International position of US physics

GUEST COMMENT
by
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In order to assess changes in the standing of physics in the US over the past few years, we have undertaken a simple study of the situation in one particular subfield, the physics of electronic and atomic collisions. Using the proceedings from the last five International Conferences on the Physics of Electronic and Atomic Collisions, we have tried to determine if a shift has occurred in the position of US research activities as measured by the fraction of contributed papers from various geographic areas as well as by the attendance figures available to us.

Since 1958, ICPEAC has been held at roughly two-year intervals and has attracted large and representative groups of atomic-collision physicists. The meetings have been broadly international throughout, but there has been understandable variation in participation, depending on the location of the conference site. Even taking such variations into account, we find it possible to see some encouraging and some disquieting trends.

Figure 1 shows the geographic distribution of the authors of several hundred papers contributed to each meeting. Comparing two meetings held in Western Europe (Amsterdam 1971 and Paris 1977) we see an increase of 50% in the total number of papers, but the US share, while remaining almost constant in number, has dropped from almost one-half to one-third of the total.

Figure 2 shows a particularly dramatic decline in US attendance this year both in absolute and relative numbers. Western Europe has been the biggest gainer.

We know, of course, that a multitude of effects contribute to such changes, but we feel that the figures support the conclusion that while atomic-collision physics is a vital field of research exhibiting strong growth, the US involvement in it has remained constant, resulting in a noticeable relative decline.

Two new areas in which the US is particularly lagging are experimental research with high-energy accelerators and with lasers. Some scientists are overcoming the handicap of a lack of facilities in the US by participating in collaborative efforts in Western Europe. Such research by US scientists accounted for 4% of the contributed papers at the Paris meeting. (Complete tables of the information used for figures 1 and 2 are available from the authors.)

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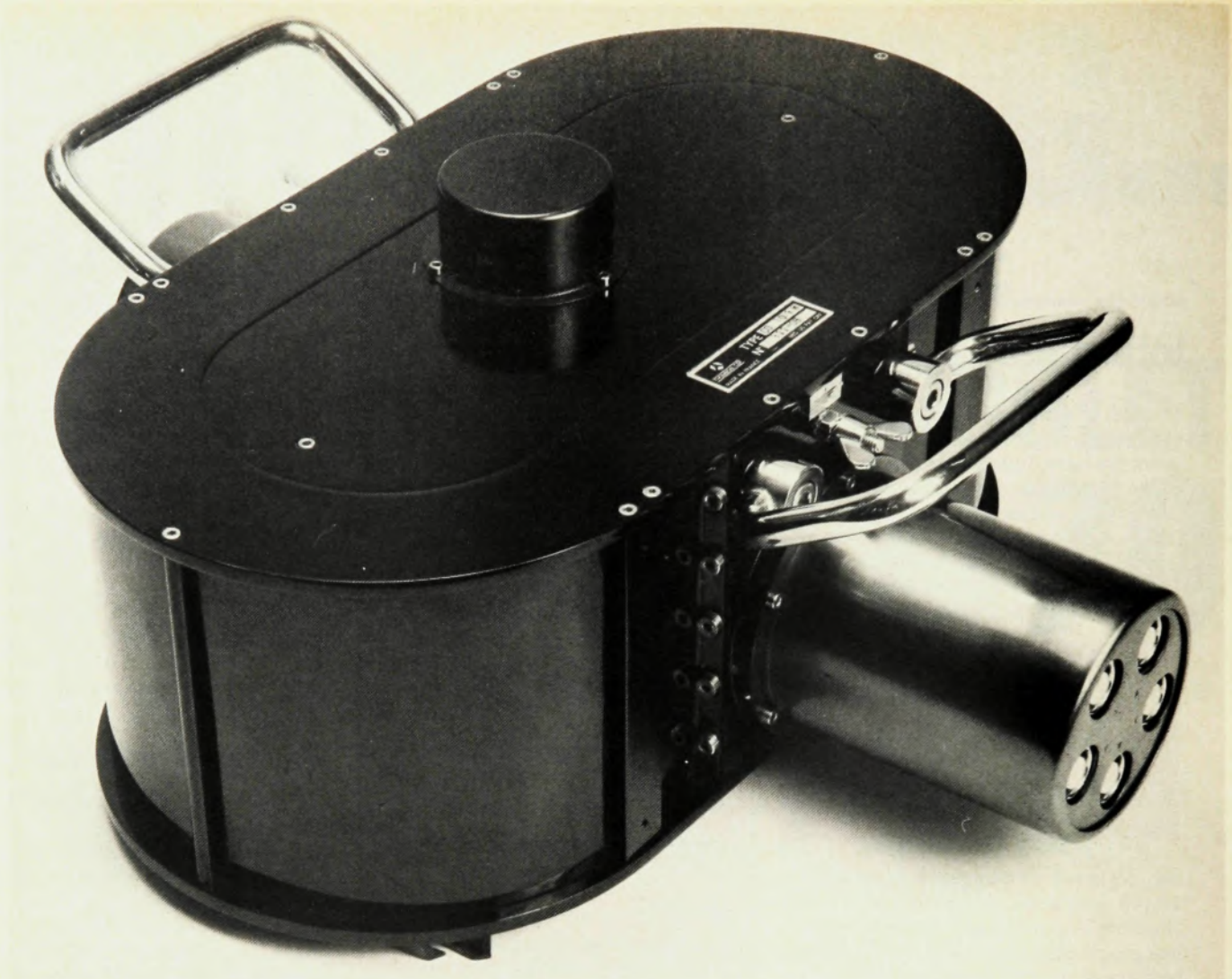
Who discovered matter waves?

I found Richard Gehrenbeck's "Electron diffraction: fifty years ago" (January, page 34) to be a splendidly informative account of Clinton Davisson and Lester Germer's work on matter waves (the observation of which is, I would suggest, one of the two most important empirical discoveries of the period since 1920—the other being the discerning of the universal galactic expansion). Many physicists must have often wondered, as have I, "How did Davisson and Germer happen to be reflecting electrons from a nickel crystal?", and it is satisfying now to know the answer.

Gehrenbeck emphasizes how ready European theorists were to accept the existence of electron waves, even on the basis of Davisson and Germer's initially rather slight evidence. In this connection, PHYSICS TODAY readers might be interested in the following account by Max Born, from the overseas side:[1]

> "This is de Broglie's law. He studied the consequences for plane waves and indicated the interpretation of Bohr's quantum conditions with the help of standing waves. But what did he predict? As far as I know, nothing. Then were the interference fringes of cathode rays discovered experimentally? There is no truth in this either. The real facts are these: Directed by a remark of Einstein, my colleague Franck and I pondered about the meaning of de Broglie's waves. One day I received a letter from Davisson in America, containing accounts of measurements on the reflexion of electrons by nickel crystals with the question whether we could make



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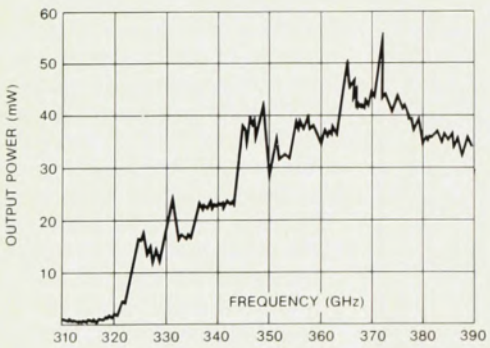

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sense of the strange maxima and minima of his curves. How it came that we connected these with de Broglie I cannot remember in detail. Some remarks by Einstein had something to do with it; also considerations about the Ramsauer effect (i.e. the increase of the range of free electrons in some gases for decreasing velocity). Anyhow, we encouraged Franck's pupil Elsasser to work it out; he found the correct explanation and de Broglie's formula was confirmed. The final demonstration of electronic diffraction by crystals is due to Davisson and G. P. Thomson."

Reference

1. Max Born, *Experiment and Theory in Physics*, Cambridge U.P., 1943, pages 21–22. The content of this booklet (44 pages) is described as being a slightly expanded form of a lecture given by Born to the Durham Philosophical Society and King's College Pure Science Society, at Newcastle-upon-Tyne, 21 May 1943.

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THE AUTHOR COMMENTS: I appreciate Richard Schlegel's interest in my article, and the opportunity he offers me to make a comment I was unable to include in my original article. It concerns the difficulties and pitfalls of using recollections or remembrances in attempting to reconstruct the history of science.

In my article I suggested that the discovery of electron diffraction by Davisson and Germer was the result of a whole combination of circumstances: a highly developed experimental technique, various "accidents," keen observations and reflections, and a key theoretical input late in the game. Max Born, in the recollection quoted by Schlegel, suggests something else; that it was not de Broglie's theory, and not experiment either, that produced the discovery. What does he say it was? I'm not quite sure. But he suggests that he, his colleague James Franck, and their student Walter Elsasser played the key role in the discovery, and that it was the combination of their efforts by which "de Broglie's formula was confirmed"—in 1925! The work of Davisson and Germer, and of G. P. Thomson, is seen as a sort of "mopping-up" action two years later.

In the course of my research on this topic I came across not only the above quotation by Born, but also extensive interviews with all of the principals concerned; they were recorded and transcribed in 1962 and now form part of the Archives for History of Quantum Physics, located at the Library of the American Philosophical Society. As I examined these materials, I found the following curious result: in the Born interview, Born claimed the major role in this episode; in the Franck interview, Franck did. I think the reader can probably guess who got the major credit in the Elsasser interview.

Since these recollections, made 37 years after the event, proved of little value in resolving the question of who originally proposed the idea of connecting the de Broglie theory with the Davisson–Kunsman experiments, I next searched the primary materials. The letter that Born claimed to have received from Davisson either never existed or cannot be found; there are no notebook entries relating to the event, and of course any conversations that took place have long since gone with the wind. I did, however, find two concrete items: a letter from Born to Einstein dated 15 July 1925,[1] and an article by Elsasser submitted to *Die Naturwissenschaften* on 18 July 1925.[2] These two pieces do agree that Elsasser made at least a qualitative comparison between the Davisson–Kunsman results of 1923 and de Broglie's theory, and the Born–Einstein letter also shows that it was a visit by Paul Ehrenfest that got Born (and Franck) interested in the de Broglie idea in the first place. But neither document enabled me to determine who (Born, Franck or Elsasser) made the original creative leap connecting de Broglie with Davisson–Kunsman.

To me, however, the more important question is not who did it, but what was done. As we saw above, Born maintains that it is the Born–Franck–Elsasser work of 1925 that should be recognized as the first confirmation of matter waves; I submit that this is vain and misleading hindsight. The Elsasser article certainly did *suggest* that the Davisson–Kunsman data could be interpreted as a demonstration of de Broglie waves, but as a matter of fact, it contained *not one single numerical calculation* to back up this hypothesis. Furthermore, to my knowledge, no one to this day has successfully shown that there is any quantitative agreement between this data (as opposed to the later Davisson–Germer data) and diffracted electrons; Davisson himself completely rejected it when he heard about it (an event which I have also found impossible to pinpoint exactly chronologically). To claim that Born–Franck–Elsasser confirmed the de Broglie hypothesis in 1925 is like saying that Aristarchus confirmed the heliocentric theory in 250 BC; had it not been for the later quantitative work of Copernicus, Kepler and Newton we would still be geocentrists today! Similarly, if it had not been for the work of Davisson and Germer and of G. P. Thomson, we would be very uncomfortable about any serious allegation that electrons do indeed behave like waves.

I would like to suggest that, although recollections made after the fact are often excellent sources of ideas for things to

look into and for establishing certain overall impressions of times and events gone by, they are seldom, if ever, to be relied upon to establish particular facts or even interpretations of facts. The reconstruction of the history of science must ultimately rely as much on the data of the contemporary documents of history as the conclusions of the research physicist must conform to the "stubborn and brute facts" of nature.

References

1. Max Born, ed., Irene Born, trans., *The Born–Einstein Letters*, Walker, New York (1971).
2. W. Elsasser, "Bermerkungen zur Quantenmechanik Freier Electronen," *Naturwiss.*, 13 (14 Aug. 1975), 711.

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2/16/78

Empty space in the galaxy?

In an admirable article on the 18th-century physicist J. H. Lambert that appeared in September (page 25), Stanley L. Jaki writes that Lambert "pictures the Milky Way as a disk composed of a large number of stellar systems, with an empty space in its central region," and speaks of Lambert's "ring-like model of the Milky Way."

The evidence for this supposed empty space rests, I believe, on the phrase '*innern Diameter*' in Letter XIV of Lambert's *Cosmological Letters*. In this letter the author discusses the relationship between our star cluster and the other clusters that make up the Milky Way system, and in my opinion the phrase refers to the extent of the empty space immediately surrounding our cluster, before one comes to neighboring clusters. But even if Jaki is right in his interpretation, still the "inner diameter" of the Milky Way is no more than the space separating our cluster from its nearest neighbors (1500 Sirius-distances), so that the center of the Milky Way—far from being empty—is as packed with clusters as the rest of the Milky Way.

I develop this theme in a review of Jaki's translation of *Cosmological Letters*, to appear in the June issue of *Journal for the History of Astronomy*.

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1/12/78

THE AUTHOR REPLIES: That the center, or central area of the Milky Way, as conceived by Lambert, cannot be as densely packed with star clusters as are its more outlying parts is a consequence of the kind of hierarchical organization that he attributes to the world-edifice. As illustrated by a diagram in my article, in such an organization the distribution of no unit (be it a cluster, or local system of stars, which on the hierarchical ladder is four to six times subordinate to the Milky Way according to Lambert's speculations) is homogeneous. The chief reason for this is the increasingly more enormous mass of higher ranking central bodies (reagents), a circumstance which entails optical and gravitational consequences set forth by Lambert himself. A "ring" may not be the best word for conveying this situation, but is possibly no more inappropriate than the expression, "a disk with a large hole in its center." Only such a hole would justify the use of the term "inner diameter." It may also be of interest that the idea of a homogeneous distribution of stars throughout the Milky Way, including its central part, was advocated by Soldner in 1800 as he advanced arguments relating to gravitation against Lambert's idea of a central body in the Milky Way. Concerning this latter point, see my articles on Soldner's calculation in 1801 of the bending of light around a celestial body forthcoming in *Sky and Telescope* and in *Foundations of Physics*, a calculation to which he was led by considerations relating to Lambert's cosmology.

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3/7/78

RMP reviews

I wish to comment on a revision in the format of *Reviews of Modern Physics*. At present, this publication consists of selected, rather lengthy, comprehensive reviews, which are intended for those wishing to delve deeply into the particular field under examination. Part of the purpose of a physics review journal should be to inform the physics community continually of the relatively new advances and approaches in the many subdisciplines of physics. A single issue of such a journal should entail several short articles, each approximately five pages long, in various physics sub-fields, *either* instead of *or* in addition to the present format of fewer, longer reviews. These reviews would be technical, more so than articles published in PHYSICS TODAY, but both more convenient and thought-provoking for the general readership than, say, the various sections of *Physical Review*. The nature of the contributions to such a journal would be similar to those in *Accounts of Chemical Research*, published by the American Chemical Society.

The present format of the *Reviews of Modern Physics* does not accomplish its stated purpose. There does appear to be a need to make such shortened reviews available to the physics community. This could probably be best accomplished the