to obtain some degree of mastery in a foreign language, if only as a concession to the historical origins and image of the scientific enterprise.

OWEN GINGERICH
Center for Astrophysics
Cambridge, Mass.
1/6/77

Physicists' image

Far from improving the image of the physicist in the public's mind, William Fowler's article (April, page 33), "Physics in 1976—A Personal Account," will only confirm his impression of the physicist as a Dr Strangelove technocrat. Society needs physicists more for their brains, not for their skill with a welding torch.

I also object to Fowler's vision of the physicist on scientific grounds. It has been pointed out many times that in the 1930's, while E. O. Lawrence—the epitome of the sort of physicist Fowler admires—was building bigger and bigger machines, the *real* discoveries were being made in Europe with very modest equipment. One should also remember the disaster caused by Lawrence (the Oppenheimer Affair) when he became involved with politics. I prefer Einstein!

ROBERT HERMANN
Harvard University
Cambridge, Massachusetts
4/15/77

•

I applaud William Fowler's article concerning the image of the physicist. I agree with him that our image is very often that of the ivory tower, lab-coated mad scientist. I only wish that Fowler had also addressed the fact that physicists are always imagined as *men*. He would not have to look far in the APS or elsewhere in the physics community to find that women physicists have contributed to the profession more than the words "chairperson" and "vice-chairperson." Was Chien-Shiung Wu's foremost accomplishment that of being the first woman president?

It would have been heartening to find pictures of women as well as men pursuing physics or their hobbies. If Fowler cannot find any in the files, I would be glad to supply a few. At least he did show a man in the kitchen.

ALICE MACNOW
McGraw-Hill Book Company
New York, New York
4/26/77

Nuclear safety

I must comment on Harold Lewis's review of my treatise *The Accident Hazards of Nuclear Power Plants* (Univ. of Mass. Press) in March, page 63. Lewis concludes that "there are mistakes," but nonetheless my book "is a good source, when read warily." However, the two "examples" he alleges to justify the implication that my work is interspersed with errors are fictitious. Firstly, he claims that my book asserts that in a water-cooled reactor "one can see leakage in the pressure vessel before a catastrophic failure." He says "one can't." On the contrary, I made no such assertion. The book merely notes that slow crack growth and a leak-before-break is "expected," as I interpreted the Atomic Energy Commission's 1973 report, *The Safety of Nuclear Power Reactors* (WASH-1250, pages 7.8–10). That report, which I neglected to cite, emphasizes leak-warning as a backup to inspection for cracks. I then added that I am "not in a position to comment on the reliability of such forewarning" (page 39). In effect, I merely left it as a question. (Britain's Marshall Report, issued after my book, shows a leak-before-break may be unlikely, as Lewis noted. This needs further study.)

Secondly, Lewis alleges that my book asserts that "a reactor can somehow explode like an atomic bomb." (He says "it can't.") This second allegation is also untrue. On page 12 of my book I state in regard to nuclear runaway (an explosive power excursion) "However, no nuclear-weapon-type explosion appears possible in a water-cooled reactor." (The physical reasons are noted on page 17.) This covers 97% of US power reactors. The book asserts a severe potential for *nuclear explosions* in the liquid-metal-cooled, fast-neutron, breeder reactor (LMFBR), but I state that it is "not of the magnitude of an atomic weapon, though that has not been ruled out, but severe enough to release great amounts of harmful radioactivity" (page 2). The highest estimate I give (and prove) is 20 000 pounds TNT (equivalent). A-bomb yields are of the order of 20 000 *tons* of TNT. (The book shows that an abandonment of 150 000 square miles could be the consequences, due to plutonium fallout alone.) However, because an LMFBR would contain several "critical masses" of fuel, there is the question whether a severe explosion might compress momentarily one such mass of fuel to generate an A-bomb size excursion. It is only a question; but because of practical limits of computation and experimentation, it remains unanswered. Regarding the LMFBR explosion potential, the book quotes Argonne National Laboratory: "mathematically derivable upper limits are not obtainable" (page 138).

Eventually, a *subjective* judgment will have to be made as to whether reactors are safe. (The book identifies many crucial subjective assumptions.) The public decision will therefore depend on *who decides*. For this reason the book devotes

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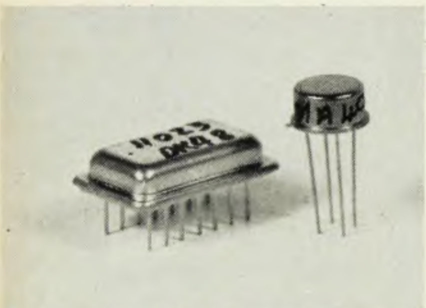

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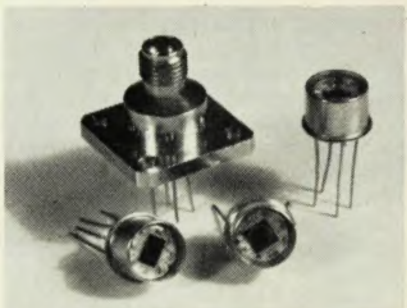

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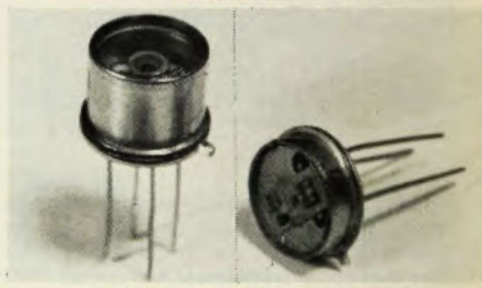

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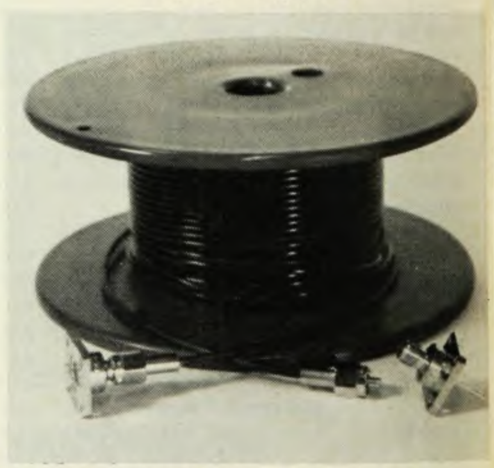

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a chapter to a constitutional law inquiry, which concludes that the federal civilian nuclear program and the safety decisions under it are unconstitutional; not because the framers did not foresee and "mandate" nuclear energy, as Lewis interprets the chapter, but because they limited the federal powers to those which were not to be, and cannot reasonably be, construed as powers "to promote manufactures" by spending and granting immunities, or to provide for "the advancement of useful knowledge and discoveries" (that is, civilian research and development). Such powers were expressly proposed but rejected in the federal Convention of 1787. New powers can only be granted by constitutional amendment—a process that was to enable the people to control their government and best pursue their safety and happiness (way of life).

Lewis asserts that on this issue I am "furthest out" and that the book "is short on practical suggestions for ascertaining" the judgment of "will of the people" on nuclear safety. However, he ignores the legal brief in the chapter. As for practical suggestions, the book does indeed specify one; namely, the constitutional method. Drawing on the amendment procedure of Article V of the US Constitution, the book explains that Congress should review civilian nuclear energy for its necessity and safety; and if they then want to continue with the federal program, they would have to request the authority to do so by an amendment proposition submitted to the States for ratification (preferably by the mode of state conventions, for which the people would elect delegates). To say that constitutional methods are not practical is to turn away from self-government. It is noteworthy that our cherished Bill of Rights was enacted by the amendment process.

In a "personal note" Lewis resents the assertion—made in my chapter on the American Physical Society reactor safety study—that there is a "reluctance of the ... physics community to discuss" certain issues, which he does not identify, nor mention anywhere else in his review, though my book does. These are the safety issues of power excursions and power-cooling mismatch accidents—both worse than the loss-of-coolant accident. As the book shows, such accidents appear to have the potential for causing agricultural restrictions over 500 000 square miles, due to strontium-90 fallout, and other huge consequences. Extensive efforts on my part to persuade the physics community to pursue these issues in funded research were unsuccessful. Also, the APS study group, of which Lewis was chairman, disregarded a key power-excursion analysis I sent them. Specifically, during their working session in 1974, I sent the group excerpts of an internal report of the National Reactor Testing Station (1964–65) on power-excursion accidents in light-water reactors. The report calculates a catastrophic explosion potential, and proposed a massive research program, which was disregarded by the AEC. That document, which is treated in my book, contains the only analysis ever prepared on the full excursion potential of present-day reactors; but it was kept secret until 1974. Lewis's group did not pursue the matter with me; nor did they even mention the document or the power excursion phenomenon and accident possibility in their report (*Reviews of Modern Physics*, July 1975). Instead, they reported that they had "not uncovered reasons for substantial short-range concern regarding risk of accidents." Was it not correct, therefore, to say that there is a reluctance to discuss such issues?

Finally, Lewis interprets the Preface as promising "a balanced account" of safety issues, but finds the text "an unremitting litany of accident hazards." However, as the book states at the outset, I sought to investigate the essential reactor hazards (not to justify safety). Moreover, I presented my findings of extensive and fundamental shortcomings in official safety analyses and experimental programs, and extremely disastrous accident potentials, not unremittingly, but with the basic mitigating facts.

RICHARD E. WEBB
5/18/77 *Toledo, Ohio*

THE AUTHOR RESPONDS: Richard Webb's letter is full of falsifications about what he has written, what I have written, and, above all, about facts. I was aware that selfrighteousness had blurred his perception of true and false, but hadn't expected him to give the rest of the game away as completely as he did in the next-to-last paragraph:

- "Extensive efforts on my part to persuade the physics community to pursue these issues in funded research were unsuccessful."
- "The report ... proposed a massive research program, which was disregarded by the AEC."

Apparently, Hell hath no fury like an engineer scorned.

H. W. LEWIS
University of California
Santa Barbara, California

Kirkhoff versus Kirchhoff

William Dean's letter (May, page 11) is headed "Kirkhoff pairs" and the text makes reference to "Kirkhoff's laws." In a physics journal, one would expect to find the correct spelling of the name of a well known physicist, Gustav Robert Kirchhoff (1824–87), FRS, professor of physics