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SYNOPSIS

This essay concerns an aspect of the speculative contributions of J. J. Thomson to a field of physics somewhat removed from that upon which his popular fame and scientific eminence were alike founded. He published a number of statements in the period 1903-1910 advocating a discontinuous structure of the electromagnetic field. His unorthodox conception of the field was based upon the presumed discreteness of Faraday's physical lines of electric force. While his ideas led to significant experimental work, they were not brought together in the form of a completed theory. It was at this same time that the quantum theory was independently evolving notions of a structure of the field, and Thomson's efforts at developing a theory of light were diverted into a protracted criticism of the hypothesis of quanta. In 1924-1936 he returned to the subject of the structure of light, but these latter speculations no longer had much relevance to contemporary physical thought.

1. INTRODUCTION

IN the early years of this century, J. J. Thomson, Cavendish Professor of Experimental Physics at Cambridge, put forward certain novel views concerning the nature of light. At the time, the wave theory of light, capped by Maxwell's interpretation of light as an interval in the electromagnetic spectrum, was seemingly one of the most secure achievements of nineteenth-century physics. The alternative Newtonian emission theory, which conceived of light as a gas of independent particles, was without a following. Yet it was only six years after Hertz's experiments confirming the existence of Maxwell's electromagnetic waves that Thomson proposed a modified version of the discredited emission theory. Challenging the widely held belief that Maxwell's theory demands a continuous, uniform distribution of energy over the surface of a wave front,[1] he argued for the existence of a discrete micro-structure underlying the continuous appearances of the radiation field.

At the turn of the century, experimentalists were uncovering varieties of discreteness in the products of radioactivity and in the radiations resulting from high-energy collisions. Thomson noted certain similarities between material radiations and energetic electromagnetic waves, and he was impressed by their potential meaning for an understanding of the physical nature of light.

As a laboratory researcher Thomson investigated the apparently universal manifestations of discreteness, while as a mathematical physicist he inherited the older tradition of an assumed universality of continuous

[1] Thomson did not argue against Maxwell's theory but rather against the way in which it it was usually presented. He claimed in fact to have "adopted exclusively" the point of view of Maxwell. J. J. Thomson, *Notes on Recent Researches in Electricity and Magnetism* (Oxford, 1893), v.

processes in nature. The way in which he thought about the physics of light was in part a response to the tension in the science of his day between the empirical discreteness of things and the theoretical ideal of continuous action. He was conditioned both by a special way of picturing the field and by an intimate knowledge of the observed properties of the several kinds of radiations to see certain corpuscular features in the phenomena of the free radiation field. Yet his theoretical imagination baulked at the implications of the denial of traditionally assumed continuities for the foundations of physics. Holding to a faith that the familiar framework of physical explanation was an ample structure to contain the revolutionary discoveries in the kinds and properties of radiation, Thomson's role in the post-Maxwellian history of physical optics was that of a bold conservative.

It was said that the "third volume" of Maxwell was written by Thomson. His *Notes on Recent Researches in Electricity and Magnetism* (1893) was intended to bring the subject up to date by filling in the twenty years' progress since the publication of Maxwell's treatise. Thomson's presentation of electromagnetic theory was based on the lines of force which Faraday had introduced into physics a half century before, and which Maxwell had turned to good advantage in his depiction of the electromagnetic field. Thomson, in the spirit of Maxwell's advice, went directly to Faraday's work, and he praised it with a zeal exceeding that of his predecessor; in fact his main criticism of Maxwell was just that he had not exploited the concept of lines of force as fully as he might.

In the *Recent Researches* the lines of force, or "tubes", as their dimensional measure is denoted, are described as real, physical things possessing all the dynamical properties of matter. The tubes are to be pictured as threads embedded in a continuous ether, imparting a fibrous structure to it. A given tube either forms a closed loop or a tautly stretched link between a positive and a negative charge. Thomson required only an "electric" field, explaining magnetic effects by the motion of the lines of electric force. All phenomena of the field are reducible to the position, motion, and shape of the lines or tubes.

In the preface to the *Recent Researches* Thomson deplored the great tendency among students to regard Maxwell's theory as a set of differential equations instead of attempting a "mental picture" of what is going on.[2] This remark holds the key to Thomson's approach to physics. He strove constantly for the concrete image, the starting point, in his judgment, of all physical understanding. The immediacy of Faraday's representation of the field understandably held a strong appeal for him. Tubes of electric force presented ever so much more possibility to the physical imagination than did the abstract symbols of the analytic approach.[3] He was fully aware of

[2] *Ibid.*, v.

[3] The "geometrical", as opposed to the "analytic", approach was the "physical" one; it was exemplified in the concept of tubes of electric force. *Ibid.*, v.

the necessary place of analytic methods in drawing forth quantitative detail to be confronted with experience. But that was the final phase of a theory, a fact, he felt, that tended to be forgotten. Above all he abhored the worship of mathematics as the end-all of science and as the philosophical engine of Swift's parody.[4] He did not specify the persons whose work exemplified the super-analytic approach, but it would be expected that he, like Maxwell, looked upon this regrettable trend as originating on the Continent.

The object of physical theory, Thomson wrote in the preface to the *Recent Researches*, is "suggestion and not demonstration",[5] a view connected with his dislike of the inordinantly mathematical way in physics. Elsewhere he explained that a "theory should be a policy and not a creed".[6] Thomson did have a creed: Newtonian mechanics. But what he means is that mechanical models which conform to Newtonian canons should not be required to have a transcendent significance. More positively, a theory's "most important work is to suggest things which can be tried by experiment, and for this the theory should be one that is easily visualized".[7] In suggesting things that were "tried by experiment" and that were "easily visualized" his views on the nature of light constituted some kind of minimum theory. He referred to them in fact as a "theory",[8] and certain others,[9] though not all,[10] followed him in this way of speaking. In a broader sense his theory of light was only one part of an encompassing theory of electricity based on a Faraday-type field.

2. THE STRUCTURE OF LIGHT

Though Thomson's earliest extended discussion of the structure of light came in 1903, the basic idea was foreshadowed at least ten years before in his *Recent Researches*. There he remarked that in a purely mathematical approach there is no limit to the divisibility of tubes of electric force; however, if the tubes are regarded as "real physical quantities", an actual limit to their division does exist.[11] He explained that since a tube of force terminates on a charge, and since electrolysis defines a "natural"

[4] *Ibid.*, vi.
[5] *Ibid.*, vii.
[6] Lord Rayleigh, *The Life of Sir J. J. Thomson* (Cambridge, 1943), 202.
[7] *Ibid.*, 202.
[8] J. J. Thomson, "On a Theory of the Structure of the Electric Field and its Application to Röntgen Radiation and to Light", *Phil. Mag.*, ser. 6, xix (1910), 311.
[9] Thomson's ideas on the structure of light were referred to as a "theory" in a number of places: G. I. Taylor, "Interference fringes with feeble light", *Proc. Camb. Phil. Soc.*, xv (1909), 114; N. R. Campbell, "Discontinuities in Light Emission", *Proc. Camb. Phil. Soc.*, xv (1909), 310; R. A. Millikan, *The Electron* (Chicago, 1963), 222.
[10] European physicists, who did not characterize a theory as Thomson did, spoke of his "hypothesis" of the structure of light. See, for example: H. A. Lorentz, "Alte und neue Fragen der Physik", *Phys. Zeit.*, xi (1910), 1250; J. Stark, *Die Prinzipien der Atomdynamik: II. Die elementare Strahlung* (Leipzig, 1910-1911), 265; M. Planck, "La loi du rayonnement noir" in *La Théorie du Rayonnement et les Quanta*, ed. P. Langevin and M. de Broglie (Paris, 1912), 101.
[11] J. J. Thomson, *Recent Researches*, 3.

unit of charge, there should also exist a natural unit of electric force. And as the electrolytic units of charge are indivisible, then so must be the associated "unit tubes".[12] A theory of the electric field thus becomes a "kind of molecular theory of Electricity, the Faraday tubes taking the place of the molecules in the Kinetic Theory of Gases".[13] Thomson also pointed out how electromagnetic radiation would relate to a Faraday field: "This view of the Electromagnetic Theory of Light has some of the characteristics of the Newtonian Emission Theory; it is not, however, open to the objections to which that theory was liable, as the things emitted are Faraday tubes, having definite positions at right angles to the direction of propagation of the light. With such a structure the light can be polarized, whereas this could not happen if the things emitted were small symmetrical particles as on the Newtonian Theory."[14]

In his 1903 Silliman Lectures at Yale University, Thomson reinforced the parallelism of the electromagnetic and Newtonian theories, bringing forward the discontinuity of the field implicit in Faraday's conception. He changed the picture somewhat, identifying light with transverse vibrations propagated along tubes of force,[15] a speculation which Faraday himself had made and which Thomson knew about.[16] Oscillating charges, according to Thomson's new approach, set up regular trains of disturbances in the electric tubes attached to them; these tremors, rather than detached loops of tube, are the present analogue of the particles of a gas on the kinetic theory, or of the particles of light on the emission theory. The tubes cannot be considered as "entirely filling" the ether, and therefore the field has a discontinuous structure, an implication which he had "not seen noticed" by anyone before.[17] Accordingly, in this "very simple way of picturing" the processes going on in the propagation of light through the ether, a swarm of "bright specks" should appear against a dark back-

[12] *Ibid.*, 3. The "unit" tubes are all of the same strength. They are distributed throughout space, and not confined to places of non-vanishing electromotive force. The electromotive force is not a measure of the number of tubes present, but of the net excess of tubes pointing in one direction over those pointing in the opposite direction. *Ibid.*, 4.

[13] *Ibid.*, 4. Thomson had already pointed to the analogy between kinetic theory and the Faraday field concept. "On the Illustration of the Properties of the Electric Field by Means of Tubes of Electrostatic Induction", *Phil. Mag.*, ser. 5, xxxi (1891), 149-171.

[14] *Recent Researches*, 43. Thomson pictured a plane wave as constituted of a series of Faraday tubes moving at the speed of light. Their numbers emitted per unit time from the plane source vary in a harmonic fashion. The moving tubes produce a calculable magnetic force at right angles to the direction of the tubes and also to the direction of their motion. *Ibid.*, 42.

J. H. Poynting, a close friend of Thomson, had noted in 1884 a resemblance to the old emission theory in the energy characteristics of light. "The Growth of the Modern Doctrine of Energy", *Collected Scientific Papers* (Cambridge, 1920), 574-575.

[15] J. J. Thomson, *Electricity and Matter* (New Haven, 1904), 62. Thomson briefly described this idea in another publication at about the same time. J. J. Thomson, *Conduction of Electricity Through Gases* (Cambridge, 1903), 258.

[16] Thomson included in his Lectures the following quotation from Faraday's paper, "Thoughts on Ray-vibrations": "The view which I am so bold to put forward considers therefore radiations as a high species of vibration in the lines of force which are known to connect particles and also masses together." *Ibid.*, 62.

[17] *Ibid.*, 63.

ground at the places where the tubes intersect an advancing wave front.[18] In his opinion a uniform wave front is not a necessary part of the electromagnetic theory of light; moreover, he could point to persuasive evidence that the front really has a structure.

Thomson had often shone Röntgen rays through glass tubes containing gases, and it had always struck him as "very remarkable" that only a small fraction of the gas molecules, fewer than one in a billion, emits electrons.[19] It was not easy to imagine how a uniform wave front might ionize only a few of the molecules it passed. If, however, the front is composed of localized spots of high intensity, then both the gas and the radiation are largely empty space, and the expected number of collisions is small. In this interpretation the incident radiation is "analogous to a swarm of cathode rays" advancing through the particles of the gas.[20] Though the evidence for the structure of light was drawn from Röntgen rays, Thomson believed that their manner of "propagation and constitution" was the same as that of a light wave, and that therefore "any general consideration about structure in Röntgen rays will apply also to light waves".[21]

This first observation on the failure of the continuum theory of the field stemmed directly from the recent discovery of the electron and of the possibility of its ejection from matter by light.[22] Thomson was well prepared to mark this critical anomaly in the photo-ionization of gases; two of his primary experimental interests were intimately related to it: conduction in gases, and cathode rays. It has been suggested that Thomson followed Maxwell in thinking that the investigation of the electrical properties of gases was the best way to approach the unresolved problems in electromagnetic theory.[23] In any case the subject became the most absorbing of all of his studies. Thomson was very involved too in the pioneering work on cathode rays, and he was instrumental in convincing many scientists of the existence of a universal, discrete unit of electric charge, the electron. It is entirely fitting that he should have been the first to propose a structure for light based upon a universal, discrete unit of the field, the tube of electric force.

The Silliman Lectures, published under the title *Electricity and Matter* (1904), were reviewed by Oliver Lodge in *Nature*. What struck the reviewer most forcibly was Thomson's treatment of the Faraday lines of force as

[18] *Ibid.*, 63.

[19] *Ibid.*, 63.

[20] *Ibid.*, 65.

[21] *Ibid.*, 63. Very soon after their discovery Thomson convinced himself that Röntgen rays were electromagnetic pulses. J. J. Thomson, "A Theory of the Connexion between Cathode and Röntgen Rays", *Phil. Mag.*, ser. 5, xlv (1898), 172-183.

[22] Millikan noted this connection in 1917. R. A. Millikan, *The Electron*, 222.

[23] D. J. Price, "Sir J. J. Thomson, O.M., F.R.S.", *Supplemento al Nuovo Cimento*, ser. 10, iv (1956), 1616.

"realities",[24] with the consequence that the actual field is discontinuous in structure; and he found the associated view of the nature of light "especially noteworthy".[25] An enthusiastic advocate of Faraday's lines of force, Lodge anticipated that their further development would lead to a "definite microscopic astronomy" and a new *Principia*.[26] Soon after the publication of the Lectures, Whetham's *Experimental Electricity* (1905) described the idea of a discrete wave front as "very unexpected and surprising". More sceptical than Lodge, Whetham would have relegated Thomson's tubes of force to a "museum of conceptual curiosities" if it had not been for the gas ionization phenomenon. That there really existed evidence favouring a discontinuous wave front seemed to him a "remarkable thing".[27]

In his biography of Thomson, Lord Rayleigh noted that no attempt was made in the Silliman Lectures to come to terms with interference phenomena, which appear incompatible with a discontinuous wave front. He recognized a characteristic of Thomson's thought in his ignoring of the optical complications. In suggesting ways of solving any problem that confronted him at the moment he was "very prolific"; but he seldom would "look round the horizon" to see what kind of trouble the suggestions might raise elsewhere.[28] Maintaining a decidedly positive approach toward his science, Thomson preferred to "dwell on what a theory *would* explain rather than what it would not".[29]

When he returned to the question of the structure of light four years later,[30] he continued to ignore the difficulties of optical interference. His task was to study the interaction of light and matter, since that approach "raises some very interesting questions as to the constitution of light waves, questions which hardly occur when we confine our attention to purely optical phenomena".[31] He pointed out that some recent experiments on the ultra-violet ionization of gases carried out in Cambridge and elsewhere provided evidence that ultra-violet light has a structure similar to that of Röntgen rays.[32]

In the same paper of 1907 Thomson widened the range of application of his theory to include the experiments of Philipp Lenard on the photoelectric effect in metals published several years earlier. Lenard found that the number of electrons emitted from an irradiated metal surface increases with the intensity of the radiation, but that the velocity of the electrons

[24] O. Lodge, "Steps Towards a New Principia", *Nature, Lond.*, lxx (1904), 75.

[25] *Ibid.*, 75.

[26] *Ibid.*, 73.

[27] Whetham's reaction is quoted by J. H. Jeans, *Report on Radiation and the Quantum Theory* (London, 1914), 85.

[28] Lord Rayleigh, *J. J. Thomson*, 136.

[29] *Ibid.*, 136.

[30] J. J. Thomson, "On the ionization of Gases by Ultra-Violet Light and on the evidence as to the structure of light afforded by its Electrical Effects", *Proc. Camb. Phil. Soc.*, xiv (1907), 417-424.

[31] *Ibid.*, 419.

[32] *Ibid.*, 421

does not change. Thomson was able to show that this "remarkable result" is a consequence of his ideas on the structure of light. Assuming that the energy of a pulse, or "unit",[33] of light travelling freely along a Faraday tube remains constant, the velocity which an electron receives upon encountering a unit cannot depend upon the number of such units crossing a given area, or, equivalently, upon the intensity of the light. And clearly as the density of the units of light increases, the number of electrons emitted should increase in a proportionate manner.

Erich Ladenburg of the Physical Institute in Berlin had recently published his experimental researches on the ultra-violet photoelectric effect in metals. He concluded that the velocity of photoelectrons is directly proportional to the frequency of the incident light. From this finding Thomson reasoned that the energy in a unit of ultra-violet light should increase with frequency, and he showed why this is the expected result. Initially the light used in the photoelectric experiments is produced by the collisions of cathode ray electrons with the molecules of solid matter. According to Thomson, cathode electrons of higher energy have shorter collision periods, and consequently the "thickness", or wavelength, of their radiative pulses is smaller.[34] Assuming that a large proportion of the electron's energy is converted into radiation, the units of light of shorter wavelength, or equivalently, of greater frequency, will contain more energy, a result in qualitative agreement with Ladenburg's experiments.

Thomson also mentioned the experiments of Innes and Guggenheimer of the Cavendish Laboratory in Cambridge. Their conclusions were similar to Ladenburg's except that they applied to Röntgen rays instead of ultra-violet light.[35] Altogether Thomson had a considerable amount of evidence to back up his theory of light. However, there was still no mathematical development, and the ideas remained in the form in which they appeared in the Silliman Lectures, pictorial and, at best, qualitatively plausible.

One rough, quantitative estimate, based on experimental figures, was entered in the 1907 paper. Thomson stated an approximate value for the spatial density of ultra-violet units, a measure of the "coarseness" of the structure of ultra-violet light. Assuming that a single unit of light gives up all of its energy to a single electron, he calculated the energy of a unit of ultra-violet light from Lenard's measurements of the maximum photoelectron velocity. Knowing the energy in one unit, the number of units crossing a given area in a given time can be found from the known intensity of the incident light. A beam of light of radiant flux equal to 10^{-4} erg per

[33] *Ibid.*, 421. Thomson is using "unit" in a different sense. It does not represent a tube itself, but a bundle of energy travelling along a tube.

[34] In an earlier paper of the same year Thomson showed that the radiation from heated bodies does not conform to the second law of thermodynamics unless the time of collision of an electron with a molecule—the mechanism producing the thermal radiation—is inversely proportional to the kinetic energy which the electron had before the collision. J. J. Thomson, "On the Electrical Origin of the Radiation from Hot Bodies", *Phil. Mag.*, ser. 6, xiv (1907), 217-231.

[35] J. J. Thomson, *op. cit.* (30), 423.

square centimetre per second would be faintly visible. For ultra-violet radiation of this same intensity Thomson found that $3 \cdot 3 \times 10^7$ units would pass through one square centimetre each second. This figure corresponds to an average density of about one unit of light per litre of space. Light of such "exceedingly coarse character" might be "pictured by supposing the particles on the old emission theory replaced by isolated transverse disturbances along the lines of force".[36] Thomson remarked that a γ ray unit probably has an energy 10^4 times that of an ultra-violet unit; so that for ordinary intensities the structure of γ rays would be very much coarser than that of ultra-violet light. These widely dispersed units "will have all the properties of material particles, except that they cannot move at any other speed than that of light".[37] This observation explains why γ rays were thought by some to be high-speed, neutral particles.

These calculations for high-frequency radiation indicate that the departure from uniformity in the distribution of energy over the wave front can be astonishingly large. A coarse-grained structure highlights the problem of making sense of the common optical, or wave-like, phenomena, and it would seem that this is the place for Thomson to bring up the difficulties. But he says nothing about them, perhaps because they were too obvious. In any event his public silence does not mean that he was unconcerned with the optical implications of his theory. Early in 1909 G. I. Taylor reported to the Cambridge Philosophical Society on a long, patient series of measurements of interference phenomena carried out in the Cavendish Laboratory the previous year.[38] They had been suggested by Thomson, and their purpose was to uncover optical evidence for the structure of free radiation. Taylor acknowledged that all support for Thomson's theory had been indirect. Since the ordinary optical phenomena represent average effects, they are not suitable in normal circumstances for differentiating between the conventional wave-continuum theory and Thomson's modification of it. In order to reach below the averages to the individual processes of the field, Taylor made photographs of the shadow of a needle in extremely weak light. Even when the flux of radiant energy was no greater than that of a standard candle at a distance of over a mile, there was no reduction in the sharpness of the diffraction pattern. The energy falling each second on one square centimetre of the photographic plate in light of this feeble intensity was 5×10^{-6} erg, and the average energy in one cubic centimetre of space was $1 \cdot 6 \times 10^{-16}$ erg. Taylor reported that Thomson regarded the figures for the energy density as establishing an upper limit on the magnitude of one of the "indivisible units of energy". Thomson must have reasoned that more than one unit was needed to produce fringes, and that units separated by more than one

[36] *Ibid.*, 423.
[37] *Ibid.*, 424.
[38] G. I. Taylor, "Interference fringes with feeble light", *Proc. Camb. Phil. Soc.*, xv (1909), 114-115.

centimetre would not be expected to interfere. The real meaning of Taylor's experiments is that the microscopic scale of the structure of light had not yet been reached.

3. EINSTEIN'S HYPOTHESIS OF LIGHT QUANTA

Thomson was not alone in arguing that light has a structure. At about the same time Einstein proposed that the radiation field was characterized by a special kind of discreteness, one that was associated with the idea of energy quanta. He grouped his ideas on the nature of light under a "hypothesis" and did not claim that they constituted a theory. His views have certain close similarities with Thomson's, and they should be examined.

The quantum theory began in 1900 when Planck published his theory on the energy distribution in black body radiation. To derive the radiation formula he needed to assume that the energy of the elementary radiators can only vary by discrete amounts, or quanta, and that the quanta are proportional to the frequencies of the radiators. The first advance beyond Planck's original theory was made by Einstein in 1905.[39] He suggested that the energy of free radiation occurs in localized, indivisible units, or quanta, and that the energy of a light quantum is proportional to the frequency of the light. For Planck the energy discontinuity was restricted to the material oscillators, while for Einstein it was contained in the radiation field. It made a very great difference to contemporary physicists which of the two positions was taken, a concern which was a distinguishing mark of the quantum theory, particularly in its early phases. For many years after 1905, Einstein's hypothesis was considered an unwarranted speculation by most, and Planck's approach to quantum physics was much preferred of the two.

The ways in which Einstein and Thomson thought about light were very different. Einstein did not see the field in terms of tubes of force and an ether. The suggestion cannot be taken seriously that Einstein was put on the track of light quanta by the 1903 Silliman Lectures, a historical misunderstanding that later arose.[40] He arrived at the notion of light quanta from a concern for formal symmetries, and from a unique statistical approach to the second law of thermodynamics. In particular he was uneasy over the formal differences in the physics of material particles and

[39] For an excellent exposition and analysis of Einstein's light-quantum hypothesis, see M. J. Klein, "Einstein's First Paper on Quanta", *Natural Philosopher*, ii (1963), 59-86. For further discussion, see also M. J. Klein, "Ehrenfest's Contributions to the Development of Quantum Statistics. I", *Koninkl. Nederl. Akademie van Wetenshappen, Proceedings*, ser. B, lxii (1959), 41-62; "Einstein and the Wave-Particle Duality", *Natural Philosopher*, iii (1964), 1-49; "Einstein, Specific Heats, and the Early Quantum Theory", *Science*, cxlviii (1965), 173-180.

[40] Millikan associated Einstein's with Thomson's ideas on a number of occasions. See, for example, R. A. Millikan, *The Electron*, 222. The unlikelihood that Einstein was acquainted with Thomson's theory is pointed out in M. J. Klein, "Einstein's First Paper on Quanta", *loc. cit.* (39), 80.

the physics of the continuous electromagnetic field. These differences are removed, he argued, if one accepts the applicability of statistical fluctuation methods to radiation in free space. The entities of the statistical counting are the localized, indivisible energy quanta. Initially Einstein drew his arguments for the existence of light quanta from the Wien black-body radiation law, the high-frequency limit of Planck's formula. However, he was soon able to show that Planck's law too demands an atomic structure of light.

From the beginning both Einstein and Thomson stressed the resemblances between their ideas and the Newtonian emission theory. Neither of them made any attempt at first to explain the purely optical phenomena on the basis of a discrete radiation field. There is a sense in which, of the two, Einstein's view more nearly approaches the older corpuscular interpretation of light. The quanta are treated as structureless point-particles, and no nod is made in the direction of the wave theory by endowing them with some sort of vibratory motion. But at the start Thomson really went further than Einstein. In his theory, as in its Newtonian counterpart, the independent units of light are granted a complete complement of material properties: mass, momentum, and energy.[41] Einstein in 1905 spoke only of the energy of quanta, and it was some time before he filled out the materialistic analogy inherent in his light-quantum hypothesis. Four years after his first paper he pointed out that the emission of radiant energy by one body and its absorption by another is relativistically equivalent to a transfer of mass. He made use of this observation from relativity theory to underpin the analogy between quanta and particles; as on the Newtonian theory, emission and absorption are seen as the exchange of massy particles. It was only in 1916 that Einstein attributed momentum to light quanta in his interpretation of emission and absorption as directed processes, though the idea was implicit in his 1909 papers.

Thomson's statements on the structure of light were not accompanied by a quantitative prediction, and Einstein's were; and this fact made a great difference in the respective futures of the two positions. The most important historically of Einstein's predictions is that of the law governing the photoelectric ejection of electrons from metals. He derived an exact, linear relation between the frequency of incident light and the maximum energy of the electrons emitted from a metal surface. In 1905 there was no experimental evidence for such a relation. The foundations of electro-

[41] According to Thomson the ether has mass but no weight. He pictured the moving lines of electric force as "gripping" the neighbouring ether and carrying it with them, the mass of the transported ether being calculable by the laws of electricity. Though the mass of light is small, its momentum and energy are considerable due to its great velocity. The main reason why Thomson endowed the "invisible universe" of the ether with mass and motion is that otherwise the interaction of electrical bodies would not obey Newton's third law of motion. For his own account of the connection between mechanics, electricity, and the ether, see J. J. Thomson, "On the light thrown by recent investigations on electricity on the relation between matter and ether", *Annual Report of the Smithsonian Institution*, 1908, 233-244.

magnetic theory, which had received vast experimental confirmation, were challenged by a hypothesis which had literally none. But there was a quantitative formula which predicted a new, verifiable law, and this fact spared Einstein's concept of light quanta the oblivion many would have chosen for it. It is not by chance that his 1905 paper became known as his paper on the photoelectric effect, even though this application was only a small part of the study. Thomson's theory foundered largely because it had nothing analogous to the photoelectric law to anchor it. His very reasonable observation on the photo-ionization of gases may have protected his theory from some of the indifference and hostility to which Einstein's hypothesis was subjected; but the observation was *a posteriori*, and it was not capable of being turned into mathematical law.

Thomson's 1907 paper, an elaboration of the ideas of the Silliman Lectures, appeared two years after Einstein's first publication on quanta. It would be interesting to know not so much if Thomson picked up Einstein's ideas—he did not—but if he knew of them, and if they were the reason why he took up his theory again after four years and why he gave his views the particular stress that he did. Thomson, like Einstein, emphasized the constancy of the energy of the free units of light. And he supported his ideas by applying them to the photoelectric effect in metals and, in particular, to Lenard's experiments, as Einstein also had done.

Any attempt to decide on the question of Thomson's awareness of Einstein's hypothesis of light quanta must be speculative. He never mentioned Einstein by name, but this means little since it was not his habit to acknowledge others for their ideas.[42] In fact it is not at all unlikely that he read Einstein's publications. Rayleigh wrote of Thomson's "unequalled knowledge of the literature",[43] and of his practice of poring over the major German journals, keeping himself "*au fait* of a constant stream of thought largely independent of the parallel development" in England.[44] But it should be said in this connection that in 1907 almost no notice had yet been taken of Einstein's hypothesis.[45] It is noteworthy that Ladenburg, whose photoelectric experiments interested Thomson so much, did not cite Einstein.

It is fair to say that if Thomson had come across Einstein's work by 1907 he had not taken it seriously. For that matter he had not taken Planck's theory, which he almost certainly did know about, very seriously

[42] Lord Rayleigh quoted G. F. C. Searle, a lifelong colleague of Thomson, who had observed that Thomson "could not always remember how an idea had got into his mind . . . He would be told by someone or would read somewhere some new idea. Later on he would find the idea floating in his mind and he would suppose that the idea was original to himself and would treat it as if it were." Lord Rayleigh, *J. J. Thomson*, 118-119.

[43] *Ibid.*, 136.

[44] *Ibid.*, 219.

[45] A rare early interruption of the nearly total silence occurred in 1907 when Joffé indicated a partial support for Einstein's photoelectric law in Ladenburg's experiments. A. Joffé, "Eine Bemerkung zu der Arbeit von E. Ladenburg: 'Über Anfangsgeschwindigkeit und Menge der photoelektrischen Elektronen usw.'," *Ann. d. Phys.*, xxiv (1907), 939-940.

either.[46] He may have directed his thoughts in 1907 to the idea of discrete energy units as a result of some recent familiarity with the quantum theory, but there is no way of establishing this as a motivating factor. One thing is clear; he was not naturally drawn to the statistical arguments of Planck and Einstein, since he used nothing like them himself. His own approach to black body radiation was based on an examination of the collisions of electrons and molecules, assuming various force laws. The more customary statistical approaches to the radiation problem were of "such an extremely general character" that he had "found some difficulty in following them and in appreciating their rigour".[47] Illustrating his attitude toward statistics generally, he recalled with approval De Morgan's saying that if a probability calculation took up more than a half-sheet of notepaper its result should not be accepted without further evidence.[48] Thomson lightly remarked that he was prepared to give up the theorem of the equipartition of energy if it were causing the quantum theorists all the trouble.[49]

There is still the question of why Thomson associated Lenard's and Ladenburg's photoelectric experiments with his theory of light if he were not responding to Einstein's work. The answer appears to be that Ladenburg had a direct influence on Thomson, leading him to abandon a prior theory of photoelectricity in metals. In 1905 Thomson had argued that incident light triggers the explosion of metal molecules, and that the energy of the ejected electrons comes from the unstable molecules rather than from the electric forces in light.[50] Two years later he acknowledged that the explosion theory was probably not compatible with the new discovery that the maximum photoelectron velocity varies continuously with the frequency of light.[51] This retraction suggests that Ladenburg's recent papers may have prompted his revival of the theory of light of the Silliman Lectures. The photoelectric effects in metals and gases are sufficiently similar so that Thomson would associate the same mechanism with each. Photoelectric selectivity may be referred to the spatial localization of the energy in the incident radiation rather than to the energies of the metal molecules. Thomson brought in Lenard's experiments, of which he was

[46] Given his interest in the subject, Thomson undoubtedly followed Jeans' and Rayleigh's discussion of the energy distribution in the radiation from heated bodies. In a letter to *Nature* in 1905 Rayleigh quoted Planck's theoretical formula, but confessed that he had not "succeeded in following Planck's reasoning". Lord Rayleigh, "The Dynamical Theory of Gases and of Radiation", *Nature*, lxxii (1905), 54-55.

In his 1907 paper on the radiation from heated bodies, Thomson wrote down Planck's final formula for black-body radiation in its empirical form, i.e. without the new constants h and k. He did not say anything about Planck's ideas but only remarked that at low (Thomson slipped and said "high") temperatures Planck's formula agreed with experiment better than Rayleigh's. J. J. Thomson, *op. cit.* (34), 230.

[47] J. J. Thomson, *op. cit.* (34), 217.

[48] "Physics at the British Association", *Nature*, xcii (1913), 306.

[49] *Ibid.*, 306.

[50] J. J. Thomson, "On the Emission of Negative Corpuscles by the Alkali Metals", *Phil. Mag.*, ser. 6, x (1905), 584-590.

[51] J. J. Thomson, *op. cit.* (30), 422.

"*au fait*" as a matter of course, in order to demonstrate the capacity of his theory of light to account for all known properties of the photoelectric effect in metals. The reason why he emphasized that the energy of a unit of light in space does "not diminish" is because Lenard's experiments demand it.

4. THOMSON AND THE QUANTUM THEORY

Thomson delivered his by now familiar views on the structure of light once more in 1907 in his Adamson Lecture.[52] When he next spoke on the subject he had already begun to take seriously the quantum theory and its bearing on his own developing ideas. In his inaugural address as President of the British Association in August 1909, Thomson stated his opinion for the first time on the concept of energy quanta. It is not exactly clear why he became involved just at this time. Perhaps he had come across more references to the quantum theory, and his interest had been aroused in this way. The tone of his discussion suggests that he had been studying Planck's book on heat radiation published in 1906. It may be seriously questioned how carefully he read it. He began his remarks by noting that an emission-type theory, i.e. a theory in which the energy of light is the "kinetic energy of the light particles", had "lately received a good deal of support" from Planck's "very remarkable series of investigations on the Thermodynamics of Radiation".[53] Planck, he continued, had "pointed out that the expressions for the energy and entropy of radiant energy were of such a form as to suggest that the energy of radiation, like that of a gas on the molecular theory, was made up of distinct units".[54] This viewpoint is the one associated with Einstein's hypothesis, and it is precisely the way Planck did *not* regard the quantum theory. This distortion of Planck's meaning probably arose from a particular reading of the part of his book dealing with the statistics of oscillators.

Thomson may have seized upon this interpretation of Planck's thought in order to form a physical picture of the energy units of the statistical calculations. A more cogent reason is that he had already put forward an argument for a certain kind of discontinuity in his theory of light, and that he saw Planck's theory as being in some sort of competition with his own. In this first mention of quanta he admitted that there were "strong reasons for thinking that the energy in the light waves of definite wave-length is done up into bundles, and that these bundles, when emitted, all possess the same amount of energy".[55] However, a unit of light in any interaction with matter cannot be considered "indivisible";

[52] J. J. Thomson, "On the light thrown by recent investigations on electricity on the relation between matter and ether, *loc. cit.* (41).

[53] J. J. Thomson, "Inaugural Address", *Nature*, lxxxi (1909), 255.

[54] *Ibid.*, 255.

[55] *Ibid.*, 255.

e.g. when a unit undergoes partial reflection it separates along two paths and its energy cannot meaningfully be thought of as still belonging to a single unit. Throughout his long argument with the quantum theory, Thomson's basic position was that energy itself has no coherence, or inherent structure,[56] but rather that the carriers of the energy—Faraday tubes, electrons, etc.—are the permanent, indivisible entities.

There were some factors lying outside the specific work of Thomson and his co-workers which may have tended to force a certain attitude toward the quantum theory. It is a curious circumstance that in Europe at this time the interpretation of the quantum theory as one of light quanta was held only by a very small minority, while in Britain the situation was reversed and nearly everyone who had any point of view at all considered the theory to be based on an atomic constitution of radiation, or of energy in general.[57] There is no decided pattern in the British reactions to the idea of quanta to indicate why there should be this tendency to read Planck's theory in a manner never intended by its author. Perhaps there was something in the way the British regarded the ether that is at the root of their responses—a certain stress on the concreteness of its mechanical representation, or on its primacy in physical explanation. It is certainly true that some British physicists were troubled for a long time about the seeming failure of the law of the equipartition of energy when applied to the ether. One thinks of Rayleigh and Jeans, who pointed to the inadequacy of classical physics for describing the ether and, in this context, who gave one of the first public notices in Britain of the quantum theory, indicating its apparent success in the place where the accepted theory broke down. The

[56] Thomson spoke both of molecular energy and of molecular radiant energy in his discussions of the quantum theory, and it is not clear that he made a distinction between them. If energy is always discrete, then radiant energy is necessarily discrete; the reverse implication, however, does not hold. This difference, which was seldom explicitly recognized, was noted in H. A. Wilson, "On the Statistical Theory of Heat Radiation", *Phil. Mag.*, ser. 6, xx (1910), 121.

[57] It appears that the first public response to the quantum theory in Britain was that of Larmor. At the meeting of the British Association in 1902 he rederived Planck's radiation formula, "discarding the vibrators, and considering the random distribution of the permanent elements of the radiation itself, among the differential elements of volume of the enclosure, somewhat on the analogy of the Newtonian corpuscular theory of optics", *Report of the British Association*, 1902, 546. In his Bakerian Lecture in 1908 Larmor put forward a new statistical basis for Planck's theory, concluding that it was "now without any implication that energy is itself constituted on an atomic basis", J. Larmor, "On the Statistical and Thermodynamical Relations of Radiant Energy", *Proc. Roy. Soc.*, ser. A, lxxxiii (1909), 90.

A. Schuster, having argued that certain ideas of the quantum theory "could only be true if energy had, like matter, an atomic constitution", thought that he had "thus finally disposed of the matter by a *reductio ad absurdum*". But he discovered that this view was not considered absurd by everyone, and that "such atomic constitution" came to be "openly advocated", A. Schuster, *The Progress of Physics During 33 Years (1875-1908): Four Lectures Delivered to the University of Calcutta During March 1908* (Cambridge, 1911), 111.

Jeans wrote in 1910 that Planck's law demanded "something more" than what was contained in Planck's original papers, namely, that the "energy in the aether itself must also be atomic", and that it should be "physically impossible to divide these atoms" of energy. The only alternative, if one "agreed that these conditions do not hold in nature", and Jeans believed they did not, was to suppose that the "state of the aether represented by Planck's law is not a final steady state", J. H. Jeans, "On Non-Newtonian Mechanical Systems, and Planck's Theory of Radiation", *Phil. Mag.*, ser. 6, xx (1910), 953.

reason why the equipartition theorem led to an incorrect prediction for black-body radiation was that the ether was imagined to be a continuum with an infinite number of degrees of freedom. It may have seemed that Planck's theory succeeded because it somehow denied the continuum. From this notion it is a short mental step to imagining the energy units to be identical with an etherial structure replacing the continuum.

There is another, related fact about the general response to the quantum theory which is an important part of the background of Thomson's thought. In Britian there was not a single supporter of the quantum theory, in any of its forms, until about 1912.[58] There is little doubt that the solid British opposition largely followed from the interpretation of the theory as one of field quanta. Planck's proposal was seen as standing in contradiction to the established electromagnetic theory of Maxwell. Of all the British, Thomson might have been expected to have most sympathy with the new theory. He was not alarmed by the presence of discontinuities in the field as many of his colleagues were. Thomson, like Einstein, was willing to accept the spherical-wave theory as true only in an average sense, and this flexibility of thought placed him in advance of his countrymen and brought him half-way to the quantum theory. But he did not move the remaining half-distance, and that is a peculiar fact about him. Both at home and abroad Thomson was looked upon as a bold thinker,[59] and yet his boldness went only so far. His unparalleled inventiveness in the realm of mechanistic imagery was constrained by his deep-seated conviction of the truth of the classical laws of mechanics. These he never questioned, and the quantum theory was doing just that. Thomson's son was right in suggesting that it was his father's wish to explain the quantum theory by Newtonian principles.[60] Wherever the new ideas threatened the foundations he would step in with one or another construction of corpuscles, tubes, ether, and forces. But in the end the

[58] The response of the British and Continental physicists to invitations to the first Solvay congress presents a striking comment on the respective reactions of British and Europeans to the quantum theory. The congress, an international gathering of leading physicists, was held in Brussels in 1911. Its purpose was to discuss the crisis in physics brought about by the quantum theory. Of the list of twenty-five to whom invitations were sent, nineteen were European and six British. Every European but one accepted his invitation, while of the six British invited only two attended. The four who declined were Larmor, Rayleigh, Schuster, and Thomson. The two who attended were Jeans and Rutherford. Five of the six were unsympathetic to the quantum theory, and the sixth, Rutherford, while not unsympathetic, was not deeply involved with quanta. The names of those invited are included in the invitational letter from E. Solvay, 15 June 1911, Lorentz Collection, Algemeen Rijksarchief, The Hague.

[59] Thomson's contemporaries frequently applied the adjective "bold" to his ideas. In reference to a paper delivered at the British Association, the reporter for *Nature* spoke of the "boldness now always expected from Sir J. J. Thomson", *Nature*, xcii (1913), 305. Millikan remarked on the "boldness" of Thomson's proposal of a structure of light, R. A. Millikan, *The Electron*, 222. Arguing that a certain atomic model of Thomson should be taken seriously, M. Born observed that such "bold concrete ideas have often led to surprising consequences", M. Born, "Über das Thomsonsche Atom-modell", *Phys. Zeit.*, x (1909), 1031. The same atomic model was described by Lorentz as a "bold hypothesis", H. A. Lorentz, "Alte und neue Fragen der Physik", *Phys. Zeit.*, xi (1910), 1251.

[60] G. P. Thomson, *J. J. Thomson and the Cavendish Laboratory in his Day* (London, 1964), 156.

quantum theory was the right way, and Thomson's vast ingenuity had been spent on stop-gaps which could not shore up forever the Newtonian edifice.[61]

Thomson's special understanding of Planck's theory may have received a strong boost from the energetic entry of Johannes Stark, a leading experimentalist at the Physical Institute in Aachen, into the discussions of the quantum theory. As early as 1907 Stark had noticed the new theory, and by the following year he had begun to speak in the same unclear way as Thomson of the "light-quantum hypothesis" as "laid down by M. Planck".[62] At this time he did not yet mean the hypothesis in Einstein's sense, and in fact he argued against the need for a discontinuous ether. In September 1909, however, Stark retracted his earlier interpretation of quanta,[63] becoming the only physicist besides Einstein to argue vigorously for the possible reality of atomic radiation. From the observation that photoelectrons have the same velocity as the cathode electrons producing the photo-ionization beam, he deduced that the energy of high-frequency radiation does not spread out in space. This route to the recognition of the atomicity of light was, he believed, a "clearer and shorter" one than Einstein's,[64] and Thomson would have agreed wholeheartedly. The theoretical speculations of Stark and Thomson remained much closer to the observed phenomena than did the statistical approaches of Planck and Einstein. It was Stark's opinion, as it was Thomson's too, that it was a question of the highest theoretical significance to decide as early as possible between the rival hypotheses of light quanta and ether waves, and in 1910 he reported on an extensive series of experiments whose outcome favoured the light-quantum hypothesis. Since Stark and Thomson were interested in many of the same kinds of researches, notably in studies of ionization phenomena, it is fair to assume that Thomson read Stark's

[61] Thomson's concern was with the preservation of Newtonian physics, that group of laws of matter and motion in terms of which he believed the field to be wholly explicable. A more common view was that electromagnetic theory and mechanics were individually challenged by the notion of discontinuous physical processes. The British physicists J. W. Nicholson, Jeans, and S. B. McLaren all believed that Newton's laws, or more generally, any laws of motion in mechanics expressible in terms of differential equations, would have to be replaced by new and as yet undiscovered laws in the description of atomic-scale phenomena. See J. W. Nicholson, "The Constitution of the Solar Corona. II", *Mon. Not. Roy. Astr. Soc.*, lxxii (1912), 677; J. H. Jeans' opening address in the "Discussion on Radiation", *Report of the British Association*, 1913, 378; S. B. McLaren, "The Theory of Radiation", *Phil. Mag.*, ser. 6, xxv (1913), 43-44. However, two of the three, Nicholson and McLaren, believed that the classical theory of light was correct. Only Jeans thought that the nature of light was an open question. McLaren forcefully expressed his sense of the relative importance of the laws of mechanics and light: it would be a "small thing to sacrifice the ordinary mechanical notions of matter" in order to "save the classical view of radiation as a continuous wave motion". *Ibid.*, 43.

[62] Stark had no illusions about the standing of the quantum theory among his contemporaries generally. By revealing that the light-quantum hypothesis was behind his experiments on canal rays he said that he was aware that he risked "discrediting the experimental results". J. Stark, "Neue Beobachtungen an Kanalstrahlen in Beziehung zur Lichtquantenhypothese" *Phys. Zeit.*, ix (1908), 768.

[63] J. Stark, "Über Röntgenstrahlen und die atomistische Konstitution der Strahlung", *Phys. Zeit.*, x (1910), 579-586.

[64] *Ibid.*, 583.

publications regularly, including those containing his interpretation of the quantum theory.

5. WAVES, PARTICLES, AND TUBES OF FORCE

As early as February 1909,[65] Norman R. Campbell had begun conducting fundamental experiments on the nature of light in the Cavendish Laboratory, publishing his results in two parts in the winter, 1909-1910.[66] He grouped Thomson's and Planck's theories—the latter interpreted in the "sense given to it by Stark"[67]—and opposed them to the ordinary wave theory of light. Since Stark's September 1909 paper was cited in Campbell's first report in November, and since the experiments had been undertaken months before, the effort must have been motivated by Thomson's challenge to the spherical-wave theory; Stark's views were brought up as later reinforcements to the challenge.

Campbell used an interferometer to divide a beam of light. The intensities of the two halves were adjusted so that their mean difference was zero. On the wave theory an elementary radiator in the source must send light into one part of the beam if it sends it into the other. According to the theories of Thomson and Stark, a radiator directs light into one or the other half, but not into both. If the wave theory is right, the divided parts are correlated, and the mean fluctuation of the difference of intensities of the half-beams ought to be zero. On the basis of the new theories, no correlation would be expected and the fluctuation should not vanish. Campbell noted that a correlation in the parts of a parallel beam might be expected on Thomson's theory and not on Planck's; but beams emerging at different angles from the source would definitely not be correlated on either theory. His experiments failed to decide between the wave theory and the new theories, the reason being that the relative intensities of the two beams depend upon the intensity of the source, and the irreducible fluctuations in the lamp masked those which were being measured. These experiments, like Taylor's earlier ones, failed to reach to the purely optical manifestations of the structure of the field. The spirit in which Campbell undertook the tests is revealed in his confession of disappointment. He had really expected to find a structure. All that he could say in the way of

[65] In a paper read 22 February 1909, Campbell hinted that he was already engaged in experiments on the structure of light. Explaining why he undertook a study of probability, he said that "it should be remembered that radioactivity is not the only discontinuous process which we study. The trend of modern theory is everywhere to replace by discontinuity the continuity which was the basis of the science of the last century. Any method which is especially applicable to discontinuous processes is certain to be fruitful of results in every department of investigation . . . at the present time I am engaged in an attempt to apply the method to a totally different form of ionization current", N. R. Campbell, "The study of discontinuous phenomena", *Proc. Camb. Phil. Soc.*, xv (1909), 117.

[66] N. R. Campbell, "Discontinuities in Light Emission", *Proc. Camb. Phil. Soc.*, xv (1909), 310-328 and xv (1910), 513-525.

[67] *Ibid.*, 311.

conclusion was that "contrary to the hopes of the author, no evidence has been produced *against* the 'spherical wave' theory".[68]

In February 1910, while Campbell was still unsuccessfully trying to perfect his instruments to turn up evidence against the wave theory, Thomson brought out a paper attempting among other things to bring together in a single theory both the wave and the particle characteristics of light.[69] These seemingly contradictory properties are reconciled on the view that radiant energy is concentrated in small specks as demanded by the "emission theory", while the tubular disturbances containing the localized energy are vector quantities normal to the direction of propagation, as in the "usual form of the undulatory theory".

Again Thomson was not the only one to discuss the dual nature of light. Einstein too spoke out on the problem of unifying wave and particle properties.[70] In imagining the energy of light to exist in local concentrations, both Einstein and Thomson made more intelligible the interaction of matter and light, and they made less intelligible certain optical effects which had received a complete understanding on the wave theory. Looking ahead, Einstein expected the next phase of physics to bring about a fusion of the wave and Newtonian emission theories. Thomson anticipated the same thing, but there was a difference in their forecasts. In Einstein's judgment both mechanics and electrodynamics would have to be supplanted by a whole new physics before the connection of wave and particle properties would be understood. For Thomson, the existing physical principles were an adequate tool for working an imminent reconciliation of wave and particle. In fact he believed that he had discovered the key to the dualistic behaviour of light in the discrete tubes of electric force. He argued the superiority of his own over the quantum theory on just these grounds; a Faraday field accommodates the disparate qualities of light, and the alternative concept of quanta cannot easily do this.

Thomson was concerned to show that optical interference, the firmest support of the wave theory and the most severe trial of a theory of structure, can be interpreted on the basis of Faraday tubes.[71] An electron vibrating in position sends a continual train of disturbances outward along an attached tube of force; and an electron in collision sends out a single "kink"-like disturbance. In order that light striking a slit or obstacle can

[68] *Ibid.*, 521.

[69] J. J. Thomson, "On a Theory of the Structure of the Electric Field and its Application to Röntgen Radiation and to Light", *Phil. Mag.*, xix (1910), 301-313.

[70] Einstein's ideas on this subject are thoroughly analysed in M. J. Klein, "Einstein and the Wave-Particle Duality." Einstein thought that the difficulties of combining wave and particle characteristics were not insuperable. At a discussion in 1909, in which Planck and Stark took part, Einstein explained that he imagined a light quantum as a "singularity surrounded by a large vector field". He thought that such an interpretation could explain interference phenomena. A. Einstein, "Über die Entwicklung unserer Anschauungen über das Wesen und die Konstitution der Strahlung", *Phys. Zeit.*, x (1909), 826.

[71] J. J. Thomson, *op. cit.* (69), 311-312.

interfere with itself, the disturbances passing along different tubes must have definite phase relations. Thomson argued that one should expect resonances to be set up in neighbouring tubes, producing the needed correlation in the parts of a wave front. Any vibration in a single tube will, when passing other electrons of the source, excite sympathetic vibrations in them, which in turn will be propagated along their respective tubes of force. On this picture there should be groups of tubes having correlated vibrations emanating from the source and running through the interferometer slit, and the explanation of the fringes proceeds as on the wave theory; the secondary motions in the group of tubes caused by the primary vibrations striking the edges of the slit will be in phase and will therefore interfere.

Thomson pointed out that the sustained sharpness of the diffraction pattern in Taylor's experiments in weak light is a consequence of the resonance effect. These experiments also served him as a focus for a renewed attack on the quantum theory. Unless the amplitude of a transverse vibration is greater than the width of the slit, in which case it might excite interfering vibrations in the electrons of the sides of the slit, one has to assume that several source tubes collectively produce the interference fringes. If the energy of the vibrations passing along each of the tubes is the same, as the quantum theory requires for light of the same frequency, the magnitude of the energy unit must be "exceedingly small, much smaller than the unit of radiant energy for light in the visible part of the spectrum given by Planck's theory".[72] For Thomson estimated, from the previously calculated energy per unit volume in the light used in Taylor's experiments, that there would be less than one of Planck's energy units in a litre of space. He concluded that unless the units were emitted in bunches, it was "difficult to imagine that they could be sufficiently crowded together to interfere".[73]

At the same time that Thomson was exhibiting the wave-like characteristics of the Faraday field, he was also pushing the corpuscular analogy to its extreme limits. He imagined that the interchanges of energy between pulses of light and charged particles are similar to those between "colliding molecules". More than that, the pulses are expected to be capable of exchanging energy among themselves independently of the presence of matter, and of establishing within an enclosure an equilibrium distribution of energy similar to that of "molecules of a gas".[74]

In this new study Thomson put forward an idea of how the tubes of force are related to the material sources of the field, the electrons. He conjectured that the electric force of an electron is exerted in one direction only, instead of acting in a spherically symmetric manner as it was

[72] *Ibid.*, 312.
[73] *Ibid.*, 312.
[74] *Ibid.*, 309.

customarily assumed to do. He identified the elementary tube of force with an indefinitely extended cone of small angle, the vertex coinciding with the centre of the electron. The actual electric field is clearly "molecular in constitution"; its smoothed-over appearance is a consequence of the superposition of the randomly oriented elementary fields of the large number of charges constituting any macroscopic source.[75] As the unit of charge, the electron, was known, Thomson urged that experiments be devised to determine the field surrounding it. He did not mean to reduce the field discontinuities to the more intelligible material ones; rather, in 1910, he advocated jointly the "atomicity" of field and matter.

6. The Structure of Light: 1910 and After

In his 1910 paper Thomson promised a sequel publication on the interaction of several elementary fields with one another. That paper never appeared. Instead, a few months later, he published a study, "On the Theory of Radiation",[76] which was in part an extension of his earlier investigation of the radiation from heated bodies. This paper was not about free radiation but rather about the means of its production. Thomson's readers were offered a detailed analysis of a particular mechanism capable of explaining black-body radiation and the photoelectric effect. There was no mention of the nature of light. In the new scheme photoelectrons are produced when light of resonant frequency falls on electrons revolving around electric doublets, the crucial mechanisms imagined to exist in molecules. It turns out that the kinetic energy of the released electrons is proportional to the frequency of light; this is the fundamental relation of the quantum theory and the reason why Thomson settled on the electric doublet as a concrete representation of the Planck resonator. He supposed that there is only a small fraction of molecules containing doublet systems of any given frequency, from which it follows that the probable number of electrons emitted by a monochromatic beam would be very small compared to the number of molecules present "on any view as to the constitution of a light-wave".[77] The kind of observation which first suggested to him that light has a certain structure was now related to a material system. And the energy of the photoelectrons does not come from the light but from the molecules, the viewpoint of the once-rejected explosion theory. Thomson's present object was expressly to put down the quantum theory of light; as the electric doublets explain the quantum relations, it is not necessary to assume that "light is made up of unalterable units", a view which was "exceedingly difficult to reconcile with well-known optical phenomena".[78] Thomson did not retract his ideas on the structure of light, but he shifted ground.

[75] *Ibid.*, 302.
[76] J. J. Thomson, "On the Theory of Radiation", *Phil. Mag.*, xx (1910), 238-247.
[77] *Ibid.*, 246.
[78] *Ibid.*, 246.

Over the next several years he concentrated on constructing classical mechanisms capable of accounting for the energy-frequency law of the quantum oscillators,[79] all the while holding his theory of light in abeyance. There can be no doubt that he was encouraged to take this direction in 1910 by the successful applications of the quantum theory, which were just beginning to have some general impact on the scientific community. Also in that same year a weighty criticism was mounted in Europe against the light-quantum hypothesis.[80] Its opponents stressed the apparent incompatibility of light quanta and optical phenomena. Thomson may have joined their arguments to his own to conclude that the core of truth in the quantum theory had really nothing to do with light. An understanding of the energy-frequency relation, whose importance he fully appreciated, was to be sought in the actions of material systems. He also knew that he could obtain quantitative results from atomic models, while he had not yet been able to discover an exact theory of the structure of light. Consistent with his general attitude toward physical theory, he dropped an unfruitful direction for the time being in favour of a more productive one.

There did not need to be any new optical experiments to gather evidence against the quantum theory. In a sense every interference effect that had ever been observed counted as evidence. The burden lay with the quantum theory to demonstrate that it could comprehend the common properties of wave motion. For phenomena involving the interaction of light and matter, however, the case was far otherwise. All of the capability of the new theory, as well as the failure of the old one, lay in this relatively uncharted territory. But even this capability was not immune from challenge. Thomson had in hand some new, direct evidence against the existence of quanta, drawn as it were from the theory's more secure provinces. In the Cavendish Laboratory A. L. Hughes distilled metals inside a vacuum to eliminate the surface films he believed responsible for the inconstant outcomes in photoelectric measurements. Early in 1911 he published his findings on the photoelectric effect in metals,[81] and they conformed exactly to the chief criticism which Thomson had levelled against the quantum theory. The slope of the line relating the electron energy and the frequency of light was not equal to Planck's constant as predicted by the photoelectric equation; its value implied that only part of the energy in a unit of light is imparted to the photoelectron. It was just such divisibility of the energy unit that Thomson had been arguing from 1909.

[79] It is an ironic note that the major role of Thomson in the development of the quantum theory was as an unwitting producer of quantizable atomic models. Thomson's atomic mechanisms of 1904 and 1912 were made the basis of Nicholson's quantum theory of atoms, the earliest attempt (1912) by a British physicist to extend the quantum theory in new directions. In Europe, too, A. E. Haas and others imposed quantum conditions on Thomson's atomic models.

[80] Planck, Lorentz, and Sommerfeld all spoke out publicly against light quanta in 1910.

[81] A. L. Hughes, "On the Velocities of the Electrons produced by Ultra-Violet Light", *Proc. Camb. Phil. Soc.*, xvi (1911), 167-174.

Thomson intensified his attack on the quantum theory in his lectures at the Royal Institution in the spring of 1912, and in a paper which grew out of them bearing the somewhat misleading title, "The Unit Theory of Light".[82] He is referring to Planck's theory, though he had originally associated the term "unit" with his own.[83] His purpose was to show that it is the "mechanism" of exchange of thermal and radiant energy and not the radiant energy itself which has "an element of discontinuity in it".[84] He explained how absorption and emission give rise to the appearance of an atomic structure of light: when an electron is received or expelled by an atom, a pulse of light is emitted or absorbed, and the energy of the field is suddenly augmented or diminished by a finite amount characteristic of the atomic system. He interpreted the scattering of light as a temporary absorption and re-radiation by periodic systems within molecules. After repeated scatterings the energy of a light pulse will be "divided up into a great number of portions with probably no kind of equality between them".[85] Thomson granted that the energy of light "may not be distributed continuously", but it definitely does not appear in the form of "constant and invariable units".[86] Again at the British Association meeting in 1913 Thomson brought out a fresh atomic model to show that it was not necessary to assume that "radiant energy is molecular in structure".[87] All in all he proved himself a tireless, competent critic of light quanta. But this idea did not lack competent foes, and Thomson's critical activity was not his foremost contribution to modern physics.

Sometime before World War I Thomson's theory of light came to be spoken of as a precursor or interpretation of the light-quantum hypothesis rather than as an alternative in its own right.[88] Among those physicists who conceded that there was a serious question about the validity of the spherical-wave theory, there was fairly wide agreement that the challenge came from the direction of light quanta. It is a remarkable fact of the time that Thomson's was not one of many theories competing for acceptance. A variety of ideas on the nature of light was not in evidence, in contrast to the many on the nature of atoms. There are a number of reasons for this. First of all everyone was sharply conscious of the recent triumph of

[82] J. J. Thomson, "The Unit Theory of Light", *Proc. Camb. Phil. Soc.*, xvi (1912), 643-652.

[83] Thomson's "unit" may be a direct translation of "Elementarquantum", a term which figured prominently in discussions of Planck's work. It refers to any elementary constant, such as the charge of the electron.

So far as I know, Planck's theory was referred to as the "quantum theory" for the first time in Britain in 1912. This terminology occurs in J. W. Nicholson, *loc. cit.* (61).

[84] J. J. Thomson, *op. cit.* (82), 643.

[85] *Ibid.*, 650.

[86] *Ibid.*, 643.

[87] J. J. Thomson, "On the Structure of the Atom", *Phil. Mag.*, ser. 6, xxvi (1913), 792-799.

[88] For example, in 1913 Campbell said that Thomson's ideas on the structure of light were "notable as the only attempt that has been made to visualize the mechanism by which the energy of radiation is divisible into quanta of finite amount which can be absorbed almost instantaneously by a system on which they fall", N. R. Campbell, *Modern Electrical Theory*, 2nd ed. (Cambridge, 1913), 251.

Maxwell's theory of the field. Another factor is that a great deal more knowledge of an exact experimental sort existed for light than for atoms. One's imagination was relatively constrained in devising alternatives to the wave theory of light; atoms by contrast were all mystery. Further, any proposed structure needed to incorporate the properties of both particles and wave motion, and no one knew for certain how to put together a viable composite. Thomson's was an early attempt at a complete theory,[89] and it was a notable one. But it lacked the capability of the quantum theory, and it did not retain a following, at least a vocal one.

There have been few ideas in the history of physics which have preserved their shocking power longer than light quanta. Einstein did not exaggerate in 1905 when he described his paper on the energy characteristics of light as "very revolutionary",[90] and neither did the British physicist, S. B. McLaren, eight years later, when he remarked *à propos* of light quanta that the "spirit of revolution is seen at its boldest in the theory of radiation".[91] Only very gradually did a largely reluctant community of physicists become reconciled to light quanta.[92] The exact confirmation of Einstein's photoelectric equation around 1915, and the analysis of the Compton effect in 1923 in terms of collisions between free electrons and X-ray quanta, pointed to the reality of light quanta. It took two decades after Einstein's first paper on quanta before the optical objections to discrete radiation were resolved in the novel concept of probability waves.

The new developments of the quantum mechanical understanding of light remained an alien viewpoint to Thomson. He struggled to retain the essential role of the "mental picture" in a physics which increasingly rejected the pictorial character of its mathematical representations. The final chapter of his contribution to the physics of light is an anticlimactic, if not somewhat repetitious, aftermath of his optical researches up to 1910. Briefly in 1914,[93] and again in 1920,[94] he returned to the problem of the structure of free radiation. But it was not until around the middle of the 1920's that the subject became a central pre-occupation. In this

[89] Stark, too, at about the same time as Thomson, attempted to unify the wave and particle properties. He imagined light quanta to be capable of forming aggregates. In explaining partial reflection, he supposed that an aggregate of quanta undergoes division, each of the resulting halves increasing its porosity so as to take up the same volume as the original undivided aggregate. Stark's explanation of optical phenomena is discussed in Lorentz, *op. cit.* (59), 1250.

Also, around this time, Einstein was looking at the question from a different point of view. At the end of one of his papers on quanta he wrote down the wave equation of optics. This equation contains one constant, c, the velocity of light. He anticipated that the equation for the motion of quanta would have somewhat the same form. He supposed that the modified wave equation would contain e, the electronic charge, as well as c. A. Einstein, "Zum gegenwärtigen Stand des Strahlungsproblems", *Phys. Zeit.*, x (1909), 193.

[90] Einstein used this expression in a letter to his friend C. Habicht, quoted in C. Seelig, *Albert Einstein, A Documentary Biography*, trans. M. Savill (London, 1956), 74-75.

[91] S. B. McLaren, *op. cit.* (61), 43.

[92] In 1914 Hughes observed that the "prevalent" view then was that Einstein's hypothesis "gives the correct mathematical expression for the energy transformations, but gives no indication of the phenomena to which it is applied", A. L. Hughes, *Photoelectricity* (Cambridge, 1914), 6.

[93] J. J. Thomson, "Ionization", *Proc. Phys. Soc. Lond.*, xxvii (1914), 105-106.

[94] J. J. Thomson, "Mass, Energy and Radiation", *Phil. Mag.*, ser. 6, xxxix (1920), 679.

relatively late period in his career, roughly 1924-1936, he devoted a very considerable body of work to questions relating to the nature of light. His basic purpose was to demonstrate that Faraday's lines of force and Newtonian mechanics were sufficient to account for all of the results of the quantum theory of light.

A few samples will suggest the tenor of these last speculations. In all of them, he regarded a unit of light no longer as a lateral disturbance propagated along a tube of electric force, but rather as a closed, circular tube of force propagated at right angles to its plane—a view similar to the one put forward in the *Recent Researches*. In 1925[95] he proposed a dual structure of light : wave characteristics were represented by closed tubes which progressively expand as they propagate, while corpuscular or quantum features were said to arise from certain other closed tubes which retain their original size as they travel through space. Then in 1930[96] he suggested a relation between the structure of light and the structure of electrons. An electron, as he conceived it, consists of two parts, a core containing the charge, and an envelope of electronic waves determining the motion of the core. The waves possess energy and momentum, and they are capable of being detached by a sudden deceleration of the core. On this view the persisting wave system, which is associated with a closed loop of electric force, represents a light quantum, or a "disembodied" electron. Still another proposal[97] was to consider detached tubes of electric force as closed vortex filaments which are propagated through the ether. His last communication[98] on the subject of light was a letter to *Nature* in 1936, in which he proposed to account for optical interference by regarding a light quantum as a coaxial train of rings of electric force rather than as a single ring.

7. Conclusion

Thomson's theory of light was inconclusive, and its original impetus was diverted into an involvement with a criticism of the quantum theory. From the relative bulk of his writings on the subject one may suppose that the theory was never his central concern in the period up to 1910. His studies on the nature of light do not stand on the same level of achievement as his discoveries in cathode and canal rays, and in the electrical properties of gases. Initially these more successful studies did not have a greater potential significance, for the structure of the field is as fundamental as the structure of matter. But Thomson was only able to press his theory of

95 J. J. Thomson, "The Structure of Light", *Phil. Mag.*, ser. 6, l (1925), 1181-1196.

96 J. J. Thomson, "On the Relation of Electronic Waves to Light Quanta and to Planck's Law", *Phil. Mag.*, ser. 7, ix (1930), 1185-1194.

97 J. J. Thomson, "On Models of the Electric Field and of the Photon", *Phil. Mag.*, ser. 7, xvi (1933), 809-845.

98 J. J. Thomson, "The Nature of Light", *Nature*, cxxxvii (1936), 232-233. Thomson also discusses this letter in his autobiography, *Recollections and Reflections* (New York, 1937), 410.

light so far. He was held up chiefly because the predicted structure remained largely qualitative in theory[99] and undetectable in the laboratory. The great advantage of the tubes of force, namely, their pictorial character, was also their weakness. They could be kneaded into the ether in such a way as to fit almost any observations or preconceptions. In themselves they provided nothing firm on which to ground unambiguous, quantitative predictions—nothing equivalent to Planck's law which established the scale of the structure of light on the quantum theory. One revolution, Maxwell's, had been made in the physics of light by the use of Faraday's lines of force, and Thomson had set out to make another starting from the same idea. But the second revolution was abortive, and Lodge's vision, excited by the Silliman Lectures, of a new *Principia* founded upon Faraday's lines of force was unfulfilled.

Though Thomson did not push his ideas to a quantitative-predictive stage, they nevertheless motivated some significant experimental work in Cambridge. Writing of the recent history of the Cavendish Laboratory in 1909, Campbell remarked that an "altogether new group of ideas has begun to influence experimental research", referring to ideas on the discontinuous structure of light, and to those of Thomson in particular.[100] He noted that studies on the nature of light were only slightly represented in the Laboratory's publications, but he believed that they might soon become of major importance. A considerable amount of research was done, if not as much as Campbell anticipated, on elucidating the corpuscular properties of electromagnetic radiation. And much of it was undoubtedly stimulated by Thomson's lead.

His theory of light also had an importance apart from the associated experimental researches. In emphasizing the reality of the tubes of force, he strongly reinforced Faraday's legacy, one which has had a recurrent appeal to modern British physicists from Jeans[101] to Dirac.[102] Aside from his advocacy of a Faraday-type field, Thomson advanced two major

[99] Thomson did introduce some mathematical analysis into his theory in 1910 (and again from 1925 on). He calculated the energy and momentum in an elementary conical tube of force when the electron to which it is attached is set in motion. The results, however, did not really lead anywhere.

[100] N. R. Campbell, "1903-1909" in *A History of the Cavendish Laboratory 1871-1910* (London, 1910), 242.

[101] Jeans supposed that a "physical explanation of the quantum-theory might be based on the atomicity and possible discrete existence of tubes of force of strength $4\pi e$, ideas with which we have been made familiar in the writings of Sir J. J. Thomson. These ideas seem to many physicists to be at variance with experience, but they have to their credit that they give a natural and simple explanation of the electrokinetic momentum in the ether, such as I believe cannot be given by any other series of physical conceptions", J. H. Jeans, *Report on Radiation and the Quantum Theory*, 81.

[102] Dirac was attracted by the possibility of representing spatially discrete objects by Faraday's lines of force, exactly as Thomson had been. Dirac observed that "when we go over to quantum theory, we bring a kind of discreteness into our basic picture. We can suppose that the continuous distribution of Faraday lines of force that we have in the classical picture is replaced by just a few discrete lines of force with no lines of force between them", P. A. M. Dirac, "The Evolution of the Physicist's Picture of Nature", *Scientific American*, ccviii, No. 5 (May 1963), 51.

insights in regard to the physics of light. He argued that the interaction of light and matter implies, as optical phenomena do not, that light has a fine structure; and he understood that a theory of light needed to incorporate both wave and particle characteristics. Thomson's theory, expressed in the familiar language of Faraday tubes, helped prepare the way for the reception in Britain of the idea of field discontinuities. It was no accident that Campbell gave one of the first sympathetic discussions of Einstein's hypothesis of light quanta.[103] In creating an awareness that there was a problem of the structure of light, and that the field might be discontinuous, Thomson contributed to the twentieth-century revolution in the theory of light. It is in this sense that there is a measure of historical truth in his remark that the theory of light of the Silliman Lectures comprised a "small part of what was afterwards known as the Quantum Theory of Light".[104]

[103] N. R. Campbell, *Modern Electrical Theory*, 2nd ed. (Cambridge, 1913).
[104] J. J. Thomson, *Recollections and Reflections*, 410.