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.

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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.

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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 individual scientist correlated fully with his nationality. . . ."

The negotiation of TWG 1 identified a number of methods to verify the occurrence of nuclear explosions in outer space. TWG 1 was relatively harmonious and can well be given credit for the fact that the Limited Nuclear Test Ban Treaty of 1963 included explosions in outer space in its prohibitions. One reason for the more positive outcome of TWG 1 relative to TWG 2 was that one option, introduced by the US side for detecting space nuclear explosions with photomultipliers deployed on orbiting satellites (the basis of the VELA satellites), was a more capable system than the ones introduced by the Soviet side. Thus, in this instance, the correlation between nationality and position taken, which occurred in TWG 2, was broken: As Barth points out, the US side generally took the position that detection and identification would be more difficult than was asserted by the Soviet side.

Let me recite one anecdote about the lack of government instruction in this respect. One method of detection, ionospheric radar, uses the fact that a nuclear explosion in outer space deposits energy in the ionosphere and thus changes its ionization density. This, in turn, modifies radar reflections from the ionosphere. The Soviets objected to the inclusion of this method in the verification system since, presumably, they knew that ionospheric radars could also detect missile trajectories. In a private conference with Yevgeny Federov, the head of the Soviet delegation, I pointed out that we were charged with laying a scientific basis for verification of a potential nuclear test ban treaty and that other factors, including political factors, were not to be considered. Federov replied (I'm quoting from memory, not from a record), "I am to take all considerations into account!" I therefore proposed that we simply should agree to disagree on that point so the work could go forward. I then cabled the State Department for instructions to permit me to "agree to disagree" on the point. A reply cable said, "What is an ionospheric radar?" I cabled back, "Please check with the President's Science Advisor." I received a cable back, "The cognizant person (Spurgeon M. Keeny Jr) in the President's Science Advisor's office is in Geneva negotiating as part of the TWG 1 team." So we went full circle and ended up making up our own instructions.

Another illuminating episode was the following. At the end of the negotiations, I proposed to write a "motherhood" clause stating (again quoting from memory): "The previous assessment of the capability of the detection system is based on existing knowledge of signals and backgrounds. A future assessment may make detection capability appear either better or worse, depending on future assessments of signals and background." Federov replied something like, "Oh no! In the future detection capability must always be better since according to Leninist dialectic, scientific progress is unlimited and therefore will favor our needs." We agreed to omit my proposed phrase altogether.

In summary, the negotiations undertaken by the Conference of Experts, TWG 1 and TWG 2—all conducted on a technical level—were an interesting political experiment that has not been repeated. It is fair to say that neither the US government nor the Soviet government had carefully analyzed the implications of asking scientists to negotiate as government delegates. Among other problems, they did not settle the question of whether the product of the negotiations would be an agreed-upon and immutable scientific basis to be referred to the political authorities (the expressed Soviet opinion) or simply a scientific assessment that could be changed as new scientific information became available (the US delegates' view). There also was no understanding on the political level—on either side—as to the meaning of "adequate" verification capabilities.

Thus, President Eisenhower's idealistic concept, that scientists who were citizens of states of opposing interests could establish objective truth to lay the basis for future political negotiations, turned out to be only partially successful.

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Award "Good Physics" Seal to Approved Nonphysics Texts

I couldn't agree more with Peter Schoch's lament about incorrect physics in nonphysics texts (PHYSICS TODAY, March, page 11). He is to be commended for writing to the publisher of the problem text he described, and the unnamed publisher is to be commended for taking Schoch's constructive criticism seriously. It would be nice if all such publishers would submit their materials to authoritative physics reviewers and thereby avoid the risk of putting out inaccurate or embarrassing texts.

I suspect that individual physics professors like Schoch and myself would not be able to get many publishers to cooperate. But perhaps the physics community could do so, acting through an organization such as the American Institute of Physics or the American Association of Physics Teachers.

Imagine, say, AIP or AAPT setting up a small committee to do the actual reviewing. Then, when the committee approves a particular book, the parent organization could issue a "Good Physics" type of seal of approval, which the publisher would be authorized to affix to the book.

Is this suggestion practical?

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Fifteen Ways to Get Your Audience to Leave You

I have found 15 effective ways to transform good science into a poor presentation. Here is the list, which I use for my own benefit and that of my students:

1. Cover too much material.
2. Include too many details.
3. Start with too much small talk.
4. Run over the allotted speaking time.
5. Avoid telling the audience why your research has been done.
6. Overestimate, or at least fail to assess, the audience's level of knowledge.
7. Fail to make contact with the audience.
8. Ignore the inherent difference that exists between oral and written communication.
9. Waste time searching for a specific overhead foil somewhere in your pile.
10. Use unexplained terminology, abbreviations and acronyms.
11. Use unexplained symbols in text or equations.
12. Use unexplained graphics.
13. Present overhead transparencies that are unreadable.
14. Read *in extenso* from projected transparencies.
15. If the moderator has just introduced you to the audience, alienate both parties by opening your presentation with such details as your name, your affiliation and the title of your talk.

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