Patent No. 568 904); Fermi, exponential pile (US Patent No. 2 780 595); Fermi–Szilard, neutronic reactor (US Patent No. 2 807 581).
3. L. Halpern, "Marietta Blau," Tallahassee, Florida State University (1992), p. 18.
4. Letter from G. Stetter to dean of philosophy faculty, 17 October 1945, Personalakte Stetter, Bundesministerium für Unterricht, Osterreiches Staatsarchiv, Archiv der Republic, Vienna, fols. 215–17, on fol. 216.

Peter Galison
Harvard University
Cambridge, Massachusetts

Dispute about Electron Discovery Resumes—and Clearly Hurts Some

My earlier reluctance to enter the debate in your pages on the discovery of the electron (Allan Franklin, "Are There Really Electrons? Experiment and Reality," October 1997, page 26) was overcome after reading the letters to the editor from George Trilling and Max Lazarus (February 1998, page 13).

All but one of the protagonists in the discovery received Nobel Prizes for their research (Heinrich Hertz died at the age of almost 37 from a bacterial infection), so there is no need to question their vital contributions. Rather, it is more useful to attempt to appreciate their intellectual environment and to recall pertinent comments that can shed light on the evolving reality, so that each of us can make a personal judgment call on the issue of proper credit. This letter is intended to help in that process.

In 1888, as a result of antagonism in Germany toward James Maxwell's theory of electromagnetism, Hertz was obliged to conclude as follows when presenting in Berlin his extensive experimental proof supporting the theory over the rival action-at-a-distance: "We have applied the term rays of electric force to the phenomena which we have investigated. We may perhaps further designate them as rays of light of very great wavelength. The experiments described appear to me, at any rate, eminently adapted to remove any doubt as to the identity of light, radiant heat, and electromagnetic wave motion. I believe that from now on we shall have greater confidence in making use of the advantages which this identity enables us to derive both in the study of optics and of electricity."[1]

In 1891, Philipp Lenard became an assistant to Hertz, who had previously demonstrated that cathode rays pass through thin metal plates. In 1894, after a series of careful experiments, Lenard succeeded in obtaining cathode rays *outside* the discharge tube after they had passed through a thin aluminum plate window. In 1896, he was an honored guest at a British Association for the Advancement of Science meeting in Liverpool,[2] where he reported his experimental results without drawing any pertinent conclusions from them. It was J. J. Thomson who, on reflection and as a result of his own research, realized that the electrified particles are much smaller than ordinary atoms. Even in the UK there were opposing views regarding those cathode rays; either they were like light waves or they were charged particles of matter. Thomson's pragmatic opinion on the matter is noteworthy: "The electrified-particle theory has for purposes of research a great advantage over the ethereal wave theory, since it is definite and its consequences can be predicted; with the ethereal theory it is impossible to predict what will happen under any given circumstances, as on this theory we are dealing with hitherto unobserved phenomena in the ether, of whose laws we are ignorant."[3]

Regarding the yellow spectral line emitted by excited sodium vapor and split into three closely spaced lines in a static magnetic field—as first performed by Leiden University student Pieter Zeeman (with the imaginative explanation offered by his professor, Hendrik Lorentz), using an oscillating model for an electron in the sodium ion—the large spectral shift does indeed imply a large charge-to-mass (e/m) ratio. Nevertheless, since the sodium ion's mass in the model was assumed by comparison (correctly as it turned out) to be sufficiently large to be neglected, at the time it could hardly be argued, as Lazarus tries to do, that "Lorentz had calculated the best value of e/m." It provided, of course, important corroborative evidence to influence scientific opinion, which was Trilling's important point.

Unlike Lorentz, who made such a gentlemanly gesture, Lenard was obsessed with being denied proper credit, and his similar conflicts with Wilhelm Röntgen and other German physicists suggest a persecution complex, although they hardly explain his later vitriolic antisemitic statements. This persecution mania was manifested in a Heidelberg University pamphlet published in 1914, the first year of World War I (and reprinted in 1940, early in World War II), in which he declared that

"One notices approximately the following from the literature of my discipline in the last ten years: England makes itself look as if it leads all alone; advancements achieved abroad are copiously used, but openly used only where they play no essential role or else they are annexed with the aid of a certain evasion—the source is then found cited somewhere in a tucked-away place deep inside the publication or only in some difficult to locate minor publication. Sometimes direct historical falsification is used. In short, an individual Englishman—even when he is an individual scientist—provides in principle approximately the same picture as that which one gets from English politics."[4]

In fact, though, British scientific publications have long been informative and well balanced regarding this matter, as reflected in the July 1997 issue of *Physics Education*, which focuses on the discovery of the electron.[5] The issue's editorial page, for instance, discusses Thomson's now-famous discourse on cathode rays on 30 April 1897 "in which he cautiously but confidently announced that his own results together with those of other experimenters (Lenard in particular)[, in Thomson's own words] 'seem to favour the hypothesis that the carriers of the charges are smaller than the atoms of hydrogen.' "[6]

References

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5. L. Gerward, C. Cousins, Phys. Education **32**, 219 (1997). I. Falconer, Phys. Education **32**, 226 (1997).
6. S. Adams, Phys. Education **32**, 207 (1997).

David Walsh
Victoria, British Columbia, Canada

Some of the points raised in the letters from George Trilling and Max Lazarus merit further comment or correction.

Trilling is certainly right in stating that Pieter Zeeman and Hendrik Lorentz deserve more credit than they usually receive in standard histories of the electron's discovery. J. J. Thomson was himself the first physicist to appreciate and remark on this precise point. In Thomson's Friday night lecture at the Royal Institution in London on 30 April 1897, he ended his lecture with the following sentence: "It is interesting to notice that the value of e/m, which we have found from the cathode rays is of the same order as the value 10^7 deduced by Zeeman from his experiments on the effect of a magnetic field on the period of the sodium light."

It seems likely from this that the experimental work by Zeeman, and its theoretical interpretation by Lorentz, gave Thomson the courage to announce to the world that cathode-ray beams consisted of particles with masses at least a thousand times smaller than that of the hydrogen atom. This suggestion was resisted by many prominent physicists until 1899, when Thomson measured the charge e with the cloud chamber method developed by his student, Charles T. R. Wilson, and thus was able to calculate an approximate value for m from admittedly crude measurements of e/m and e. This was the research that finally established Thomson as the discoverer of the electron—albeit not in 1897 but in 1899, as Abraham Pais has pointed out.[1]

Regarding the electron-discovery question, Lazarus makes three statements that I would like to challenge.

First, he states that Wilhelm Hallwachs (1859–1922) studied under Heinrich Hertz (1857–94). Hallwachs received his doctoral degree under August Kundt in Strasbourg in 1883 and was later a professor of physics and electrical engineering at the Dresden Polytechnic. The only time Hertz and Hallwachs were together was briefly during their student days in Berlin, where they both worked under Hermann von Helmholtz's direction.

Second, Lazarus claims that credit for the discovery of the electron really belongs to Hallwachs, Julius Elster and Heinrich Geitel. However, the first paper ever published on the photoelectric effect was Hertz's in 1887.[2] In 1888, Hallwachs, who was at that time a *Privatdozent* in Leipzig, followed up on Hertz's work and soon became the world's leading expert on the photoelectric effect (Hertz had abandoned this field, after six months of dedicated and extremely successful work, to return to his research on the production, propagation and properties of the electromagnetic waves predicted by Maxwell's theory, the research that brought Hertz undying fame). It was due to Hallwach's work that in Germany the photoelectric effect was often referred to as the Hallwachs effect. Hertz, however, deserves the credit for its original discovery, and his assistant in Bonn in the years 1891–93, Philipp Lenard (1862–1947), made many additional contributions to both photoelectric and cathode-ray research in the years leading up to Lenard's 1905 Nobel Prize in Physics "for his work on cathode rays." It was only a year later that Thomson received his Nobel Prize, as the Nobel awards committee stated, "in recognition of the great merits of his theoretical and experimental investigations on the conduction of electricity by gases." (Strangely enough, the citation contained no reference to the discovery of the electron, probably for reasons that have been discussed by Pais.[1])

Third, Lazarus makes the point that Walter Kaufmann measured the charge-to-mass ratio for cathode-ray particles in 1897. In fact, Kaufmann actually found a value of 1.77×10^7 emu/g (that is, 1.77×10^{11} C/kg),[3] a much better result than Thomson's original value. Lazarus rightly points out that Kaufmann demonstrated the dependence of e/m on the velocity of the beta rays (electrons) emitted by radium. However, that was some years later, in the period between 1899 and 1902.

References

1. See A. Pais, *Inward Bound*, Oxford U. P., New York (1986), pp. 84–86.
2. H. R. Hertz, Ann. Phys. (Leipzig) **31**, 983 (1887).
3. W. Kaufmann, Ann. Phys. **62**, 596 (1897). Kaufmann submitted this article to the journal on 25 October 1897, six months after the April announcement by Thomson of his remarkable results.

JOSEPH F. MULLIGAN
Salisbury, Maryland

LAZARUS REPLIES: With regard to David Walsh's letter, I am afraid that I do not see the connection between his discussion of Heinrich Hertz's discovery of radio waves (third paragraph) and his discussion immediately thereafter of cathode rays (fourth paragraph).

As for Joseph Mulligan's three challenges, I have already published much about Hertz and know his papers well, so I stand by what I stated in my original letter. However, please allow me to supplement my remarks by summoning the venerable *Brockhaus Enzyklopaedie* to support my contention that Hertz indeed was Wilhelm Hallwach's teacher[1] and the renowned Max Born to back up my crediting Walter Kaufmann for his relativistic e/m observation in 1897.[2]

Let me close by saying that I regret that what showed signs of being a good-natured debate about the electron centennial has been somewhat tarnished by inclusion of the notorious Philipp Lenard, who, thanks to his venomous and open racism, lost all credibility even before the emergence of the Third Reich.

References

1. *Brockhaus Enzyklopaedie,* Wiesbaden, Germany (1969), vol. 8, p. 89 (entry on Heinrich Hertz).
2. M. Born, *Atomic Physics*, 6th ed., Blackie, London (1960), p. 27.

MAX J. LAZARUS
(m.lazarus@lancaster.ac.uk)
University of Lancaster
Lancaster, England

Memories of Casting a Wide *Nyet* at Geneva Talks in Late 1950s

In "Science and Politics in Early Nuclear Test Ban Negotiations" (PHYSICS TODAY, March, page 34), Kai-Henrik Barth has given an excellent account of the work of the Geneva Conference of Experts, convened in 1958, and the subsequent work of Technical Working Group 2, of which I was vice chairman. Between those two endeavors was an additional negotiation called Technical Working Group 1, which I chaired and which addressed the technical aspects of verifying potential nuclear test explosions in outer space. Those three sets of meetings constituted a unique experiment in the use of scientists as negotiators representing nations of diverging interests.

The following account is intended to complement Barth's article.

It has long been, and still is, customary for scientists to be attached as advisors to diplomatic delegations and also to provide advice during the formulation of negotiating positions. In addition, it is customary for nongovernmental organizations (NGOs) to organize meetings in which scientists from various nations discuss controversial issues with major policy implications in a problem-solving spirit, but without committing their governments in any way. Examples of such meetings are the discussions held among the national academies of various countries on security issues, as well as those organized by the Pugwash group and the Italian Isodarco group.

I and the other scientists who served as official members of the US delegations to the Conference of Experts and TWG 1 and TWG 2 were in no way instructed by the US government to bias the outcome of the discussions. Nevertheless, to quote from the conclusion of Barth's article: "In the end the position taken by an indi-