

Non-Einsteinian Interpretations of the Photoelectric Effect

Few, if any, ideas in the history of recent physics were greeted with skepticism as universal and prolonged as was Einstein's light quantum hypothesis.¹ The period of transition between rejection and acceptance spanned almost two decades and would undoubtedly have been longer if Arthur Compton's experiments had been less striking. Why was Einstein's hypothesis not accepted much earlier? Was there no experimental evidence that suggested, even demanded, Einstein's hypothesis? If so, why was it met with such profound skepticism?

Questions such as these, which relate to the processes by which scientific theories gain acceptance, are less popular with historians of science than they ought to be. A good deal of insight into the nature of science, especially into the interplay between theory and experiment, may be obtained from a study of these transition periods. This point may be illustrated immediately by briefly sketching Einstein's arguments for his light quantum hypothesis, its relationship to the photoelectric effect, and the experimental evidence on the photoelectric effect available to physicists in the early years of this century.

Einstein gave two powerful arguments for light quanta, one negative, the other positive. His negative argument was that classical radiation theory was fundamentally inadequate; it could not account for the ob-

served spectral distribution for black-body radiation, and it led inexorably to the blatantly false conclusion that the total radiant energy in a cavity is infinite. (This "ultra-violet catastrophe," as Ehrenfest later termed it, was pointed out independently and virtually simultaneously, though not as emphatically, by Lord Rayleigh.²)

Einstein developed his positive argument in two stages. First, he derived an expression for the entropy of black-body radiation in the Wien's law spectral region (the high-frequency, low-temperature region) and used this expression to evaluate the difference in entropy associated with a change in volume $v - v_0$ of the radiation, at constant energy. Secondly, he considered the case of n particles freely and independently moving around inside a given volume v_0 and determined the change in entropy of the system if all n particles accidentally found themselves in a given subvolume v at a randomly chosen instant of time. To evaluate this change in entropy, Einstein used the statistical version of the second law of thermodynamics in conjunction with his own strongly physical interpretation of the probability. He found that the expression for this change in entropy is formally identical to the one he had derived earlier for black-body radiation, and from this fact he drew what to him was the unavoidable conclusion: monochromatic black-body radiation in the Wien's law spectral region behaves with respect to thermal phenomena as if it consists of independently moving particles or quanta of radiant energy. It was also clear from his arguments that the energy of each quantum is proportional to its frequency.

Einstein's light quantum hypothesis (his "heuristic point of view") was therefore a necessary consequence of very fundamental assumptions: in no sense did he propose it in an ad hoc fashion to "explain" certain experiments. If, however, under certain circumstances light indeed exhibited a quantum structure, this ought to be experimentally verifiable. Einstein gave a detailed discussion of three experimental applications, one of which was the photoelectric effect. The famous equation he derived showed that the maximum kinetic energy T of the photoelectrons depends linearly on the frequency ν of the incident radiation. In modern notation, $T = h\nu - w_0$, where h is Planck's constant and w_0 is the work function of the photosensitive surface. (It is worth noting that Einstein himself used a combination of other constants instead of the single constant h , which merely emphasizes the fact that Einstein did not, as many believe, build

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¹ Über einen die Erzeugung und Verwandlung des Lichtes betreffenden heuristischen Gesichtspunkt," *Annalen der Physik*, 17 (1905), 132-148; translated in H. A. Boorse and L. Motz, eds., *The World of the Atom*, vol. 1 (New York: Basic Books, 1966), pp. 545-557. For a detailed historical discussion, see Martin J. Klein, "Einstein's First Paper on Quanta," *The Natural Philosopher* (New York), 2 (1963), 57-86; "Thermodynamics in Einstein's Thought," *Science*, 157 (1967), 509-516. My brief discussion of Einstein's paper draws heavily on Klein's work.

² "The Dynamical Theory of Gases and of Radiation," *Nature*, 72 (1905), 55.

on Planck's earlier work.) Einstein also pointed out that the intensity of the incident radiation should determine only the number and not the energy of the ejected photoelectrons, predictions which were consistent with Lenard's "trailblazing" 1902 experiments.³

But what about Einstein's extremely bold prediction that the maximum photoelectron energy depends linearly on the frequency of the incident radiation? Here the experimental situation was highly uncertain. Thus, Lenard had found only a general increase in electron energy with incident frequency; and several years later in 1907 Ladenburg concluded⁴ that the energy varied quadratically with the frequency. While Joffé was quick to point out⁵ that Ladenburg's experiments were also consistent with Einstein's linear relationship, it developed that they were also consistent with a frequency to the $2/3$ power variation proposed in 1911 by F. A. Lindemann.⁶ A year later in 1912 O. W. Richardson and K. T. Compton,⁷ as well as A. L. Hughes,⁸ reasserted the validity of the linear relationship. But their experiments were immediately challenged by Pohl and Pringsheim,⁹ who maintained that the best fit to Richardson and Compton's data could be obtained with a logarithmic relationship. It was not until 1915–16 that R. A. Millikan settled the issue.¹⁰

Now the remarkable fact is that these disparate experimental results, as we shall see, did not lead those who opposed Einstein's interpretation of the photoelectric effect to seriously question the validity of Einstein's linear relationship. Their opposition stemmed from a different quarter. They failed to see that Einstein's light quantum hypothesis was a necessary consequence of Einstein's assumptions, and they tended to take ac-

³ P. Lenard, "Ueber die lichtelektrische Wirkung," *Annalen der Physik*, 8 (1902), 149–198.

⁴ E. Ladenburg, "Über Anfangsgeschwindigkeit und Menge der photoelektrischen Elektronen in ihrem Zusammenhange mit der Wellenlänge der Auslösenden Lichtes," *Berichte der deutschen physikalischen Gesellschaft*, 9 (1907), 504–515; *Physikalische Zeitschrift*, 8 (1907), 590–594.

⁵ A. Joffé, "Eine Bemerkung zu der Arbeit von E. Ladenburg," *Bayrische Akademie der Wissenschaften, München, Sitzungsberichte, mathematisch-physikalische Klasse*, 37 (1907), 279–280; *Annalen der Physik*, 24 (1907), 939–940.

⁶ "Über die Berechnung der Eigenfrequenzen der Elektronen im selektiven Photoeffect," *Berichte der deutschen physikalischen Gesellschaft*, 13 (1911), 482–488.

⁷ "The Photoelectric Effect," *Philosophical Magazine*, 24 (1912), 575–594.

⁸ "On the Long-Wave Limits of the Normal Photoelectric Effect," *Philosophical Magazine*, 27 (1914), 473–475.

⁹ "On the Long-Wave Limits of the Normal Photoelectric Effect," *Philosophical Magazine*, 26 (1913), 1017–1024.

¹⁰ "A Direct Photoelectric Determination of Planck's 'h,'" *Physical Review*, 7 (1916), 355–388.

ceptance of quanta to imply rejection of classical electrodynamics—this in spite of Einstein's 1909 proof that a necessary consequence of Planck's law is the coexistence of quanta and waves in black-body radiation.¹¹ (Einstein's proof rested on his analysis of energy and momentum fluctuations in black-body radiation, a proof his contemporaries were unprepared to accept or even understand.) Maxwell's theory had been repeatedly confirmed, by Hertz's electromagnetic wave experiments, Lebedev's (and Nichols and Hull's) radiation pressure experiments, and all interference and diffraction experiments.¹² The heuristic value of Maxwell's theory was beyond question. To believe that it would not eventually encompass photoelectric phenomena appeared to most of Einstein's contemporaries—the exception was Johannes Stark¹³—to be most unreasonable.

Given the belief (to a large degree unjustified by the experimental evidence, as we have seen) in the validity of the linear relationship between energy and frequency, as well as the disbelief in Einstein's interpretation of it, a natural question arose: Were there perhaps other, less radical, interpretations of the photoelectric effect? A historical examination of the answers proposed to this question will form the main thrust of my paper, but before I begin I should like to make two general remarks. First, we shall evidently be discussing theories formulated not primarily in response to a troublesome experimental observation,¹⁴ but rather in response to an unpalatable interpretation. Second, while revolutionary new theories tend to be generated by young minds, and while it is therefore natural to attribute any resistance to them to the "old guard," we do not seem to have such a situation here. We do not seem to have an example of Planck's contention that "a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with

¹¹ "Zum gegenwärtigen Stand des Strahlungsproblems," *Physikalische Zeitschrift*, 10 (1909), 185–193; "Über die Entwicklung unserer Anschauungen über das Wesen und die Konstitution der Strahlung," *Physikalische Zeitschrift*, 10 (1909), 817–826. See also Martin J. Klein, "Einstein and the Wave-Particle Duality," *The Natural Philosopher*, 3 (1963), 3–49.

¹² For discussion and references, see E. T. Whittaker, *A History of the Theories of Aether and Electricity*, vol. 1 (New York: Harper, 1960), pp. 275, 319ff.

¹³ "Zur experimentellen Entscheidung Zwischen Ätherwellen und Lichtquantenhypothese," *Physikalische Zeitschrift*, 10 (1909), 902–913. See also 11 (1910), 24–25, 179–187.

¹⁴ For such examples, see T. S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962), pp. 52–90.

it.”¹⁵ We do not seem to have this situation because the opponents in this case were H. A. Lorentz, J. J. Thomson, Arnold Sommerfeld, and O. W. Richardson, ages 57, 54, 42, and 31, respectively, in 1910. Needless to say, apart from the great spread in ages, the most noteworthy characteristic of these physicists is their eminence.

II

Correspondence deposited in the Einstein Archives in Princeton shows that between 1909 and 1911 Einstein used H. A. Lorentz as a sounding board for his developing ideas on the nature of radiation. Thus by the time Lorentz was invited by the Wolfskehl Commission to deliver six lectures at Göttingen in October 1910, he was completely familiar with Einstein’s work. In his fifth lecture, Lorentz treated Einstein’s interpretation of the photoelectric effect in detail, and, notwithstanding our earlier remarks, maintained that the predicted linear relationship between energy and frequency was “confirmed by experiment.” Lorentz then told his audience that “in spite of [this evidence] the speaker holds the light quantum hypothesis to be impossible, if the quanta are regarded as completely incoherent, an assumption which is the most natural one to make.”¹⁶ The interference and diffraction difficulties appeared insurmountable to Lorentz, the physicist who (to quote Ehrenfest) had brought clarity to the “kind of intellectual jungle”¹⁷ that was Maxwell’s *Treatise*.

Fortunately, Lorentz noted, there was another possible explanation of the photoelectric effect, namely, Haas’s. According to Lorentz, Haas imagined a light wave to be incident on an electron in a Thomson atom, thereby setting the electron into oscillation. As long as the electron remained inside the positive sphere, the incident wave would only be scattered or dispersed; if, however, the electron received more than some threshold energy, it would be ejected from the positive sphere and a discrete amount of energy would be simultaneously abstracted from the incident wave. Planck’s constant entered into Haas’s theory by assigning the quantum energy $h\nu$ to the electron at the boundary of the positive sphere, and Lorentz noted with satisfaction that application of Haas’s theory to a gas, Argon, yielded order of magnitude agreement with Planck’s own value.

¹⁵ *Scientific Autobiography*, trans. Frank Gaynor (New York: Philosophical Library, 1949), pp. 33–34.

¹⁶ “Alte und neue Fragen der Physik,” *Physikalische Zeitschrift*, 11 (1910), 1249.

¹⁷ Quoted in G. L. de Haas-Lorentz, ed., *H. A. Lorentz: Impressions of His Life and Work* (Amsterdam: North-Holland, 1957), p. 55.

Lorentz concluded: “In Haas’s hypothesis, the riddle of the energy elements is combined with the question of the nature and action of positive electricity, and it may be that these different questions will, for the first time, together find their complete solution.”¹⁸

The crux of Lorentz’s argument was that it was far more reasonable to assume that the photoelectric effect would become understandable on the basis of atomic structure considerations than to believe it entailed abandoning Maxwell’s theory. I should like to urge the reasonableness and naturalness of Lorentz’s point of view. It was reasonable because in 1910 little of a definite nature was known about the dynamics of the atom; it was natural because it was not new—already in 1902 Lenard had proposed that the incident electromagnetic radiation stimulated the emission of electrons by “triggering” atomic disruptions.¹⁹ To a certain degree, therefore, Einstein’s interpretation must have appeared radical on two counts: not only did he explicitly reject a Maxwellian approach; he also implicitly rejected Lenard’s approach. Since not only Lorentz but J. J. Thomson also embraced Lenard’s approach in 1910, Einstein’s interpretation might be viewed as a temporary detour from an established route.

III

J. J. Thomson was one of the great atom builders of all time, the traditional “Thomson Atom” being only one of a rather formidable collection of models. As his biographer Lord Rayleigh remarked: “J. J. was not inclined to be dogmatic about his atomic theories, and indeed he was quite prepared to change them, sometimes without making it altogether clear that he had wiped the slate clean.”²⁰ Believing that “the chief value of any theory was as a basis for further experiments,”²¹ Thomson concentrated on what a theory would explain and not on what it would not. This attitude led him to proliferate atomic models—J. G. Crowther termed them “semi-manufactured scientific goods”²²—and it is only by understanding Thomson’s attitude toward theory construction that we can understand how he could propose two completely different theories of the photoelec-

¹⁸ “Alte und neue Fragen,” p. 1253.

¹⁹ “Ueber die lichtelektrische Wirkung,” pp. 169–170.

²⁰ *The Life of Sir J. J. Thomson, O.M.* (Cambridge: Cambridge University Press, 1943), p. 141.

²¹ *Ibid.*; see also p. 136.

²² *British Scientists of the Twentieth Century* (London: Routledge and Kegan Paul, 1952), p. 12.

tric effect between 1910 and 1913 based on two completely different atomic models.

Thomson published his first theory²³ a few months before Lorentz delivered his Wolfskehl Lectures. Thomson postulated that in the atom there are electric doublets, each with an electron (“corpuscle”) circling around it below its positive pole. Proceeding directly from the electron’s equations of motion, Thomson proved that the electron’s kinetic energy is proportional to its frequency of revolution and that it rotates on the surface of a cone of half-angle 55° ($\tan^{-1}\sqrt{2}$) with apex at the center of the doublet. Since, however, the electron’s distance from the apex is not fixed, and since its frequency of revolution depends on this distance, there will be electrons of many different kinetic energies present in a metal plate containing many atoms. That was all Thomson required, for now an incident wave of given frequency would certainly find an electron rotating at the same frequency, so that resonance would occur. According to Thomson, this would twist the electric doublet, and if it were twisted enough, it would cast off its electron. Since at the time of ejection, the electron’s kinetic energy would be proportional to its frequency of rotation, it would by the principle of resonance also be proportional to the frequency of the incident radiation. From an argument involving Wien’s displacement law, Thomson was able to infer a value for the electric moment of the doublet, which in turn enabled him to calculate the constant of proportionality—which turned out to be of the same order of magnitude as Planck’s constant h ! To Thomson, the implication was perfectly clear: he wrote that “we cannot regard Ladenburg’s experiments as proof of the unitary structure of light. . . . [My] theory enables us to explain the electrical effects produced by light, without assuming that light is made up of unalterable units, each containing a definite and, on Planck’s hypothesis, a comparatively large amount of energy, a view which it is exceedingly difficult to reconcile with well-known optical phenomena.”²⁴

J. H. Jeans criticized²⁵ Thomson’s doublet-electrons on the grounds that they are dynamically unstable, but Thomson answered²⁶ that their long-term stability was unnecessary for the success of his theory. Perhaps, however, Thomson did not entirely forget Jeans’s criticism, because three

²³ “On the Theory of Radiation,” *Philosophical Magazine*, 20 (1910), 238–247.

²⁴ *Ibid.*, p. 246.

²⁵ “On the Motion of a Particle about a Doublet,” *Philosophical Magazine*, 20 (1910), 380–382.

²⁶ Letter to Editors of *Philosophical Magazine*, 20 (1910), 544.

years later in 1913 Thomson proposed a second theory²⁷ based on an entirely new atomic model. In his new model, Thomson postulated the co-existence of two forces: a radial inverse cube repulsive force “diffused throughout the whole of the atom,” and a radial inverse square attractive force “confined to a limited number of radial tubes in the atom.”²⁸ Thus, inside such a radial tube both forces would be present, and by setting up the equation of motion of an electron in it, Thomson readily demonstrated that the electron could oscillate about an equilibrium position with a frequency depending on the force constant of the repulsive force. Once again, that was all Thomson required, for now an incident wave would certainly find an electron with which it could resonate, and if, after being set into oscillation, some “casual magnetic force” moved it laterally out of the tube, it would come under the “uncontrolled action” of the repulsive force and be expelled from the atom. Thomson proved that in leaving the atom the electron (charge e , mass m) would pick up energy $T = \pi \cdot \sqrt{Cem} \nu$. The quantity C is the repulsive force constant, which Thomson, in a completely ad hoc manner, fixed at the value $h^2/mc\pi^2$, so that by substitution $T = h\nu$. “Thus,” Thomson wrote, “we see that the kinetic energy with which the corpuscle is expelled is proportional to the frequency of the light and is equal to the frequency multiplied by Planck’s constant.” “This,” he concluded, “is the well-known law of Photo-Electricity.”²⁹

While Thomson’s theories attracted some attention and stimulated some experimental work, particularly at the Cavendish Laboratory, the ad hoc character of many of Thomson’s basic assumptions must have been apparent to many of his contemporaries. Indeed, W. H. Bragg at the University of Leeds regarded certain features of Thomson’s first theory as “fantastic”;³⁰ in general, Bragg considered attempts like Thomson’s to squeeze quantum manifestations out of classical theories to be retrogressive. Thomson’s second theory actually took on an even more contrived

²⁷ “On the Structure of the Atom,” *Philosophical Magazine*, 26 (1913), 792–799.

²⁸ *Ibid.*, p. 793.

²⁹ *Ibid.*, p. 795.

³⁰ Letter to A. Sommerfeld dated February 7, 1911, on deposit in the Archive for History of Quantum Physics, American Philosophical Society Library, Philadelphia; depositories also in Copenhagen and Berkeley. For more on Thomson’s work, see Russell McCormach, “J. J. Thomson and the Structure of Light,” *British Journal for the History of Science*, 3 (1967), 362–387. John L. Heilbron and Thomas S. Kuhn discuss the influence of Thomson’s first theory on Niels Bohr in “The Genesis of the Bohr Atom,” in *Historical Studies in the Physical Sciences*, vol. 1, ed. Russell McCormach (Philadelphia: University of Pennsylvania Press, 1969), pp. 226–229.

cast a few weeks after he proposed it when he found it necessary to replace the radial attractive cones with attractive cylinders!³¹ But while Thomson's theories (if indeed, this term is fully appropriate, considering their ad hoc features) rested on insecure grounds, a basic point Thomson was making should not be lost. Like Lorentz, Thomson showed it was possible to envision atomic models upon which a quantitative theory of the photoelectric effect could be based. This very same point was made in 1911 in a more sophisticated way by Arnold Sommerfeld.

IV

Sommerfeld's motivation and method form an interesting contrast to Thomson's. Whereas Thomson was basically unsympathetic toward the new quantum theory, Sommerfeld was convinced (as he wrote in 1913) that "Planck's discovery of the universal quantum of action has been called to heal the momentary sufferings of theoretical physics."³² Thomson, in his second theory, had introduced Planck's constant at the end of his calculation in a strictly ad hoc manner; Sommerfeld introduced it at the outset by postulation. He contended that "an electromagnetic or mechanical 'explanation' of [Planck's constant] h seems to me to be just as unnecessary and unpromising as a mechanical 'explanation' of Maxwell's equations. It would be much more useful to pursue the h -hypothesis in its various consequences and trace other phenomena back to it."³³ One of these phenomena was the photoelectric effect, concerning which Sommerfeld wrote Einstein was "at present [1911] not able to maintain his completely audacious point of view."³⁴

Sommerfeld postulated that in "every purely molecular process a definite and universal amount of action"³⁵ is taken up or given up. He introduced Planck's constant by fixing the exact amount by the condition

$$\int_0^{\tau} (T - V) dt = \frac{h}{2\pi}$$

where T and V are the relevant kinetic and potential energies, and τ is the time during which the process takes place. Sommerfeld applied this con-

³¹ Letter to Editors of *Philosophical Magazine*, 26 (1913), 1044.

³² "Theorie des lichtelektrischen Effektes vom Standpunkt des Wirkungsquantums," *Annalen der Physik*, 41 (1913), 873.

³³ "Das Plancksche Wirkungsquantum und seine allegemeine Bedeutung für die Molekularphysik," *Berichte der Deutschen Physikalischen Gesellschaft*, 9 (1911), 1092.

³⁴ *Ibid.*, p. 1074.

³⁵ *Ibid.*, p. 108.

dition to the photoelectric effect as follows. Assume that the incident radiation—electromagnetic waves—sets a bound electron into oscillation by resonance, and that after a certain time τ the electron's energy becomes large enough for it to be released from the atom. (Sommerfeld later³⁶ estimated τ to be on the order of 10^{-5} second, a small, and in 1911, unobservable, delay, but nonetheless one that is inconsistent with Einstein's interpretation.) Substituting the electron's kinetic and potential energies into the above condition, transforming the result by partial integration, and introducing the electron's equation of motion, Sommerfeld proved that $T = h\nu_0$, where ν_0 is the natural frequency of the electron in the atom. At resonance this frequency is equal to the frequency of the radiation, ν , so that $T = h\nu$; and this, concluded Sommerfeld, is "Einstein's Law."

(Neither Thomson nor Sommerfeld was ever particularly concerned that neither of their photoelectric effect equations, in contrast to Einstein's, contained an additive constant, namely, the work function. Sommerfeld expressed the reason for his lack of concern as follows: "This [the work function] is obviously foreign to the pure molecular process and would only appear if we were to follow the photoelectron [after its ejection from the atom] along on its subsequent path through the surface of the metal."³⁷ Thomson no doubt wholeheartedly endorsed this statement.)

Sommerfeld drew attention to both a similarity and a difference between his theory and Einstein's. The theories were similar in that both predicted that as the electron was ejected from the atom, a discrete amount of energy would be abstracted from the incident radiation. The theories were different in that Sommerfeld, unlike Einstein, but like Thomson, envisioned a resonance phenomenon. This difference had observable consequences. Whereas Einstein's equation predicted that the electron's energy should be entirely independent of any atomic frequencies, Sommerfeld's theory predicted that a plot of energy versus frequency "should possess, for each natural frequency of the atom, a maximum." (Had Thomson not concentrated only on the case of complete resonance, he would have been driven to the same conclusion.) Which theory fit the existing data better, Sommerfeld's or Einstein's? Sommerfeld had the answer. J. R. Wright in Millikan's laboratory at the University of Chicago had recently shone ultraviolet light on aluminum and had demonstrated "with certainty . . . that the maximum photoelectron energy does not

³⁶ "Theorie des lichtelektrischen Effektes," p. 885.

³⁷ *Ibid.*

vary approximately linearly with the frequency." Furthermore, Wright had found evidence that the photoelectric effect depends on the plane of polarization of the incident radiation. "With respect to both points," Sommerfeld concluded, "our theory is in better accord with Wright's measurements than Einstein's light quantum theory."³⁸

Sommerfeld, who first proposed his theory in mid-1911, soon had a chance to discuss it—actually, a refined version of it—personally with Einstein at the Solvay Conference in Brussels. From Einstein's comments, which are recorded in the proceedings of the conference,³⁹ it appears that he was at best lukewarm toward Sommerfeld's theory. More positive reactions, however, came from other conferees, notably Planck, Lorentz, and Brillouin.⁴⁰ At any rate, Sommerfeld was in general apparently encouraged by the reception accorded his theory—Max Born later termed it his "wild adventure"⁴¹—because, aided by his former student Peter Debye, he continued to refine it, eventually publishing a 58-page paper on it⁴² in the *Annalen der Physik* of 1913.

v

The theories of Lorentz, Thomson, and Sommerfeld shared one salient feature: each rested on a definite hypothesis on the structure of the atom. Consequently, the acceptance of Bohr's model in the years following 1913 forced their abandonment, which, incidentally, illustrates rather nicely how an advance in one area of science generally has considerable ramifications in a related area. No one today would be tempted to advocate the theories of the photoelectric effect we have examined so far.

Of a very different character is the theory O. W. Richardson began developing at the end of 1911.⁴³ Since I believe Richardson's theory merits study even today, the question arises why only historians have heard of it. The answer is, I think, that Richardson's approach, which he felt possessed a great advantage, strikes us now as possessing a great disadvantage. For Richardson adopted not a microscopic, but a macroscopic, approach. As he wrote: "For the present, I wish to avoid discussion of the vexed ques-

³⁸ "Das Plancksche Wirkungsquantum," pp. 1090–91.

³⁹ P. Langevin and M. de Broglie, eds., *La Théorie de Rayonnement et les Quantas* (Paris, 1912), pp. 373–392, especially pp. 373, 381–382, 384, 390.

⁴⁰ *Ibid.*, pp. 390–392, 451.

⁴¹ "Arnold Johannes Wilhelm Sommerfeld," *Biographical Memoirs of Fellows of the Royal Society*, 8 (1952), 283.

⁴² "Theorie des lichtelektrischen Effektes," pp. 873–930.

⁴³ "Some Applications of the Electron Theory of Matter," *Philosophical Magazine*, 23 (1912), 594–627.

tion of the nature of the interaction between the material parts of the system and the aethereal radiation and to confine my remarks to the conclusions which may be drawn from the existence of a statistically steady condition of the aethereal and electronic radiations."⁴⁴ Thus, Richardson would adopt the methods of thermodynamics, which explicitly avoids hypotheses on the micro-world.

In the photoelectric effect, radiation incident on a metal plate causes the emission of electrons from it, and ordinarily one does not worry about the possibility of any electrons returning to the plate and being absorbed by it. Richardson, however, did worry about these returning electrons: he set as his goal the determination of the equation describing equilibrium between the rates of electron emission and absorption. To analyze this problem, Richardson carried out a thought experiment using the most common of all thermodynamic thought apparatus—a piston in a cylinder. He assumed that the only photosensitive surface in the system was the bottom of the cylinder; further, he assumed the piston to be both completely impervious to electrons and completely transparent to radiation. If, now, the cylinder is filled with radiation and the piston, initially in contact with the bottom of the cylinder, is slowly raised, the radiation will pass through the piston from above and eject electrons from the bottom of the cylinder, thereby causing the space below the piston to fill with electrons. By slowly moving the piston up and down, the electrons could be pumped in and out of the bottom of the cylinder in a reversible manner. It was this process that Richardson analyzed in detail thermodynamically—a later treatment⁴⁵ involved the direct application of the Clausius-Clapeyron equation to it, considering the electron emission to be analogous to the evaporation of a monatomic gas.

Richardson of course had to specify the spectral distribution function of the radiation in the cylinder, and in his first treatment he used Wien's distribution. Later, in a more refined version of his theory,⁴⁶ he used Planck's distribution, but this change affected only the precise form of the electron energy distribution and not any of his basic conclusions. Assuming Wien's distribution, Richardson derived an integral equilibrium equation for N , the rate of electron emission as a function of frequency. By

⁴⁴ *Ibid.*, p. 617.

⁴⁵ A. L. Hughes and L. A. DuBridge, *Photoelectric Phenomena* (New York: McGraw-Hill, 1932), pp. 196–199.

⁴⁶ "The Theory of Photoelectric and Photochemical Action," *Philosophical Magazine*, 27 (1914), 476–488.

direct substitution, Richardson easily proved that the equation was satisfied by

$$N = \begin{cases} 0 & 0 < h\nu \leq w_0 \\ \frac{B}{\nu^3} \left(1 - \frac{w_0}{h\nu}\right) & w_0 < h\nu < \infty \end{cases}$$

where B is a constant and w_0 , in Richardson's interpretation, is the latent heat of evaporation per electron. We see that for radiant energies $h\nu \leq w_0$, no electrons should be emitted from the photosensitive surface, while for radiant energies $h\nu > w_0$, a continuous emission should occur. Furthermore, from his equilibrium equation for N, Richardson immediately derived a second equation for T, the kinetic energy of the emitted electrons. This equation had the solution:

$$T = \begin{cases} \text{meaningless} & h\nu \leq w_0 \\ h\nu - w_0 & w_0 < h\nu < \infty \end{cases}$$

It is obvious that these results are formally identical to Einstein's, and Richardson was fully aware of this significance of his theory. The equations above, he wrote, have "been derived without making use of the hypothesis that free radiant energy exists in the form of 'Licht-quanten,' unless this hypothesis implicitly underlies the assumptions:—(A) that Planck's radiation formula is true; (B) that, *ceteris paribus*, the number of electrons emitted is proportional to the intensity of monochromatic radiation. Planck . . . has recently shown that the unitary view of the structure of light is not necessary to account for (A) and it has not yet been shown to be necessary to account for (B). It appears therefore that the confirmation of the above equation . . . by experiment would not necessarily involve the acceptance of the unitary theory of light."⁴⁷

Einstein, in view of his 1909 insights into the wave-particle duality which were mentioned earlier, would of course have contended that his light quantum hypothesis was, indeed, a necessary consequence of the validity of Planck's law. Furthermore, he would also have pointed out that his light quantum hypothesis simply and naturally accounted for the fact that the number of electrons emitted is proportional to the intensity of the incident radiation. That Planck's law could be *derived* from other, weaker, assumptions in no way affects the validity of the necessary consequences of that law.

⁴⁷ "The Theory of Photoelectric Action," *Philosophical Magazine*, 24 (1912), 574.

Richardson, however, did not appreciate the significance of this point and concluded that his derivation undercut Einstein's light quantum hypothesis. To Richardson this was an extremely important conclusion, because concurrently with his theoretical work, he and his former student Karl T. Compton were carrying out experiments on the photoelectric effect.⁴⁸ These experiments were unquestionably the most accurate and refined to date, and as I noted at the beginning of my paper, Richardson and Compton concluded in favor of the linear relationship between energy and frequency. Notwithstanding his theoretical work, this came as a surprise to Richardson: "when these experiments were started," he later wrote, "I thought it improbable that the equation would turn out to be correct, on account of the very grave objections to the form of quantum theory on which it had up to that time been based by Einstein." Fortunately, he continued, this equation "evidently has a wider basis than the restricted and doubtful hypothesis used by Einstein."⁴⁹ Indeed, his and Compton's experiments, he concluded, could be taken to "confirm the theory of the photoelectric action which was recently developed by one of the writers."⁵⁰

It is no doubt unfair to generalize at this point and conclude that whenever a scientist has a choice between a conservative and a radical theory he will choose the conservative one (especially if he himself has developed it!) but that was clearly the case here. Actually, of course, the choice was not so much between a conservative and a radical theory, as it was between a desire to avoid hypotheses on the micro-world and a lack of that desire. But just because Richardson had succeeded in deriving Einstein's equation without explicitly invoking Einstein's light quantum hypothesis, the validity of that equation was not automatically established. The experimental issue was by no means settled. As a matter of fact, it was in response to Richardson and Compton's *experimental* work that Pohl and Pringsheim proposed the logarithmic relationship between energy and frequency that I mentioned at the beginning of my paper. Richardson and Compton's work was open to two major criticisms: first, their experiments had been carried out over a very restricted range of frequencies, so that a number of different functional relationships fit their data reasonably well; sec-

⁴⁸ "The Photoelectric Effect," *Philosophical Magazine*, 24 (1912), 575–594.

⁴⁹ "The Photo-electric Action of X-Rays," *Proceedings of the Royal Society*, 94 (1918), 269.

⁵⁰ Karl T. Compton and O. W. Richardson, "The Photoelectric Effect—II," *Philosophical Magazine*, 26 (1913), 550.

ond, there was, in Richardson's words, an experimental error of an "insidious [sic] nature"⁵¹ in their work, an error that led Richardson and Compton to put much more faith in the average electron energies they measured than in the *maximum* electron energies. It is, of course, the *maximum* energies that are the important ones theoretically.

All experimental difficulties were not overcome until R. A. Millikan completed his painstaking work in 1915–16. In the course of years of research, he eventually constructed what he called a "machine shop in vacuo"⁵² to make his photoelectric measurements. The accuracy with which Millikan's data points fell on the predicted straight line was truly remarkable. Einstein's light quantum hypothesis was therefore finally vindicated. Or was it? Concerning the theoretical interpretation of his experiments, Millikan suggested that in the atom there were electrons "in all stages of energy loading up to the value $h\nu$," so that the incident radiation merely triggered an "explosive emission."⁵³ And in 1917, in an entirely fascinating passage, Millikan carefully distinguished between Einstein's equation and Einstein's theory. He wrote:

Despite . . . the apparently complete success of the Einstein equation, the physical theory of which it was designed to be the symbolic expression is found so untenable that Einstein himself, I believe, no longer holds to it, and we are in the position of having built a very perfect structure and then knocked out entirely the underpinning without causing the building to fall. It stands complete and apparently well tested, but without any visible means of support. These supports must obviously exist, and the most fascinating problem of modern physics is to find them. Experiment has outrun theory, or, better, *guided by erroneous theory* [my italics], it has discovered relationships which seem to be of the greatest interest and importance, but the reasons for them are as yet not at all understood.⁵⁴

When the long-awaited experimental confirmation came, it was not accepted as such! The fact that Lorentz, Thomson, Sommerfeld, and Richardson had been in varying degrees successful in developing alternate interpretations of the photoelectric effect goes a long way in accounting for this remarkable situation. There were of course other factors, perhaps the most significant being von Laue's very striking 1912 discovery of the crystal diffraction of x-rays,⁵⁵ which, after many years, seemed to conclu-

sively demonstrate their wave nature. Curiously enough, the discovery of the Bohr Atom in 1913,⁵⁶ with its transitions between discrete energy levels, seems to have done little to alleviate the skepticism toward Einstein's light quantum hypothesis, though this may be due in large measure to the fact that Bohr himself was so strongly opposed to light quanta. At any rate, the skepticism prevailed until roughly 1924, until after Arthur Compton's famous interpretation of his beautiful x-ray scattering experiments⁵⁷ had been accepted. Compton's discovery possessed huge shock value and finally forced physicists to cope seriously with Einstein's hypothesis. Eventually, the photon (to use G. N. Lewis's lasting term⁵⁸) became accepted into physics as a particle of light possessing both energy and momentum.

VI

I should like to conclude with a postscript. Most of what I have just related I was familiar with when, in the fall of 1968, I met Professor Peter Franken at the Sommerfeld Conference in Munich and learned from him that it is possible, even today, to account for the main features of the photoelectric effect along non-Einsteinian lines, that is, without assuming light quanta or photons to be incident on the atom. In brief, Franken has constructed his theory⁵⁹ from a well-established quantum mechanical base, time-dependent perturbation theory. An atom in its ground state, described by Schrödinger's equation, is assumed to be subjected to a *classical electromagnetic wave* of frequency ν , which perturbs the state of the atom. Assuming that the incident wave has enough energy to bring about a transition of an electron to the continuum, time-dependent perturbation theory shows that only a definite level in the continuum will be excited. This level is of energy $h\nu$ above the ground state, so that if w_0 is the ionization potential of the atom, the level corresponds to a free electron of kinetic energy $T = h\nu - w_0$, which is Einstein's equation. Two things are crucial to observe: first, Planck's constant h is introduced not by assumption but by Schrödinger's equation as applied to the atom; second, the frequency ν is the frequency of the perturbing *electromagnetic wave*.

schaften, München, Sitzungberichte, mathematisch-physikalische Klasse, 42 (1912), 303 (with W. Friedrich and P. Knipping).

⁵¹ "On the Constitution of Atoms and Molecules," *Philosophical Magazine*, 26 (1913), 1–25, 476–502, 857–875.

⁵² "A Quantum Theory of the Scattering of X Rays by Light Elements," *Physical Review*, 21 (1923), 207, 483–502.

⁵³ "The Conservation of Photons," *Nature*, 118 (1926), 874–875.

⁵⁴ Private communication.

⁵¹ *Ibid.*, p. 567.

⁵² "A Direct Photoelectric Determination of Planck's 'h,'" p. 361.

⁵³ *Ibid.*, pp. 387–388.

⁵⁴ *The Electron* (Chicago: University of Chicago Press, 1917), p. 230.

⁵⁵ "Interferenzerscheinungen bei Röntgenstrahlen," *Bayrische Akademie der Wissen*

Franken also points out that in addition to the linear relationship, two other consequences follow immediately. First, "Fermi's Golden Rule" shows that the *rate* of electron emission is proportional to the square of the perturbation matrix, and since the perturbation matrix is proportional to the amplitude of the perturbing wave, we have immediately that the rate of electron emission is proportional to the *intensity* of the incident radiation. Second, since the rate is established as soon as the perturbation is "turned on," there should be no systematic time delay between the time of electron emission and the time when the radiation is incident. Thus, all known aspects of the photoelectric effect may be explained *without assuming photons to be incident on the atom*. I should point out that after Franken called my attention to this rather striking conclusion, I found similar derivations in other sources, for example, in Eugen Merzbacher's *Quantum Mechanics*⁶⁰ and in J. R. Oppenheimer's 1939 Lecture Notes on *Quantum Mechanics*.⁶¹ Neither Merzbacher nor Oppenheimer, however, draws attention to the point emphasized by Franken.

Franken fully recognizes, it should be noted, that photons may be produced, as for example in atomic transitions. His claim is only that it is possible to interpret the photoelectric effect (and other quantum electro-dynamical phenomena) without introducing the photon concept at the outset—without assuming that photons are *incident* on the atom. Thus, he would regard as highly misleading, or even erroneous, statements like that of S. Tolansky, who recently wrote that there "is absolutely no possible way of accounting for photoelectric effects . . . except by adopting the idea of the photon as a sort of particle carrying its full energy and traveling with the velocity of light."⁶² Perhaps one might ask the following question: If the photoelectric effect were the only relevant experiment available to us, would we be justified in concluding from it that radiation consists of quanta? In view of our earlier remarks, it would seem that we would have to give a negative answer to this question.

But this in no way diminishes the force of Einstein's original arguments for light quanta: Planck's law entails, as a necessary consequence, the light quantum hypothesis. That one experiment—the photoelectric effect—may be explained on weaker or different assumptions in no way affects Einstein's basic arguments. And certainly nothing is more artificial than con-

centrating on one experiment to the exclusion of all other phenomena: the photon concept, like all fruitful concepts in physics, derives its validity from an interlocking theoretical and experimental matrix. The main purpose of this paper has been to display some of the difficulties that were involved in recognizing and understanding only one of the elements in this matrix.*

* EDITOR'S NOTE: Dr. Buchdahl's comments on this paper are combined with his comments on Professor Stein's paper, following the latter.

⁶⁰ New York: Wiley, 1961, pp. 459–460.

⁶¹ Notes taken by B. Peters. I should like to express my gratitude to Professor Morton Hamermesh for giving me a copy of these notes.

⁶² *Revolution in Optics* (Harmondsworth: Penguin, 1968), p. 40.