

letters

Mayer's work at Columbia University during World War II can be divided into three categories: a study of uranium spectra and of the possibility of photochemical isotope separation, a study of the possibility of chemical separation, and the analysis of the structure of uranium hexafluoride—the last being a topic of interest in both gaseous diffusion and chemical separation work. Bigeleisen seems to object to my mentioning only Mayer's work on the structure of UF_6 and not discussing her work on chemical separation. My article, however, was not intended to be a comprehensive analysis of Mayer's scientific contributions. I focused instead on particular aspects of her work that led her to the concept of a nuclear shell model. The analysis of the structure of UF_6 was one such aspect. This work was conceptually related to her later discovery of the nuclear shell model in ways that her study of chemical exchange separation (which I discuss in my dissertation) was not.

Bigeleisen is unhappy with my description of the organization of the photochemical separation project at Columbia. I did in fact consult Gerhard H. Dieke and Albert B. F. Duncan's *Spectroscopic Properties of Uranium Compounds* when I wrote my dissertation. These authors, in describing the spectroscopic work at Columbia, wrote, "Theoretical analyses were under the supervision of Mrs. M. G. Mayer," and they then presented a list of "scientific collaborators."² Beyond that, they did not describe the organization of the project. Mayer, however, in a letter she wrote to Max Born in 1946, described herself as "running an experimental group,"³ and she also later referred to the photochemical separation project as her first experience in being personally responsible for the progress of a research project.⁴ My description of the project's organization, therefore, was based on published and unpublished documents that date to the period in question, and these do seem to conflict to some degree with Bigeleisen's recollections. While this point is not of pivotal significance to my analysis, I share Bigeleisen's hope that future historical research will shed more light on it.

References

1. K. E. Johnson, *Maria Goeppert Mayer and the Development of the Nuclear Shell Model*, PhD dissertation, University of Minnesota, Minneapolis, 1986.
2. G. H. Dieke, A. B. F. Duncan, *Spectroscopic Properties of Uranium Compounds*, McGraw-Hill, New York (1949), p. xiii.
3. M. G. Mayer, letter to M. Born, 8 January

1946, Born *Nachlass*, Staatsbibliothek preussischer Kulturbesitz, Berlin.

4. J. Dash, *A Life of One's Own: Three Gifted Women and the Men They Married*, Harper and Row, New York (1973), p. 294.

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7/87

Role of physics in industry

In his editorial (August 1986, page 94), Venkatesh Narayanamurti notes numerous failings of technologically based American industry, but makes a recommendation—increased support for exploratory physics research—that is at odds with these observations. Improved product reliability results from careful attention to engineering details, not the implementation of new, and therefore poorly characterized, processes and devices. (For example, it took nearly a decade for the transistor to be made more reliable than its predecessor, the vacuum tube. The economic advantage of the transistor has gone to those who solved the engineering problems.) Our failure in instrumentation, which is based on old physics, is due to the minimal direct participation of our physicists in that line of business. Inadequate applications support results from the paucity of available people for field engineering and marketing positions, a situation that would only be further aggravated by an increased emphasis on basic research without a dramatic increase in the number of physicists. Our biggest failure has been in manufacturing, which is completely an engineering problem—again, old physics.

If the physics community is going to make a significant contribution to solving America's current industrial problems, it must become directly involved. Experimental physics is well suited for training students, since it involves instrumentation and materials. These skills need to be applied in manufacturing and marketing, where our failures have been the greatest, not in the basic research labs.

HARRY J. LEVINSON
San Jose, California

9/86

NARAYANAMURTI REPLIES: Harry J. Levinson has missed the main point of my editorial. Our failure in instrumentation is due to industry's unwillingness to adjust to the customer's needs for ever more advanced functions, that is, to stay at the leading edge. We are losing the instrumentation market because we are not keeping up in performance with the competition, not because of poor reliability. To get an advantage we need to pay increasing

attention to the interface between research and development, which is a long-term problem by necessity. That is what our competitors in Europe and Japan are doing most effectively. Therefore, for our long-term health we need to maintain steady support for research and, at the same time, increase the support at the interface of R&D and manufacturing.

VENKATESH NARAYANAMURTI
APS Subcommittee on International
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5/87

Rename Livermore (part 2)

As someone who was raised in Livermore and who has worked at Lawrence Livermore National Laboratory, I feel compelled to respond to Mary B. Lawrence's letter (October 1986, page 9). I sympathize with her efforts to remove her late husband's name from LLNL. However, I must take exception to some of her reasons. She writes: "Of course the Livermore people enjoy the prestige and the aura of scientific expertise and sanctity that accrue to them through the use of two famous names, that of a great university and that of a great scientist." With hundreds of first-rate scientists at Livermore, it is ridiculous to suppose that their work is respected in the scientific community merely because of the Lawrence name. Indeed, people elsewhere generally refer to LLNL simply as "Livermore"; LLNL employees use "the Lab," "L3" or "the Rad Lab." Lawrence also contends that it is confusing to have the labs in Livermore and Berkeley, as well as the Lawrence Hall of Science, all bear the Lawrence name. No matter what one does, some confusion will always arise. I have personally known professionals who thought that LLNL was named after "Larry Livermore."

LAURA CHAN-JU KANG
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10/86

Mary B. Lawrence was absolutely correct that Lawrence Livermore National Laboratory has become a very different kind of institution from what it was in Ernest O. Lawrence's day. In Lawrence's day the Radiation Laboratory in Livermore was devoted almost entirely to defense-related research and development. Perhaps when the Regents of the University of California named the Radiation Laboratories (both Berkeley and Livermore) in Lawrence's honor in 1958, their intention was to use the Livermore institution to recognize the part of his great contributions that went to national defense.