Conversations with Albert Einstein

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The following account of talks with Professor Einstein are the notes made by the writer in Princeton immediately after each of five visits about ten years ago. They were originally written without any thought of publication, but rather as a private record of very wonderful experiences. However, since they may contain matters of interest to others, it has been decided to publish them.

IT was my privilege to call on Professor Einstein in Princeton on five occasions in the period 1950–1954. The conversations at these visits dealt principally with the work of Albert A. Michelson at Case, the Michelson–Morley experiment and the studies which led to clarification of the results obtained by Dayton C. Miller at Mount Wilson. These conversations with Einstein were a rare and inspirational experience, and it has been suggested that they might be reported so that younger physicists who have not met Professor Einstein personally might get a better impression of the kind of man he was. The notes are exactly as written down at the time except for minor changes. Consequently, the reader will find certain repetitions, and perhaps a few discrepancies. These, however, only emphasize the fact that all Professor Einstein's comments were made from memory, often on events that had occurred 50 years before our meetings. Some references and footnotes have been added for clarification.

I. 4 February 1950

The first visit to Princeton to meet Professor Einstein was made primarily to learn from him what he really felt about the Michelson–Morley experiment, and to what degree it had influenced him in his development of the Special Theory of Relativity. I had not had experience in making appointments with such great men as Einstein, and I puzzled a good deal about how this should be done. Finally after considerable hesitation, I wrote directly to him stating the purpose of my visit and suggesting a day at the end of the New York meetings of the American Physical Society to call on him. I received an immediate reply from Miss Helen Dukas, Einstein's secretary, giving me a most friendly invitation to come to Einstein's office at the Institute for Advanced Study at 11:00 a.m. on the suggested day.

Needless to say, I was there in more than ample time and at last I could see him approaching on foot as was his custom. Exactly at 11:00 a.m. Professor Einstein finished his brisk walk from his home on Mercer St. to the Institute for Advanced Study, and after pausing to pick up his mail, he greeted me, shook hands and invited me into his office which was room 115. He was very friendly, informal, and courteous, instantly impressing one by his marvelous eyes—large, discerning, but above all, kindly. After first nodding me to a chair, he seated himself at his desk. His large office was attractively furnished but was completely devoid of gadgets, honors or mementos, although his desk was covered with papers, books and journals. It had an atmosphere of friendliness and scholarliness.

He began by asking me to remind him of the purpose of my visit and smiled with genuine interest when I told him that I wished to discuss the Michelson–Morley experiment performed at Cleveland in 1887. When I reminded him that Michelson had been Professor of Physics at Case for eight years, he nodded. I showed him the drawing of the apparatus as reproduced in my paper.[1] He seemed greatly interested, asking me to sketch on his blackboard the details of their method of floating the stone on mercury. I was a little hesitant, because the blackboard was covered with neat equations dealing with his new Unified Field Theory. But he waved his pipe and said, "Oh rub that out!" I made some sketches, and he commented on them with keen interest. He was especially pleased with the float design that required the use of so little mercury. His eyes brightened, and he chuckled at this. He asked me where the parts of the apparatus are now located, especially the stone, and when I told him it had disappeared, he said, "It would be nice if all the parts could be brought together again."

When I asked him how he had learned of the Michelson–Morley experiment, he told me that he had become aware of it through the writings of H. A. Lorentz,[2] but *only after 1905* had it come to his attention! "Otherwise," he said, "I would have mentioned it in my paper."[3] He continued to say the experimental results which had influenced him most were the observations on stellar aberration[4] and Fizeau's measurements[5] on the speed of light in moving water. "They were enough," he said. I reminded him that Michelson and Morley[6] had made a very accurate determination at Case in 1886 of the Fresnel dragging coefficient with greatly improved techniques and showed him their values as given in my paper. To this he nodded agreement, but when I added that it seemed to me that Fizeau's original result was only qualitative, he shook his pipe and smiled, "Oh it was better than that!" He thought Zeeman's[7] later precise repetition of this experiment was very beautiful. He seemed really delighted when I mentioned to him how elegant I had found (as a student) his method of obtaining the Fresnel dragging coefficient from his composition of velocities law of special relativity.

I asked Professor Einstein how long he had worked on the Special Theory of Relativity before 1905. He told me that he had started at age 16 and worked for ten years; first as a student when, of course, he could only spend part-time on it, but the problem was always with him. He abandoned many fruitless attempts, "until at last it came to me that time was suspect!" Only then, after all his earlier efforts to obtain a theory consistent with the experimental facts had failed, was the development of the Special Theory of Relativity possible.

This led him to comment at some length on the nature of mental processes in that they do not seem at all to move step by step to a solution, and he emphasized how devious a route our minds take through a problem. "It is only at the last that order seems at all possible in a problem."

I showed him what I had written in the paper about the Michelson–Morley experiment and the

[1] R. S. Shankland, Am. J. Phys. **17**, 487 (1949).

[2] H. A. Lorentz, Arch. Néerl. **2**, 168 (1887), and many later references.

[3] A. Einstein, Ann. Physik **17**, 891 (1905); also in English translation (Dover Publications, New York). My colleague, Professor L. L. Foldy makes the following comment: Although Einstein may have been unaware of the Michelson–Morley experiment in 1905, he does make reference in the second paragraph of his 1905 paper to "unsuccessful attempts to discover any motion of the earth relatively to the 'light medium'." It is not clear whether Einstein is here referring to v/c or $(v/c)^2$ experiments, particularly since in the next sentence he goes on to say "They suggest rather that, as has already been shown to the first order of small quantities, the same laws of electrodynamics and optics will be valid for all frames of reference for which the equations of mechanics hold good." There is an implication, but by no means a certainty, that the first reference is to second-order experiments such as the Michelson–Morley experiment, and suggests the possibility that Einstein was aware of the negative results of such experiments if not with the experiments themselves. Einstein derives the Lorentz–Fitzgerald Contraction but says nothing about whether there is experimental confirmation. The whole paper is rather strange in the respect that Einstein reveals very little about what he knows to be experimentally verified and in that he makes no *specific* references to the work of others. The paper in fact presents an enigma in that it is very difficult to see how much of the special theory of relativity is a pure mental construct and how much is an inference from experimental results (or a theoretical formulation of them) of which Einstein had knowledge. See also G. Holton, Am. J. Phys. **28**, 627 (1960).

[4] J. Bradley, Phil. Trans. Roy. Soc. (London) **35**, 637 (1728); G. B. Airy, Proc. Roy. Soc. (London) **20**, 35 (1871); **21**, 121 (1873).

[5] H. L. Fizeau, Compt. rend. **33**, 349 (1851); Ann. Chem. Phys. **57**, 385 (1859).

[6] A. A. Michelson and E. W. Morley, Am. J. Sci. **31**, 377 (1886).

[7] P. Zeeman, Proc. Amsterdam Acad. **17**, 445 (1914); **18**, 398 (1915).

Special Theory of Relativity. This he read eagerly, puffed at his pipe, and nodded approval. When I suggested that the notice given to Fitzgerald's contribution was perhaps a little overemphasized, he said, "Oh no, he had the idea to try to clear up the mess."

Einstein first met Michelson in Pasadena and considered him "a great genius—he will always be thought so in this field." Einstein added that it was very remarkable that Michelson with little mathematics or theoretical training and without the advice of theoretical colleagues[8] could devise the Michelson–Morley experiment. Michelson's instinctive feeling for the essentials of a crucial experiment without completely understanding the related theories, Einstein considered the surest sign of his genius. This he feels was in large measure due to Michelson's artistic sense and approach to science, especially his feeling for symmetry and form. Einstein smiled with pleasure as he recalled Michelson's artistic nature—here there was a kindred bond. The artist was greatly in evidence in the Michelson–Morley experiment. Einstein remarked, "Most people would think the experiment silly."[9] I remarked on Michelson's excellent eyesight and the great advantage this gave him in optical experiments. Einstein's eyes lit up and he said, "Oh, but behind the eyes was his great brain!"

I mentioned the experiments which had disproved the Ritz emission theory of light[10] especially de Sitter's work on spectroscopic binaries[11] and the null result obtained by D. C. Miller[12] at Case with the modification of the Michelson–Morley experiment using sunlight. He said he knew de Sitter well but told me he considered the most decisive experiment along these lines to be the repetition of the Michelson–Morley experiment performed with starlight at Heidelberg by a student of Lenard's (Tomaschek)[13]; because here, the high radial velocities involved made the null results really decisive in establishing the speed of light to be independent of the motion of the source. Here we have the true measure of Einstein the man. Lenard, who along with Stark, was the most violently Nazi of all German scientists was referred to by Einstein with complete fairness and with not the slightest trace of malice or bitterness.

This led him to a discussion of emission theories of light, and he told me that he had thought of, and abandoned the (Ritz) emission theory before 1905. He gave up this approach because he could think of no form of differential equation which could have solutions representing waves whose velocity depended on the motion of the source. In this case, the emission theory would lead to phase relations such that the propagated light would be all badly "mixed up" and might even "back up on itself." He asked me, "Do you understand that?" I said no and he carefully repeated it all. When he came again to the "mixed up" part he waved his hands before his face and laughed, an open hearty laugh at the idea!

Then he continued, "The theoretical possibilities in a given case are relatively few and relatively simple, and among them the choice can often be made by quite general arguments. Considering these tells us what is possible but does not tell us what reality is."

When I suggested that Ritz's theory was the best of the several emission theories of light, he shook his head and replied that Ritz's theory is very bad in spots.[14] But he quickly added, "Ritz made a great contribution when he showed that frequency differences are the crucial thing in spectral series."

I then asked Professor Einstein if he had considered Michelson and Gale's interferometer

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[8] This is in general true, but the indirect influence of James Clerk Maxwell was probably decisive in directing Michelson's interests away from the speed of light measurements to the other problem. While at the Nautical Almanac Office in 1879 Michelson was able to study a letter from Maxwell to David Peck Todd [see Nature **21**, 314 (1880); Proc. Roy. Soc. (London) **A30**, 109 (1880)] which discussed basic matters on the possibility of detecting the earth's motion through space by optical observations. Professor Todd's reply to Maxwell dated May 19, 1879, has recently been made available to the writer through the courtesy of his daughter, Mrs. Millicent Todd Bingham, of Washington, D. C.

[9] Just as my father, F. N. Shankland; my uncle, S. D. Shankland; and Mr. S. S. Wilson told me that in their student days at Western Reserve University the Michelson–Morley experiment was referred to as though it had been a failure, since it gave a null result.

[10] W. Ritz, Ann. Chem. Phys. **13**, 145 (1908).

[11] W. C. de Sitter, Proc. Amsterdam Acad. **15**, 1297 (1913); **16**, 395 (1913).

[12] D. C. Miller, Proc. Natl. Acad. Sci. U. S. **11**, 311 (1925).

[13] R. Tomaschek, Ann. Physik **73**, 105 (1924).

[14] A. Einstein, Physik Z. **10**, 185 (1909).

measurement[15] of the earth's rotation important. He said, "Oh, yes, that is Sagnac's experiment[16] with a small velocity and a large area." He thought the Michelson–Gale experiment very beautiful but added that "there had existed no theoretical doubt" as to its outcome.

This was followed by some brief comments on the new unified field theory. He said that somehow it must also be made to include "atomicity." When I asked if he had any experiments in mind relating to the new theory he said, "No, all advances must now wait on great developments in mathematics." The solutions of the nonlinear partial differential equations of the theory must be exact to be useful, and any solution obtained by approximation methods, such as perturbation theory, would not be helpful. He felt that only when such rigorous solutions were obtained would it be possible to see how "atomistic" phenomena will fit into the unified field picture and that until this is done, the situation will continue to be very unsatisfactory.

Here, Einstein made some general observations about atomic theory and quantum mechanics and said at the outset, "You know I am in disagreement with most of my colleagues on the quantum theory." He felt that they were not "facing the facts" in their present methods. In fact, he spoke much more strongly than this and several times said that they had "abandoned reason" and that quantum-mechanical physics "avoids reality and reason." He spoke several times of Bohr whom he greatly likes and admires but with whom he disagrees in many fundamental ways. He said that Bohr's thinking was very clear but that "when he begins to write he becomes very obscure and that "*he thinks of himself as a prophet.*" Here I had difficulty in deciding whether Einstein was a bit stubborn or whether he was really convinced that quantum mechanics must change its viewpoint in a very fundamental way to make further progress possible.

Our conversation then returned to the Michelson–Morley experiment and the Special Theory of Relativity. I could not help feeling that this elegant special theory, the product of his youthful efforts, held the place nearest to his heart. I asked him if he felt that writing out the history of the Michelson–Morley experiment would be worthwhile. He said, "Yes, by all means, but you must write it as Mach wrote his *Science of Mechanics*." Then he gave me his ideas on historical writing of science. "Nearly all historians of science are philologists and do not comprehend what physicists were aiming at, how they thought and wrestled with their problems. Even most of the work on Galileo is poorly done." A means of writing must be found which conveys the thought processes that lead to discoveries. Physicists have been of little help in this because most of them have no "historical sense." Mach's *Science of Mechanics*, however, he considered one of the truly great books and a model for scientific historical writing. He said, "Mach did not *know* the real facts of how the early workers considered their problems," but Einstein felt that Mach had sufficient insight so that what he says is very likely correct anyway. The struggle with their problems, their trying everything to find a solution which came at last often by very indirect means, is the correct picture. He also expressed the highest regard for Laue's scientific writings.

I referred to Einstein's visit to D. C. Miller at Case in 1921; an event which I am sure was decisive in Miller's work on the aether-drift experiments from 1921 through 1926. He told me that when he came to the United States that year, he did not know a word of English. On the trip he picked up some by ear. He told me, "I am the acoustic type; I learn by ear and give by word. When I read I hear the words. Writing is difficult, and I communicate this way very badly." He added that he never really felt sure of the spelling of any English word. He told me that he even hated to write his *Autobiographical Notes* in German.[17]

[15] A. A. Michelson and H. G. Gale, Astrophys. J. **61**, 137, 140 (1925). Nature **115**, 566 (1925). In this experiment, two beams of light were made to travel in opposite directions in a partially evacuated pipe which enclosed a large area—at that time a field—now built up as the Clearing Industrial Area near the Chicago O'Hare airport. The earth's rotation affected the times of travel of the two light beams by amounts consistent with the predictions of both the special and general theories of relativity, and also the older aether theory.

[16] G. Sagnac, Compt. rend. **157**, 708 (1910); J. phys. **4**, 177 (1914). In this experiment, two beams of light travelled in opposite directions in an optical system rotated at high speed in the laboratory. Sagnac's measurements preceeded the general theory of relativity.

[17] Paul Arthur, Schilpp, *Albert Einstein, Philosopher–Scientist* (Library of Living Philosophers, Evanston, Illinois, 1949).

My lasting memory of Professor Einstein will be of his kindness and courtesy, his deep respect for the work and personalities of others. There was in his manner not the slightest condescension but rather an earnest desire that all my questions be answered and that his own views or position be clearly stated. He told me how the day before he had tried "to explain simultaneity to a layman." He said, "It is very hard sometimes to explain these things you know." Finally, when all my questions were answered, we both rose and he walked slowly to the door with me. On leaving I again shook his hand and said, "Thank you sir." He smiled again and the great deep eyes lit up as he replied, "Thank you!"

II. 17 November 1950

Up early and to the Pennsylvania Railroad Station for breakfast before taking the train for Princeton where I arrived at 9:20 a.m. It was a beautiful day, and I walked first to the Nassau Tavern and called the Institute for Advanced Study where the mathematics secretary arranged for me to see Einstein at 11:00 a.m. I walked to the Institute and after chatting with the secretary spent an enjoyable hour in the library and lounge. About eleven o'clock I started toward Einstein's office. He had arrived promptly at that hour, and when the secretary told him I was in the lounge, he started off full-tilt to get me! It was all the secretary could do to catch him and bring him back to his office door where I was waiting. He invited me in and after we were both seated, asked me about my visit. When I told him I wanted to tell him some of the things we had found in D. C. Miller's observations[18] he said, "Do you really think there is something in them?" When I replied that I felt confident that a thorough analysis of the observations might show that they were consistent with a null result, he was all interest and excitement. We both got up and went to the blackboard and for an hour worked there, walked around the room while talking, sat on the table, and had a wonderful time. He was far more animated than on my visit in February. Several times he exclaimed to me, "This is very beautiful!" I told him that it had always been a great puzzle to me why Miller's data seemed to yield this small positive result and that I had concluded that it might be due to his method of treating the data. On the blackboard I carried through a "sample run" in outline and explained my view that connecting the 16 averaged points by line segments was wrong. He emphatically agreed and added that what this really does to the data would require "a very complicated analysis." Then I told him of the Wood's Hole work on ocean wave analysis showing that mechanical analyses such as those given by Miller's Henrici Harmonic analyser might introduce false periods in finite sets of data.[19]

I outlined our plan to apply an auto-correlation analysis to Miller's data, and told him about the machine R. L. Stearns was building to carry out the analysis. He did not know of this method but immediately understood when I explained the process and he pointed out that it would not give phases and that these are more important for an "aether-drift" than amplitudes. This I agreed to but also replied that if the data seemed to give a null result in amplitude by this method of analysis, then the phases would not be a consideration. He repeated several times during our talk that since the phases found by Miller ("which fix the direction in space") were not consistent, this was the strongest argument against the drift reported by Miller. He said several times, however, that he (and also H. A. Lorentz) considered Miller an excellent experimenter and thought his data must be good. He emphasized that mere multiplication of data is not a good thing. This reinforced our resolve to concentrate on that part of Miller's data where he indicated "excellent conditions." Einstein also told me that H. A. Lorentz had studied Miller's work for many years and could not find the trouble.

Einstein asked about strains in the interferometer and the experimental conditions at Mount Wilson. He emphasized that if there is a systematic effect, however small, it must be

[18] D. C. Miller, Revs. Modern Phys. **5**, 203 (1933), and original data sheets given by Miller to the writer.

[19] Note added February 1955: It was not until early 1954 after the complete analysis of variance results were available that we were convinced that the periodic effects found by D. C. Miller were not due to statistical fluctuations or to his method of analysis. Only then did we plunge deeply into a study of the temperature effects to find the real cause of Miller's results (see reference 34). The essential correctness of Miller's harmonic analysis had meanwhile been proved by the autocorrelation analysis carried out by R. L. Stearns, M. S. Thesis, Case Institute of Technology (1952).

explained. He was most interested and encouraged me to carry the problem through.

I then told Professor Einstein about the unpublished 1924 Case data, and he was very interested. He fully understood why Miller did not consider the null results of this work significant because it was done in a basement room where the "drag" would be large. He pointed out, however, that there should be nearly as much "drag" at Mount Wilson as in the Case basement room due to the mass of the interferometer itself. He also reminded me that any "drag" would be inconsistent with aberration. I told him that I felt the 1924 results should be published as these seemed to be among the best of all data taken in these experiments. I also told him of Miller's observations[18] using sunlight as a source and that these seemed to me to be better than Tomaschek's.[13] He did not protest but nodded agreement. He likes Tomaschek's work with starlight, however, because of the large radial velocities.

We discussed the difference between the Morley–Miller result of 1904 and Miller's 1925–26 experiments.[18] He said that he had always been puzzled by the difference between these two groups of experiments. I suggested to Einstein that the discrepancy might be due to the method of analysis.[20] I showed him at his blackboard that by reporting the calculated velocity of the earth "through the aether" (v) as given by his experiments, Miller tended to find a greater average speed than would be the case if he had averaged the amplitudes of the fringe displacements (A).[21] Einstein agreed that our study should be confined to the experimental amplitudes and phases of the actual observed fringe displacements "*until a trend was certainly established.*"

Several times during our talk, Einstein said, "This is very nice" and laughed his hearty naive laugh. Several times also he asked me very seriously, "Why did not Miller find this?"[22] This seemed a great puzzle to him, one that I could not resolve, but I did tell him of Miller's great concentration on sound and the flute in his later years. He asked me about Miller's acoustical work, and when I told him of Miller's studies of vowels and their relation to Helmholtz's theory, he was much interested. He had not realized that Helmholtz had done this and exclaimed "What that man accomplished!" I also told Einstein of Miller's flute collection and this seemed to interest him a great deal.

Toward the end of our talk, I asked if Michelson had ever told him how he came to invent the interferometer. He said no. I told him that I felt it had been developed in Paris while he worked with Jamin and Mascart. He agreed that this was very probable and that Michelson's basic ideas for the Potsdam experiment had also evolved in Paris. He exclaimed at one point, "How many ideas Michelson originated for optical research!" Einstein then asked me what I thought about the supposed variability in the speed of light.[23] I told him I felt it was not a real effect.

At last I said goodbye after an hour of pleasure and inspiration with this great man—so kind, so gentle, so interested only in the truth.

III. 2 February 1952

I left New York on the 9:15 a.m. train and arrived at Princeton at 10:40. I taxied to the Institute for Advanced Study where the telephone girl told me that Einstein's secretary had called to leave word that he was coming over to see me. "He is expecting you," she said. At 10:55 I saw him walking briskly along the path, from the direction of Oppenheimer's white house, in stocking cap, long coat and cigarette. He came in, spoke to the telephone girl and then came rapidly down the corridor towards his office where I stood. He extended his hand and very cordially invited me into his office (room 115). He had apparently put his cigarette into his coat pocket, and as we took off our coats he had a small conflagration in his. When I thanked him for coming over to see me he said, "I always come to the office." He sat down behind his desk and invited me to be seated. Then in a most friendly way he asked me, "What is your question?"

I asked him if he had heard of the recent work

[20] Note added in June 1962–our later studies (reference 34) make it certain that the differences were in fact due to the greatly differing temperature conditions existing in the basement laboratory at Case and at Mount Wilson.

[21] This is due to the relationship between the observed fringe displacements (A) and the calculated speed (v): $A=2L(v/c)^2$, where L is the optical path length of the interferometer, and c is the speed of light.

[22] Our final conclusions (reference 34) were different!

[23] R. T. Birge, *Reports on Progress in Physics* (The Physical Society, London, 1941), Vol. VIII, pp. 100–101.

of Synge.[24] When he said no, I told him of our correspondence and gave an account of my understanding of Synge's theory relating to the concept of a rigid body in relativity and its possible bearing on Miller's results in the Michelson–Morley experiment. He felt very strongly that Synge's approach could not be significant and that the results of any experiments of the kind proposed by Synge would be irrelevant to questions regarding relativity, including the relativistic concept of a rigid body. When I told him that Synge predicted a small positive effect due to the acceleration of the interferometer, Einstein asked, "What acceleration, the rotation of the instrument?" When I told him that I understood it was the acceleration due to the rotation of the earth on its axis through the possible coupling with the interferometer through the "pin" (assumed by Synge), he shook his head vigorously and replied, "This can have no connection. All such accelerations, including any due to Coriolis forces would be completely indistinguishable from gravity." Einstein stated strongly that he felt Synge's approach could have no significance. He felt that even if Synge devised an experiment[25] and found a positive result, it would be completely irrelevant. He was sure that all questions relating to the coupling of the "pin" and rigidity of the apparatus were meaningless in relativity. He emphasized, however, that the question of "rigidity" was most important and needed study. He talked at some length about the problem of rigidity in relativity, emphasizing its importance for such questions as the finite speed of propagation of signals, etc. However, he told me that no significant definition or theory of rigidity existed which corresponded to "reality," since the only rigid bodies yet studied were either those having zero mass or whose behavior could be treated in the absence of all external forces.

He then referred to the fact that I had previously told him that "Miller's positive result obtained at Mount Wilson could be explained by his method of analysis, and he felt sure that this was a more likely explanation than Synge's. Once again he told me that Lorentz could never explain Miller's result and felt that it could not be ignored, although Einstein was not sure whether Lorentz really believed Miller's result.

I asked Einstein if Synge was justified in attacking problems regarding acceleration by special relativity, and he said, "Oh yes, that is all right as long as gravity does not enter; in all other cases, special relativity is applicable. Although, perhaps the general relativity approach might be better, it is not necessary."

I then asked him about the speech he gave in Berlin on Michelson's death in 1931 and asked especially about his reference to the Michelson–Morley experiment and its relation to general relativity. He had apparently forgotten the speech, much less what he had said, so this came to naught. However, his eyes shone as he thought about Michelson and once more he referred to him as "an artist." When I told him that Michelson's daughter (Mrs. Dorothy Michelson Stevens) had told me of Einstein's visit to her parents home in California, he smiled broadly and seemed to recall it with great pleasure. He was very complimentary about the Michelson–Gale experiment but said that at first he did not understand how it worked.

I referred again to Synge's proposed experiment—contrasting the possible difference between Miller's and Joos'[26] apparatus regarding "pin" etc.,—but he just shook his head. Apparently he had completely forgotten Joos' work.

I next asked Einstein about his early interest in de Broglie's work. He told me that in his studies of the degeneracy of gases he had worked out a theory for the statistical fluctuations from the entropy which contained "an undulatory term," which he identified as bearing a close relationship to de Broglie's ideas on matter waves.[27]

He went on to say that in quantum theory he was "in the opposition" because he felt that the ψ functions do not represent reality. He described quantum mechanics as a brilliant "short-cut" which successfully avoided many of the difficulties and the hard work which the final correct theory must face and solve. He talked at length

[24] J. L. Synge and G. H. F. Gardner, Nature **170**, 243 (1952); also Proc. Roy. Dublin Soc. **26**, 45 (1952).

[25] Such an experiment devised by Ditchburn did in fact give again a null result for the aether-drift: R. W. Ditchburn and O. S. Heavens, Nature **170**, 705 (1952).

[26] G. Joos, Ann. Physik **7**, 385 (1930); Naturwissenschaften **38**, 784 (1931).

[27] A. Einstein, Berliner Berichte, p. 261 (1924); p. 3 (1925).

about the ψ function description especially as related to a wave-packet localizing an electron to the extent permitted by the Uncertainty Principle. (He did not call it by this name nor did he mention Heisenberg.) Descriptions of the position, velocity, spreading with time of the ψ function, etc., he did not like at all. He emphasized that quantum theory only admits defining the position of a particle exactly by an act of observation which completely changes it. He readily admitted, however, that quantum theory provided the only way at present known for describing quantum (stationary) states. He felt that the ψ description was not "reality" and that the quantum-theory people all have a "narrow view" (holding his hands to his eyes to show me). In arguing that the quantum view was not complete, he emphasized that it had no explanation for the constancy of the elementary electrical charge. He added, however, that "it is correct to use quantum theory as long as it is useful, even though it is not a complete description." He told me that J. R. Oppenheimer was sure that quantum theory provided a complete solution and description but added, "I have talked very little to him about it." Einstein felt sure that the final correct theory must start with general relativity (although he said that his own attempts in this direction were probably wrong).

The difficulties in quantum theory are becoming very acute in nuclear theory which Einstein considered hopeless in its present state. He felt that just the multiplication of facts and experimental data in nuclear physics would not clarify the situation or lead to a final correct theory. This is in marked contrast to the prevalent view that experimental facts will utimately reveal regularities and thus give the hints that will lead to a theoretical solution. He disagreed completely with this view and emphasized again that even in atomic problems the quantum theory description is unsatisfactory. We talked about the big machines used in nuclear physics, and he feared a real danger from these for the scientific method. They make us "slaves to the means," and new ideas would be lost or never found.[28]

I asked if he had seen Dirac's article in *Nature*[29] on the aether, and he said no and asked me about it. I told him a little, and when I mentioned the relationship of the Uncertainty Principle to the impossibility of defining a unique reference direction in space, he said, "I do not like it!" He continued to say that if one needed properties that are in space before matter, the field equations, etc., are introduced, then you would need an *aether* but that this need does not exist. He went on to explain why "Maxwell's equations are not reality," but I did not follow this.

He then recalled Synge and his work and asked me if he were not somewhat of a philosopher. He again said that more experiments were not necessary, and results such as Synge might find would be "irrelevant." He told me not to do any experiments of this kind.

As I left Einstein, I realized that he seemed much older. His eyes still had the deep smile but were not as keen as before. His hands were a little feeble, and when I asked him if he still played the violin (a Brahms' score was on his desk) he said, "No, my fingers don't work any more." I thanked him for his time, but he waved his hands in protest. Then we shook hands, he smiled, and I left.

IV. 24 October 1952

It was a beautiful October day, and I walked down Mercer Street to Professor Einstein's home which is number 112. Einstein's secretary greeted me and led me upstairs and to the rear of the house to Professor Einstein's study. This was an attractive simple room with open white bookshelves on two sides filled in rather a disordered fashion with books, reprints, and papers. Towards the back of the room large windows look out on a yard where today the trees are in their last autumn foliage. I was struck at once by a fine portrait of Maxwell on the wall and by another of Faraday. Professor Einstein was sitting on the adjacent porch when I entered, but he immediately came into the study and greeted me in a very friendly way. He asked me to be seated by his desk, and I then told him of the plans at Case to celebrate the 100th birthday of Michelson at which time I was to speak on his work and especially about the interferometer experiments. He smiled broadly and expressed real satisfaction that we were doing this. He said, "I always think of Michelson as the artist in

[28] One must remember that this was 10 years ago!
[29] P. A. M. Dirac, Nature **168**, 906 (1951).

science. His greatest joy seemed to come from the beauty of the experiment itself and the elegance of the method employed. He never considered himself a strict 'professional' in science and in fact was not—but always the artist."

I asked Professor Einstein where he had first heard of Michelson and his experiment. He replied, "This is not so easy, I am not sure when I first heard of the Michelson experiment. I was not conscious that it had influenced me directly during the seven years that relativity had been my life. I guess I just took if for granted that it was true." However, Einstein said that in the years 1905–1909, he thought a great deal about Michelson's result, in his discussions with Lorentz and others in his thinking about general relativity. He then realized (so he told me) that he had also been conscious of Michelson's result before 1905 partly through his reading of the papers of Lorentz and more because he had simply assumed this result of Michelson to be true.

I told Einstein of my father's report as a student at Western Reserve University that the Michelson–Morley experiment was considered to have been a failure and that Morley was in a certain sense an object of pity. He shook his head vigorously at this and said, "No one should have said that! Many negative results are not highly important, but the Michelson experiment gave a truly great result which everyone should understand."

Professor Einstein felt that the work of Lorentz should be studied much more by present-day physicists. He said that the Maxwell electromagnetic theory had not been left in good form and really only applied to a vacuum. This was made clear by the work of Lorentz who showed that electric field and displacement must always be related by the properties of the medium, and so theory must assume or find these properties before it can proceed. Lorentz's contributions to this subject were, in Einstein's opinion, his great achievement. His proof that essentially there are not four fields but rather two is an achievement of the greatest historical importance.

Professor Einstein told me that he felt that the history of the development of ideas in science is neglected. He was not interested in the history of data—when, who did this, etc.,—but in the tracing of the evolution of ideas. "Most scientists today do not seem to realize that the present position of science can have no lasting significance." I asked him if it was not probable that the next great advances in physics would be farther in the future than most "planners" would admit, and he laughed and said, "Yes, yes they are all trying to get their results too cheap!"

Einstein then told me again how beautiful he considered "Michelson's rotation of the earth experiment." (Michelson–Gale experiment). He considered this as one of the most beautiful of all the experiments in physics and after the Michelson–Morley experiment, he considered it as Michelson's greatest achievement. As Einstein said, "Michelson could not stop and reverse the rotation of the earth, so he accomplished the same result by using a big path and a little path. (Gestures). It is really not self-evident how this could give the same result as stopping the earth." (Here Einstein smiled with real joy.)

Einstein also mentioned the earth-tide experiment[30] which he also liked very much. "This experiment was out of his (Michelson's) line, but an optical principle suggested itself to him and made the measurement succeed."

I asked Einstein if he had any explanation for the fact that Michelson first repeated the Fizeau moving water experiment at Case before taking up the repetition of his own Potsdam experiment. Einstein said that it seemed very natural to him that Michelson would take up the Fizeau experiment and that this simply proved, "that the whole problem was in Michelson's mind and that he was thinking deeply on all aspects of it. The Fizeau experiment and result was so fundamental that an improved repetition of it would in any case be highly desirable." I asked Einstein if he felt there had been any serious doubt at the time about Fizeau's result, and he replied, "Oh, every experiment should be repeated and refined whenever possible." Several times he said, "I really loved Michelson."

I told Einstein that in Michelson's lectures on optics to his students at Case he had not mentioned the results of his own Potsdam experiment or even described his own form of interferometer. Einstein thought this was natural, as a teacher

[30] A. A. Michelson and H. G. Gale, J. Geol. **27**, 595 (1919).

should tell his students only the completely established facts. He added that if he were to give lectures on theoretical physics, he would omit all reference to his own more speculative work and would reserve this for listeners who were more of the type of "connoisseur," and who would realize that many of the points were still not finally settled.

I asked Professor Einstein about the three famous 1905 papers[31] and how they all appeared to come at once. He told me that the work on special relativity "had been his life for over seven years and that this was the main thing." However, he quickly added that the photoelectric effect (he could not for a moment recall the English word) paper was also the result of five years pondering and attempts to explain Planck's quantum in more specific terms. He gave me the distinct impression that the work on the Brownian movement was a much easier job. "A simple way to explain this came to me, and I sent it off."[32]

I mentioned that the parts of the Michelson–Morley apparatus are lost and that even the exact place where the experiment was performed is not entirely certain. To this he smiled and shrugged and reminded me that physicists do not collect and preserve in the sense of old book collectors but that the ideas are the things of permanent value.

Einstein told me that Michelson did not like the relativity theory. He told Einstein this and he also heard it from others. Einstein laughed and added, "You know we were very good friends!" Michelson said to Einstein that he was a little sorry that his own work had started this "monster." Einstein then went on to tell me how natural this attitude would be in Michelson and that it was due simply to the fact that Michelson's love was to experience phenomena directly and that as a result he did not like abstractions. I commented that in a certain sense this was similar to Goethe's attitude toward color theory. To this Einstein replied, "Oh Michelson's attitude was not that extreme or he would not have been a physicist!" He also added that for Goethe every observation of Nature was a deep and direct personal psychological experience and that he would not permit the abstractions of science to interfere. Much of Goethe's description of color is very useful, and up to the point where he simply attacked Newton blindly, he made real contributions to the subject of colors, especially for art.[33]

Einstein talked to me a little about quantum theory and added, "Here I am a heretic you know (laughed), but someday I believe my views will be found true. God did not invent the Science of Probability you know!" He spoke very highly of Bohm's work but was sure it is wrong. "He got his results too cheap." Einstein told me that Bohm was not reappointed because he refused to testify against other people. Einstein spoke very bitterly about the "dirty work" of the Rosenbergs' relative who saved his life with theirs. About publicity Einstein told me that he had been *given* a publicity value which he did not *earn*. Since he had it he would use it if it would do good; otherwise not. He looked mostly at me during the conversation, but occasionally his eyes looked far out into the autumn leaves. As he thought his great eyebrows and the top of his head would move, and then his eyes would brighten again to tell me something more.

It was now time for me to leave, and I thanked him and will treasure the memory of him sitting with a blanket over his knees, his great deep smile, his boyish laugh and the sincere friendliness of that heart and mind.

He came to the door of his study as I left. He shook my hand and smiled and thanked me for coming. Miss Dukas was standing in the hall as I came out. I hope I did not tire him.

V. 11 December 1954

Arrived in Princeton on the 10:45 train from New York and called Einstein's secretary from the Princeton Inn. She told me that Professor Einstein wanted to see me but that the doctor was there and would I be willing to come in a half-hour. I, of course, agreed and arrived at 112 Mercer Street about 11:30. I left my coat and briefcase in the lower hall and Miss Dukas led me upstairs and to the back of the house into

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[31] A. Einstein, Ann. Physik **17**, 132, 549, 891 (1905).

[32] Of course Einstein worked on problems of the Brownian Movement a great deal. See his collection of papers edited by R. Fürth (Methuen and Company, Ltd., London, 1926).

[33] J. W. von Goethe, *Nachtrage zur Farbenlehre* (1810).

Professor Einstein's study. He arose from his chair, shook hands and greeted me in a very friendly manner. He began our conversation by telling me that he found our arguments convincing and "a very fine paper."[34] He then "told me a story." At a meeting at the Institute for Advanced Study several years ago, they discussed Miller's results and Einstein had commented, "God is hard on us, but He is not malicious![35] Surely there is an explanation for Miller's results." (Much laughter by Professor Einstein.) Einstein told me again that he was sure we had found this, and that everyone was pleased. I mentioned that Professor Dyson had made a valuable suggestion which we had used to improve our correlations with temperature conditions. Einstein asked me to tell him about Dyson's suggestion, and I discussed the $\overline{\Delta T} = \mathrm{Av}|\bar{T} - T_i|$, $(i=1,2,3,4)$. Einstein thought this a good suggestion and asked how it worked out. When I told him rather well in all but one case he replied, "You would expect that!" I told him that Professor Miller had given me his data and that I was pleased that we could carry out his wishes and look into it a little more. He again told me that he thought we had settled the problem at last. He then told me that he felt making the apparatus large was a mistake. "You make your troubles bigger and gain so little." He also asked me why no one had used vacuum in the light paths to get away from temperature effects. I told him I did not know but that Joos[26] had gone pretty far and that by using helium, Kennedy[36] had accomplished much the same result.

When I began to tell him why I had felt it necessary to get into the "frame of mind" that suggested going to Mount Wilson, he emphatically agreed. "You could not properly comprehend the work unless you did this." However, he added that the idea that a bigger "aether wind" would exist on a mountain had always been impossible to bring into a picture with the facts of astronomical aberration.

We then chatted briefly about Michelson, and he again told me that he considered him a "wonderful man." "Of course," Einstein added, "He told me more than once that he did not like the theories that had followed from his work!" (Much laughter by Professor Einstein.)

We looked out of his windows to the bare trees of the December land, and then I felt I had stayed long enough. I asked him not to rise as I took my leave, but of course he did. I paused to look at the pictures on his study wall of Faraday and Maxwell. He spoke highly of each and said he especially liked the picture of Faraday. I told him I had his latest book and had enjoyed reading in it, especially the last tribute to Lorentz (for his centenary).[37] He became most serious and said, "People do not realize how great was the influence of Lorentz on the development of physics. We cannot imagine how it would have gone had not Lorentz made so many great contributions."

We shook hands, and I started down the hall, and he followed, telling me (only now!) that he was not too well and had a "strong anemia." At the top of the stairs we again shook hands, and he smiled with that deep and humble expression with which he had first greeted me, gripped my hand, then raised his hand in a sort of salute and said, "Goodbye."

ACKNOWLEDGMENTS

I am greatly indebted to my colleagues, Professors Sidney W. McCuskey, Leslie L. Foldy, and Martin J. Klein for advice in the preparation of this paper.

[34] This was the final draft of the paper subsequently published by, R. S. Shankland, S. W. McCuskey, F. C. Leone, and G. Kuerti, Revs. Modern Phys. **27**, 167 (1955).

[35] Over a fireplace in Princeton's Fine Hall are almost these same words of Einstein: "Raffiniert ist der Herr Gott, aber boshaft ist er nicht."

[36] R. J. Kennedy, Proc. Natl. Acad. Sci. U. S. **12**, 621 (1926); Astrophys. J. **68**, 367 (1928).

[37] A. Einstein, *Ideas and Opinions* (Crown Publishing Company, New York, 1954), pp. 70–76. See also G. L. deHaas-Lorentz, *H. A. Lorentz* (North-Holland Publishing Company, Amsterdam, 1957), pp. 5–9.

Conversations with Albert Einstein. II*

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Professor Einstein's views on the experimental basis for relativity theory and his attitude toward quantum mechanics continue to be of general interest. This paper is based on notes on these subjects made by the writer after each of five visits with Einstein at Princeton during 1950–1954 and not fully reported or discussed previously.

In 1963 I reported on five visits with Prof. Einstein in Princeton during the period 1950–1954.[1] These discussions were published almost verbatim with but little comment by me. They have since been referred to in several articles on the history of physics, so it now seems appropriate to supplement the first publication by a more complete discussion of certain statements made to me by Prof. Einstein. I would like especially to record my impressions of Einstein's views on the role of the experiments of Michelson and Morley and others in the development of the theory of relativity and also to comment more fully on his attitude on quantum mechanics.

THE MICHELSON–MORLEY EXPERIMENTS

Professor Einstein's statements on the Michelson–Morley experiment and its influence on his work in the special theory of relativity were made on several occasions and are not entirely consistent; this is not too surprising in view of the fifty year interval involved. As this experiment was performed in Cleveland and had played a central role in the contributions of H. A. Lorentz, Henri Poincaré, and others, it was only natural to ask Einstein what relation it had had to his own work. During our discussions he always expressed the highest admiration for Lorentz ("the great Lorentz," as he said several times) and for his publications during the decade prior to 1905. It did not occur to me to ask Prof. Einstein about Poincaré and his influence on his work, but I have since learned from Miss Helen Dukas, Prof. Einstein's secretary, that they had very little correspondence. In the Einstein archives at Princeton there is a letter of recommendation[2] which Poincaré wrote for Einstein in 1911; however, it does not specifically mention Einstein's contributions to the special theory of relativity. I have regretted that I did not ask Prof. Einstein about Poincaré for he would certainly have given me detailed answers to any questions and made clear the relationship of his own work to that of Poincaré, as he did with Lorentz. It is well established that during Einstein's years in the Swiss Patent Office at Berne (1902–1909) he was the leader in an evening study group (including Maurice Solovine and Conrad Habicht) called "Olympia," which was active from 1902 through 1905. Books which they studied in detail included Ernest Mach's *Science of Mechanics*, Dedekind's work on *Numbers*, and Poincaré's *La Science et L'Hypothèse.*[2] The latter work discusses the Michelson–Morley experiment and includes Poincaré's thoughts on the principle of relativity, both for uniform motion and accelerated systems. It thus seems clear that Einstein, who had already published several original research papers in the *Annalen der Physik* in the period 1900–1905, was acquainted with some of Poincaré's contributions to the principle of relativity, as he was with the writings of Lorentz.[3]

Professor Einstein told me several times during our talks in Princeton that he had learned of the Michelson–Morley experiment through the writings of H. A. Lorentz. It was my understanding during our talks that Prof. Einstein was well aware of the null results of a wide range of both first order (v/c) and second order $(v/c)^2$ aether-drift experiments conducted throughout the 19th century. This is clearly substantiated by the following statement in his famous 1905 paper[4]: "... *the unsuccessful attempts* to discover any motion of the earth relative to the 'light medium,' suggest that the phenomena of electrodynamics as well as mechanics possess no properties corresponding to the idea of absolute rest. They suggest rather that *as has already been shown to the first order of small quantities*, the same laws of electrodynamics and optics will be valid for all frames of reference for which the equations of mechanics hold good ...." The *unsuccessful attempts* clearly refer to the $(v/c)^2$ experiments; among these, the Michelson–Morley experiment was the most definitive.

It is unlikely that he had studied the details of the technique of the various experiments, but he probably knew more about these than might be assumed. I found his interest in and knowledge of the essential features of the apparatus used by Michelson and Morley, Fizeau, Zeeman, Sagnac, Harress, Tomaschek, and Michelson and Gale to be much more than I had anticipated would be the case.

The several statements which Einstein made to me in Princeton concerning the Michelson–Morley experiment are not entirely consistent, as mentioned above and in my earlier publication. His statements and attitudes towards the Michelson–Morley experiment underwent a progressive change during the course of our several conversations. I wrote down within a few minutes after each meeting exactly what I recalled that he had said. On 4 February 1950 he said, "... that he had become aware of it through the writings of H. A. Lorentz, but only after 1905 had it come to his attention." But at a later meeting on 24 October, 1952 he said, "I am not sure when I first heard of the Michelson experiment. I was not conscious that it had influenced me directly during the seven years that relativity had been my life. I guess I just took it for granted that it was true." However, in the years 1905–1909 (he told me) he thought a great deal about Michelson's result in his discussions with Lorentz and others, and *then he realized* (so he told me) that he "had been conscious of Michelson's result *before 1905* partly through his reading of the papers of Lorentz and more because he had simply assumed this result of Michelson to be true." Since there are these conflicting statements it may be of interest to comment further on this situation.

When I first called on Professor Einstein in Princeton on 4 February 1950, he was unacquainted with me and may possibly have wondered if I had not come as the successor of Professor Dayton C. Miller at Case to talk about Miller's "aether drift" experiments at Mount Wilson. These were certainly on my mind, but at that first meeting my primary objective was to ask him about the two Michelson–Morley experiments performed at Cleveland in 1886 and 1887. But before my views became clear to him, Prof. Einstein volunteered a rather strong statement that he had been more influenced by the Fizeau experiment on the effect of moving water on the speed of light, and by astronomical aberration, especially Airy's observations with a water filled telescope, than by the Michelson–Morley experiment. This keen interest in the Fresnel-drag of moving water had been shared by many workers on the "aether problem." For example, at the "Baltimore Lectures" of Lord Kelvin (then Sir William Thomson) in 1884, both he and Lord Rayleigh, who was present, urged Michelson and Morley to repeat the Fizeau experiment as an essential preliminary to their more famous Michelson–Morley experiment. Their interest was to clarify the effect of the postulated Stokes drag of the aether by the earth and the resulting decrease in the expected velocity of the aether through the interferometer in the Michelson–Morley experiment. However, the close relation of the Fizeau experiment to the relativistic velocity addition theorem was the prime reason for Einstein's interest in this subject.

As clearly reported by Max Wertheimer,[5] who in 1916 discussed with Einstein the development of his ideas in special relativity in great detail, it is evident that the importance of the Michelson–Morley experiment for Einstein was that it gave positive confirmation to his belief that the speed of light is invariant in all inertial frames, independent of the motion of source, apparatus, or observer. Such invariance in c was necessary for his interpretation of Maxwell's equations and for his derivation of the Lorentz transformation, as well as for his conviction that the "local time," first introduced by Lorentz, is indeed the only true time for the description of physical phenomena. Professor Einstein's statement to me was that "at last it came to me that [absolute] time was suspect," and that the new absolute for physics must be the speed of light in vacuum, rather than space and time. These conclusions, and also the relativity principle for uniform motion to the greater sensitivity $(v/c)^2$ than had been shown by any previous "aether drift" experiments, were strongly supported by the Michelson–Morley experiment, which showed no change in the speed of light with respect to direction and revealed no absolute motion of the interferometer through space. It is of interest to note that even in 1950 Einstein was concerned with the possible periodic changes in c which had recently been reported, and asked if I thought the effects were real. When I told him I believed they were spurious, he seemed obviously pleased. The fact that he was so interested in these measurements in 1950 makes it very difficult to believe that he was unaware of any important work bearing on the problem in the years before 1905.

When Prof. Einstein realized that I had not come as an advocate for Miller's results his attitude became less formal and our conversations were much more relaxed. Indeed, it was largely his encouragement and advice that made me feel it would be desirable for us to make the study which finally revealed the cause of Miller's periodic fringe shifts. I should add that in doing this we were also carrying out the wishes of Prof. Miller who died without having resolved the question and who had suggested that we restudy his data if it were thought desirable. Professor Einstein's statements to me about Prof. Miller and his experiments were uniformly respectful, though it was clear that he felt sure they were in error. He also told me that H. A. Lorentz had tried without success to explain Miller's results.

In this connection it is of interest to note that in 1921 when Miller first announced the periodic fringe shifts in his interferometer experiments on Mount Wilson, Einstein was lecturing on relativity at Princeton. He told me that it was Miller's work that had prompted his remark now inscribed in the stone fireplace at Fine Library, "*Raffiniert ist der Herr Gott, aber boshaft ist er nicht.*" When he learned of Miller's result he travelled to Cleveland to see him and they had a long discussion about the Mount Wilson experiments. Professor Einstein told me that at that time he did not feel sure of a single English word,[1] but Prof. Miller could speak German rather fluently, for as a youth in Berea, Ohio, his playmates were the children of German Methodist ministers teaching at Wallace College (now a part of Baldwin–Wallace College). The writer has a sketch made by Prof. Einstein during this visit on 25 April 1921 indicating how the postulated Stokes drag of the "aether" by the earth would become progressively smaller with elevation above sea level so that on Mount Wilson the expected fringe shifts in the interferometer should be considerably greater than in the basement laboratory at Cleveland. Of special interest on the sheet with Einstein's sketch is the word "Fizeau" written by Miller, probably because Einstein had discussed the Fizeau experiment and its greatly improved repetition by Michelson and Morley at Case in 1886. This notation again emphasizes the importance that Einstein attached to this experiment and proves that even in 1921 he considered the phenomenon of the effect of moving water on the speed of light as a major observational fact for his work on special relativity. By assuming an invariant upper limit for the speed of light in vacuum, the derivation of the relativistic velocity addition theorem, which is closely related to the Fresnel drag, follows directly. The word "Fizeau" written on Einstein's sketch may also indicate that as early as 1921 when Miller's positive result seemed definite, Einstein may have been looking elsewhere than to the Michelson–Morley experiment for experimental support for the postulates of relativity. It also could mean, as he told me in 1952,[1] that he had suggested to Miller that the Fresnel drag by the Mount Wilson interferometer itself would greatly reduce the expected fringe displacements.

When we[6] finally found the cause of Miller's periodic fringe shifts to be temperature gradients across the interferometer, Einstein was genuinely pleased and, in fact, wrote me a fine letter on the subject. I only learned after his death that he had written to Ehrenfest after Miller's 1921 announcement, suggesting that temperature effects might be responsible for the results; it is curious that he never mentioned this to me. Whether he had forgotten about it or whether he wanted us to find the solution in our own way, I cannot say;

but of his genuine interest in finding the correct answer there can be no doubt.

It is important to note that in talking with M. Wertheimer in 1916, Einstein's references to experiments were amost exclusively to the better known Michelson–Morley experiment.[5] This was five years before Miller's first experiments. This writer finds the account of Wertheimer entirely consistent with his own notes and recollections of Einstein's attitude in 1952 toward the Michelson–Morley experiment.

The writer is convinced on the basis of his discussion with Prof. Einstein that he certainly knew about the Michelson–Morley result before 1905 and also was conversant with a wide range of both the experimental and theoretical work in the physics literature bearing on the "aether problem" before 1905. However, the exact relation which any specific experiment or the theoretical work of other physicists bore to his own creations is not possible to determine with certainty; even Einstein himself was not sure. But to believe that he could be unaware of important experiments or theoretical developments in the decade before 1905 is entirely inconsistent with his profound understanding of the development of physics, as was clearly evident even in his later years.

It should be noted that in 1905 it was not the practice to give specific references in published papers as it is today. Many important papers gave no references whatever, so the fact that Einstein's 1905 paper (which has no references) makes no explicit mention of the Michelson–Morley experiment is not in the least unusual. To what degree and at what stage of his activities he was directly influenced by the work of others, it is now impossible to determine. But I became convinced that his interest in the Michelson–Morley experiments (both those of 1886 and 1887) had existed before 1905.

EINSTEIN'S ATTITUDE TOWARD QUANTUM MECHANICS

One of the great puzzles in physics was the attitude of Prof. Einstein toward the new quantum mechanics developed during the years after 1925. His position was strange not only because of his great contributions to physics in general, but especially so in view of his leading role in the development of the old quantum theory from 1905 to 1925, when his photoelectric theory, photon hypothesis, the concept of induced transitions, the first quantum theory of the specific heat of solids, and the Bose–Einstein condensation and statistics were all major achievements leading to the new quantum mechanics. However, when it became clear that the new theory had essential statistical elements, then Einstein turned completely against it and for the remainder of his life was an active opponent.

There is, of course, no way to determine with certainty why this change of heart took place. But we can recall that during the years 1919–1925 following Eddington's successful eclipse confirmation of the deflection of starlight by the sun, Einstein suddenly became a world figure and began lecturing and working for political causes that required a significant fraction of his time and energy. It was in this period that the new quantum mechanics was developed by Bohr, Heisenberg, Pauli, Dirac, Schrödinger, Born, and others, without Einstein having a major role. Nevertheless, during this period he had maintained an active interest. He was one of the first to hail the discovery of the Compton effect; here his opposition to probability theories first strongly appeared when he disavowed the statistical theory of Bohr, Kramers, and Slater[7] as a valid description of this scattering phenomenon. Even when the Compton effect was shown to give a clear example of the Heisenberg uncertainty principle, Einstein was not won over, although enthusiastic for its support of the photon theory of radiation. But even his earlier use of statistical methods to predict the existence of photons traveling with energy $h\nu$ did not affect his attitude toward the new quantum mechanics and especially its statistical features toward which he remained consistently hostile.

This greatly influenced his relations with Niels Bohr and the "Copenhagen School." Although Bohr and Einstein were uniformly courteous at their meetings and in correspondence, they both at times became highly emotional in their views on the basic correctness and completeness of the quantum-mechanical description of nature. Einstein made statements to the writer which were very strong in his criticisms of Bohr, Heisenberg, Dirac, Oppenheimer, and others who shared their views. Even Dirac's relativistic development of quantum theory failed to alter Einstein's opinion in statements he made to me. We also know from friends who studied in Copenhagen that Prof. Bohr, in spite of his extremely polite and detailed refutations of the various quantum paradoxes proposed by Einstein, was often very emotional on this subject. Bohr even became highly critical of Bohm's work on "hidden variables," and considered his attempt for a deterministic picture to be philosophically a step backward.

Recent experiments at Berkeley by E. D. Commins and his research students[8] have shown that the correlation in linear polarization between two photons emitted in an atomic cascade is in complete agreement with quantum mechanics and in direct conflict with all "hidden variable" theories which have been proposed to give an underlying deterministic structure such as Einstein held to be essential for a complete theory.[9]

The discussions between Einstein and Bohr at Solvay Conferences and elsewhere of the paradoxes by which Einstein hoped to reveal basic defects in quantum mechanics are highly interesting and clarify many subtle points. But it is clearly evident that time and again Bohr successfully defended the quantum mechanical description of nature against Einstein's criticisms. One of the most dramatic of these exchanges is that where Bohr used the general theory of relativity to explain a paradox proposed by Einstein at their last Solvay Conference meeting in 1930.[10]

The question has often been asked: How would Einstein's attitude toward the unified field theory have been affected by the problem of including the weak and strong nuclear interactions; and especially how would he have reacted to the failure of parity and other invariance principles. He, of course, did not discuss this, but it seems to me that since his basic philosophy was always directed toward the unification of physics, he would have maintained his faith that an all embracing unified field theory would ultimately be found. He did express the strong conviction to me that the present lack of success is due to approximation methods that fail to reveal true insights into phenomena, that only when great advances in mathematics make rigorous solutions possible will significant progress be made in theoretical physics, and that "the final solution must start with general relativity."

It was clear that Prof. Einstein (in his statements to me) regarded quantum mechanics as a valid computational tool, but he could never accept the philosophical implications of its statistical basis or believe it was a complete description of nature. He felt sure that eventually a foundation would be found for quantum mechanics that would restore causality and eliminate the uncertainty principle and the statistical description and provide a complete theory. This viewpoint emphasizes the fact that Einstein was at heart a classical physicist. His relativity theories were the capstone of the classical physics of the 19th century, and the complete break with this structure made by the new quantum mechanics appeared to be too much for Einstein to accept.

Today, the majority of physicists have not been greatly influenced by Einstein's attitude toward the new quantum mechanics, but there must nevertheless be a lingering question that some elements of his position may yet prevail, eventually to influence future developments. However, certain points are clear. In the development of physics which led to the theory of relativity, there had gradually accumulated basic paradoxes and conflicts between theory and experiment that were only finally resolved by the theory of relativity. These included the null result of the Michelson–Morley experiment, the Fizeau and Michelson and Morley measurements of the Fresnel drag on light in moving water, an apparent favored inertial frame for Maxwell's equations, certain asymmetries in electrodynamics that did not seem inherent in the phenomena, etc. These finally pointed the way to new concepts of space and time in physics, leading at first to the special theory of relativity and then to the general theory of relativity and gravitation.

Other paradoxes in physics were finally resolved only by the development of the new quantum mechanics. Chief among these were the photoelectric effect, the Compton effect, phenomena that required wave or particle properties for radiation, electron diffraction and the wave nature of matter, the anomalous Zeeman effect, the Stern–Gerlach experiment, the existence of discrete energy states and transitions in atomic and molecular systems, and others. These paradoxes and contradictions were only removed with the advent of quantum mechanics.

Today the situation is entirely different. At present there seem to be no basic paradoxes or conflicts between theory and experiment of the nature that suggest that quantum mechanics is wrong. To be sure, there are great difficulties, but these can be viewed as largely computational in nature and not related to the basic validity of quantum mechanics itself. The development of more powerful forms of mathematics may enable many of these problems ultimately to be solved. The present difficulties in the application of quantum mechanics chiefly involve such factors as the lack of detailed and certain knowledge of the nuclear force and things of that nature and are not inherent in quantum mechanics. Even the failure of invariance principles, such as "C," "P," and "CP" in no way affect the validity of quantum mechanics but merely affect the forms of the Hamiltonian that can be employed in the solution of specific problems.

Einstein's attitude toward quantum mechanics was well known but for the record we give some of the statements he made to me during my visits in Princeton. These are listed here out of context but they serve to show the depth of feeling for the convictions he held. In making these statements to me, Prof. Einstein often exhibited great emotion and there was certainly no question that his opposition to quantum mechanics was very intense.

> Atomistic phenomena must fit into a unified field theory.
>
> You know I am in disagreement with most of my colleagues on the quantum theory.
>
> They are not facing the facts!
>
> They have abandoned reason! [with great emphasis!]
>
> Quantum mechanical physics avoids reality and reason!
>
> Bohr's thinking is clear but when he begins to write he becomes very obscure and he thinks of himself as a prophet.
>
> Bohr always speaks *ex cathedra*.
>
> On Quantum Theory I am in the opposition.
>
> The ψ functions do not represent reality.
>
> Quantum mechanics is a brilliant shortcut which successfully avoided many of the difficulties and the hard work which the final correct theory must face and solve.
>
> Bohm got his results too cheap.
>
> I have talked very little with Oppenheimer about it.
>
> The final correct solution must start with general relativity.
>
> Here in quantum mechanics I am a heretic, you know [laughed] but someday I believe my views will be found true. God did not invent the science of probability, you know.

In closing this account, I would like to express my admiration for Professor Einstein in words of one of the greatest poets[11]:

> And now his work is done. It will endure,
> We trust, beyond Jove's anger, fire and sword,
> Beyond Time's hunger. It will be borne
> Above the stars, and shall be living, always.

* This account was given during 1972–1973 for physics colloquia at Purdue University; Wilmington, Delaware R.E.S.A.; Rensselaer Polytechnic Institute; and Indiana University. It is a pleasure to acknowledge my thanks for the helpful discussions at these meetings, and also with my colleagues at Case Western Reserve University and Prof. Luis W. Alvarez.

[1] R. S. Shankland, Amer. J. of Phys. **31**, 47 (1963).

[2] See *Albert Einstein* by Carl Seelig for Poincaré's letter in French on p. 163 in the original 1954 edition; in the English edition, Savill translation, pp 134–135 (Staples, London, 1959). See also p. 57 of the English edition for "Olympia" reading list.

[3] For an excellent, balanced, and detailed discussion of the contributions of Lorentz, Poincaré, Einstein, and Minkowski to the development of the special theory of relativity, see G. H. Keswani, British J. Phil. Sci. **15**, 286 (1965); **16**, 19, 273 (1965–66). See also, "Poincaré's Rendiconti Paper on Relativity" by H. M. Schwartz, Amer. J. Phys. **39**, 1287 (1971); **40**, 862 (1972); **40**, 1282 (1972).

[4] A. Einstein, Ann. der Physik **17**, 891 (1905). Italics are mine.

[5] M. Wertheimer, *Productive Thinking* (Harpers, New York, 1945, 1959), Chap. 7 on "Einstein: The Thinking that Led to the Theory of Relativity."

[6] R. S. Shankland, S. W. McCuskey, F. C. Leone, and G. Kuerti, Rev. Mod. Phys. **27**, 167 (1955).

[7] N. Bohr, H. A. Kramers, and J. C. Slater, Phil. Mag. **47**, 785 (1924).

[8] C. A. Kocher and E. D. Commins, Phys. Rev. Letters, **18**, 575 (1967); S. J. Freedman and J. F. Clauser, Phys. Rev. Letters **28**, 938 (1972).

[9] A. Einstein, B. Podolsky, and N. Rosen, Phys. Rev. **47**, 777 (1935).

[10] Niels Bohr in *Albert Einstein, Philosopher–Scientist*, edited by P. A. Schlipp, (Library of Living Philosophers, New York, 1949), pp. 224–228.

[11] Ovid, *Metamorphoses XV*, Epilogue.

LETTER TO THE EDITOR

Comment on "Conversations with Albert Einstein. II"

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Several readers of the article on "Conversations with Albert Einstein. II," published in the July 1973 issue of this Journal,[1] have expressed interest in the work of Poincaré, which was studied by Einstein and the Olympia Club in Bern prior to 1905. This letter gives the specific reference alluded to on p. 895 of Ref. 1.

The following statement appears on p. 201 of the 1903 edition of Poincaré's book *La Science et l'Hypothése*[2] (the italics are mine).

> Et maintenant il faut qu'on me permette une digression; je dois expliquer, en effet, pourquoi je ne crois pas, malgré Lorentz, que des observations plus précises puissent jamais mettre en évidence autre chose que les déplacements relatifs des corps matériels. On a fait des experiénces qui auraient dû déceler les termes du premier ordre; les résultats ont été negatifs; cela pouvait-il être par hasard? Personne ne l'a admis; on a cherché une explication générale, et Lorentz l'a trouvée; il a montré que les termes du premier ordre devaient se détruire, mais il n'en était pas de même de ceux du second. *Alors on a fait des expériences plus précises; elles ont aussi été négatives*[3]; ce ne pouvait non plus être l'effet du hasard; il fallait une explication; on l'a trouvée; on en trouve toujours; les hypothèses, c'est le fonds qui manque le moins.[4]

This statement clearly distinguishes between the v/c and v^2/c^2 "aether drift" experiments that were so much in the scientific literature of that day. While there is no explicit mention of the Michelson–Morley experiment by name, it seems to me that the words in italics (mine) clearly refer to their result. It was not then the usual practice to burden research papers with footnotes and references to well-known results, and the Michelson (1881) and the Michelson–Morley (1887) experiments had been discussed repeatedly in the literature from 1881 on into the 20th century. Often their result was referred to in a very general way as in Poincaré's book, although in his 5 June 1905 paper[5] Poincaré was explicit about Michelson (but not Morley).

Einstein, in his famous 1905[6] paper (which has no specific references), refers to both v/c and v^2/c^2 experiments in much the same way as Poincaré had done. Einstein's words are as follows (the italics, again, are mine).

> Beispiele ähnlicher Art, sowie *die misslungenen Versuche,* eine Bewegung der Erde relativ zum "Lichtmedium" zu Konstatieren, führen zu der Vermutung, dass dem Begriffe der absoluten Ruhe nicht nur in der Mechanik sondern auch in den Electrodynamik keine Eigenschaften der Erscheinungen entsprechen, sondern dass vielmehr für alle Koordinaten systeme, für welche die mechanischen Gleichungen gelten, auch die gleichen elektrodynamischen und optishen Gesetze gelten, *wie dies für die Grössen erster Ordnung bereits erwiesen ist.*

[1] R. S. Shankland, Am. J. Phys. **41**, 895 (1973).

[2] H. Poincaré, *La Science et l'Hypothèse* (Flammarion, Paris, 1903).

[3] This clearly refers to the v^2/c^2 experiments of Michelson and Morley and also, possibly, to those of Trouton and Noble [Philos. Trans. R. Soc. Lond. **202,** 165 (1903)] and of Lord Rayleigh [Philos. Mag. **4,** 215 (1902)].

[4] One detects in these last words Poincaré's coolness to the *ad hoc* nature of the original Fitzgerald–Lorentz contraction hypothesis. This was in 1903 before its later formulation by Lorentz in 1904.

[5] H. Poincaré, C. R. Acad. Sci. (Paris) **140,** 1504 (1905). In this paper Poincaré first named the *Lorentz transformation* and proved that it is a mathematical group and then extended the 1904 theory of Lorentz to obtain the transformations for electric charge and electric current density.

[6] A. Einstein, Ann. Phys. (Leipz.) **17,** 891 (1905).