclei by the tensor interaction |
| Margaret Galland Kivelson (1957) | Bremsstrahlung of high energy electrons |
| Abraham Klein (1950) | Part I. Electromagnetic properties of nucleons<br>Part II. The coupling of a Kemmer field to a Dirac field |

| Name (Year of dissertation) | Title of dissertation |
|---|---|
| Daniel J. Kleitman[2] (1958) | Static properties of heavy Fermi particles; deuteron–nucleon scattering at high energies |
| Walter Kohn (1948) | Collisions of light nuclei |
| Roger B. Lazarus (1950) | Two-body interactions in Quantum-electrodynamics |
| Joseph V. Lepore (1948) | Neutron scattering from helium and the polarization of neutrons and protons by scattering |
| Michael Lieber (1967) | Application of the hidden symmetry group of the non-relativistic hydrogen atom |
| Bernard Lippmann (1948) | Scattering of slow neutrons by bound protons: a variational calculation |
| David Dexter Lynch[3] (1967) | Expectation value formalism for many-particle systems. Some transport coefficients of an electron gas in a magnetic field |
| Kalyana T. Mahanthappa (1961) | Multiple production of bosons in quantum field theory |
| Bertram J. Malenka (1951) | Non-linear meson theory and shell structure |
| Paul Martin[4] (1954) | Bound state problems in electrodynamics |
| Bruce Howard McCormick (1955) | Two investigations in meson theory in the non-relativistic limit |
| Eugen Merzbacher (1950) | Higher order effects in beta-decay |
| Kimball Alan Milton (1971) | Unitarity and vertex functions: some analyses in source theory |
| Ben R. Mottelson (1950) | The ground state of lithium 6 and lithium 7 |
| Maurice Neuman[5] (1949) | On the interactions of mesons with an electromagnetic field |
| Roger G. Newton (1953) | Radiative corrections to Coulomb scattering |
| Yee Jack Ng (1974) | Electron–electron scattering and hyperfine structure of positronium |
| Hwa-tung Nieh (1966) | Selected topics in the phenomenological theory of matter |

[2] Joint student with Roy Glauber

[3] Joint student with Paul Martin

[4] Joint student with Roy Glauber

[5] Joint student with Wendell Furry

| Name (Year of dissertation) | Title of dissertation |
|---|---|
| Alfred Tredway Peaslee, Jr. (1955) | Electrodynamic modes of isomeric decay |
| Alain Joseph Phares[6] (1973) | Schwinger's modified propagation function and restrictions on the three and four-point vertex functions |
| Alfred Franz Radkowski (1968) | Some aspects of the source description of gravitation |
| Robert Barnett Raphael (1954) | Phenomenological methods in nucleon–nucleon scattering |
| Fritz Rohrlich[7] (1948) | Part I. Neutron–proton scattering at intermediate and high energies (with Julian Eisenstein)<br>Part II. On the configurations $d^2sp$ $d^3p$ $d^7p$<br>Part III. The classification of the odd terms in Ti I |
| Raymond Francis Sawyer (1958) | $K$ mesons and hyperon decays |
| Charles Michael Sommerfield (1957) | The theory of the magnetic dipole moment of the electron |
| Adolph Stern (1951) | On a model for exchange magnetic moments in nuclei |
| Robert Nicol Thorn (1953) | The production of proton pairs |
| Wu-yang Tsai (1971) | Selected topics in source theory and higher spin theory |
| Luis Fernando Urrutia (1978) | Selected problems in source theory |
| Charles Warner III (1958) | Associated production and strong interactions |
| Robert Lee Warnock (1959) | Approximation methods in the theory of strong interactions |
| Harold Weitzer (1958) | Hyperon–nucleon interactions |
| Walter Wilcox (1981) | Calculational aspects of quantum gravity |
| Gregg Wilensky (1981) | Problems in gravitation and quantum mechanics |
| Hiroshi Yamauchi (1950) | High energy proton–proton scattering |
| Tung-mow Yan (1968) | Selected topics in the theory of magnetic charge and the phenomenological theory of particles |

[6] Joint student with Richard Ivanetich

[7] Joint student with John H. Van Vleck

| **Name** (Year of dissertation) | Title of dissertation |
|---|---|
| York-Peng Edward Yao (1964) | Some aspects of quantum field theory |
| Asim Yildiz (1973) | Charged particles in a homogeneous magnetic field |
| Ariel Charles Zemach[8] (1955) | I. Neutron diffraction<br>II. Size of the proton magnetization |

[8] Joint student with Roy Glauber

[redacted]

# Index of names

# Index of subjects

*Note*: a list of Julian Schwinger's publications will be found in Appendix A. JS denotes Schwinger he did not use the form JSS. CC denotes Clarice Carrol, later Schwinger

**Plate** 1 Julian Schwinger as a child of age 3, 1921.

**Plate 2** Julian Schwinger in 1931, age 13, while a student at Townsend Harris High School.

**Plate 3** Julian Schwinger, Professor at Harvard University, 1948.

Plate 4 Wolfgang Pauli visiting Julian Schwinger and other members of the Physics Faculty at Purdue University, Fall 1942.

Plate 5 Photograph taken at Harold Schwinger's wedding in 1944. Left to right: Benjamin Schwinger, Bella Schwinger, Harold Schwinger, Jeanne Schwinger (Harold's bride), Julian Schwinger, and Jeanne's parents.

**Plate 6** Julian Schwinger on his visit to Los Alamos in July, 1945, seated between Bernard Feld, left, and Norman Ramsey, right.

**Plate 7** Julian Schwinger and J. Robert Oppenheimer in Berkeley, California, 1948.

**Plate 8** Julian Schwinger, Bernard Lippmann, Harold Levine, and Clarice Schwinger, in Washington, D.C., May 1948.

Plate 9 I.I. Rabi, Stephen White, Julian Schwinger, Edwin McMillan, and Robert E. Marshak at the joint meeting of the Italian and Swiss Physical Societies at Lake Como in 1949.

Plate 10 Photograph of some of the participants at the Shelter Island Conference in June 1947. Standing are Willis Lamb and John A. Wheeler, and seated are Abraham Pais, Richard Feynman Herman Feshbach, and Julian Schwinger.

**Plate 11** Julian and Clarice Schwinger at Rockport, Massachusetts, 1951.

**Plate 12** Julian Schwinger receiving an honorary D.Sc. degree from Harvard University, Commencement, June 1962.

**Plate 13** Frances Townes, Julian Schwinger, Charles Townes, and Elisabeth Heisenberg at a picnic on the grounds of Duino Castle, near Trieste, Italy, Summer 1968.

Plate 14 Robert E. Marshak, Abdus Salam, and Julian Schwinger on a ferry in Puget Sound, during the International Congress on Theoretical Physics in Seattle, Washington, September 1956.

**Plate 11** Julian and Clarice Schwinger at Rockport, Massachusetts, 1951.

**Plate 12** Julian Schwinger receiving an honorary D.Sc. degree from Harvard University, Commencement, June 1962.

**Plate 13** Frances Townes, Julian Schwinger, Charles Townes, and Elisabeth Heisenberg at a picnic on the grounds of Duino Castle, near Trieste, Italy, Summer 1968.

Plate 14 Robert E. Marshak, Abdus Salam, and Julian Schwinger on a ferry in Puget Sound, during the International Congress on Theoretical Physics in Seattle, Washington, September 1956.

**Plate 15** Julian Schwinger receiving the Nobel Prize for Physics from the King of Sweden on 10 December 1965.

**Plate 16** Julian Schwinger and Richard Feynman at the Nobel ceremonies in Stockholm, Sweden December 1965.

Plate 17 Julian Schwinger traveling with graduate students in Hokkaido, Japan, June 1970.

**Plate 18** Julian Schwinger and I.I. Rabi at the Nobel Prize winners' meeting in Lindau, July 1968

**Plate 19** Julian Schwinger lecturing a UCLA, November 1970.

**Plate 20** Participants in the Symposium on 'The Present and Future Goals of Science' in celebration of the Decennial Assembly of Tel Aviv University, at the Century Plaza Hotel, Los Angeles, California, 3 October 1973. Standing (L to R): Willis E. Lamb, Jr., Sir John Eccles, Robert Sinsheimer, Allan Sandage, Edwin McMillan, Owen Chamberlain, Leon N Cooper, Jagdish Mehra. Seated (L to R): Murray Gell-Mann, Emilio Segrè, Julian Schwinger (chairman), Felix Bloch, and Alfred Kastler.

**Plate 21** Morton Hamermesh, Julian Schwinger, and Herman Feshbach during Schwinger's 60th birthday celebration, Los Angeles, February 1978.

**Plate 22** I.I. Rabi, Julian Schwinger, and V.F. Weisskopf at Schwinger's 60th birthday celebration at UCLA, February 1978.

**Plate 23** Julian Schwinger and some of his students at Schwinger's 60th birthday celebration at UCLA, February 1978.

**Plate 24** Julian Schwinger delivering his tribute to Sin-itiro Tomonaga, co-recipient of the Nobel Prize in 1965, in Tokyo on 8 July 1980.

**Plate 25** Nobel Prize winner's meeting in Lindau, June 1979. Julian Schwinger with Paul Dirac, Pyotr Kapitza, Eugene Wigner, Felix Bloch, Emilio Segrè, Willis Lamb, Isidor Rabi, Samuel Ting, and others.

**Plate 26** Berthold-Georg Englert and Julian Schwinger at his Humboldt Prize ceremony, 1981.

**Plate 27** Julian Schwinger at the Nobel Prize winners' meeting in Lindau, Germany, 1982.

Plate 28 Julian Schwinger meeting with students at the meeting in Lindau, 1982.

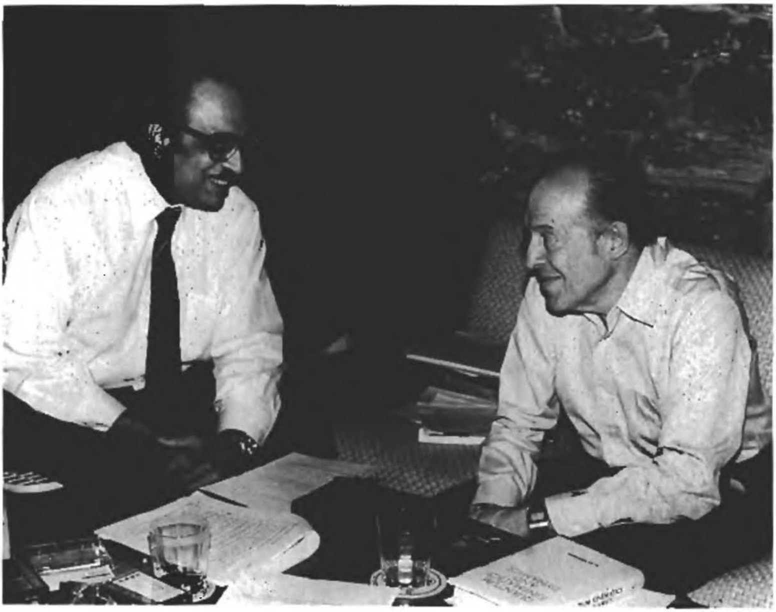

Plate 29 Jagdish Mehra and Julian Schwinger relaxing after a day's interview, Bel Air, California, March 1988.

**Plate 30** Julian Schwinger photographing the Matterhorn in Zermatt, Switzerland, perhaps in 1949.

**Plate 31** Julian and Clarice Schwinger at a wine tasting at the V. Sattui Winery, St. Helena, California, September 1989.

**Plate 32** Julian Schwinger and Kimball A. Milton in Schwinger's office at UCLA, Spring 1976.

**Plate** 33 Walter Kohn, Julian Schwinger, and Sidney Borowitz outside Schwinger's house in Bosto 1949. Kohn and Borowitz had both been assistants to Julian Schwinger, and later became lecture at Harvard University; Walter Kohn won the Nobel Prize for Chemistry in 1998.

**Plate** 34 Julian Schwinger, Edward Teller, and Jagdish Mehra together at the fundraising banqu for the State of Israel and Tel Aviv University in Los Angeles, 3 October 1973.