little short of hard currency. He hopes, however, to persuade them to contribute rugs.

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Did Heisenberg Misconceive A-Bomb?

I was disappointed to see that Samuel Goudsmit's defenders (May 1991, page 13) have yielded the central point to the campaign being conducted by Mark Walker against Goudsmit's scientific and personal intelligence.

Your correspondents seem to have accepted it as a fact that Werner Heisenberg did understand that any atomic bomb would depend on the scientific principle of a fast-neutron reaction. There is a great deal of direct evidence to the contrary, starting with Heisenberg's original paper of December 1939 that mapped out the strategy of the German uranium project. This paper (now available in *Heisenberg: Gesammelte Werke*, series A, part 2 [Springer-Verlag, 1989]) suggests that Heisenberg's concept of a bomb at this stage was a reactor fueled by pure U-235 that would use an immense mass of the isotope to ensure that there would be enough time (given the length of time that slow-neutron diffusion would require) for a useful enough amount of the U-235 to be turned into energy. It was the vast amount of U-235—tons—that was needed for this reactor–bomb that in fact forestalled the German atomic project. (For a reference to this absurd conception of a critical mass of tons, see R. V. Jones's introduction to the American Institute of Physics reissue of Goudsmit's *Alsos* [Tomash, 1988]: Jones and his colleague Charles Frank actually heard the tapes of Heisenberg's Farm Hall internment conversations in 1945 and affirm that he made out the critical mass to be 4 or 5 tons.)

As to the still classified transcripts of the Farm Hall tape recordings of the German scientists' reactions to the news of Hiroshima in August 1945, all those who have had access to them—including Goudsmit himself, Jones, Frank, Leslie Groves, Paul Rosbaud and Margaret Gowing (the author of the official history *Britain and Atomic Energy 1939–1945* [Macmillan, London, 1964])—are agreed that they bear out the truth of Goudsmit's charge that Heisenberg never did understand during the war the scientific principle of the atomic bomb. Is Walker going to argue that all these people are wrong or blinded by personal losses during the war? I do not believe that Walker can continue to assert blithely that Heisenberg knew what he was doing and that Goudsmit was all at sea. At the very least, the fact that Heisenberg claimed after the war never to have made a calculation of the critical mass of an atomic bomb should ring warning bells about the danger of relying on anything he or other German scientists had to say after Hiroshima about their wartime uranium project.

If it seems farfetched that a physicist of Heisenberg's caliber should have got wrong the essential principle of the bomb, one should recall that in February 1940 hopes for a uranium bomb had been written off in Britain and Otto Frisch—the true inventor of the bomb—had himself so far missed the fast-neutron principle. It was only a brain wave that inspired him in March 1940 to consider a fast-neutron bomb and, along with Rudolf Peierls, calculate that a comparatively small amount of U-235 was needed. In the US this idea was not appreciated until the summer of 1941, as is apparent from the memoirs of Arthur Compton and Mark Oliphant and from much other documentation. Even Niels Bohr himself believed that a fast-neutron bomb would entail a large mass of U-235—that is, until his arrival in London and briefing by the British on 8 October 1943.

PAUL LAWRENCE ROSE
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WALKER REPLIES: I deeply regret that Paul Lawrence Rose has chosen to follow Jonothan Logan's and Max Dresden's lead by misconstruing gravely my words and intent. Thus I feel compelled to reiterate: I have never written or said that Samuel Goudsmit's claims should be rejected *because* he had suffered at the hands of Germans and therefore was no longer objective. Goudsmit's claims are false because of *evidence* to the contrary of those claims.

The report Werner Heisenberg wrote in 1939 is a good source for his understanding of the problem at that time, but people can change their minds. Subsequent documents such as "Die theoretischen Grundlagen für die Energiegewinnung aus der Uranspaltung" (26 February 1942), which is also reprinted in Heisenberg's *Gesammelte Werke*, prove that Heisenberg knew that both uranium-235 and plutonium could be used as nuclear explosives and that these nuclear explosives (and thereby nuclear weapons) used fast-neutron chain reactions. As far as the critical mass is concerned, page 13 of the comprehensive German Army Ordance report of February 1942, "Energiegewinnung aus Uran" (from Erich Bagge's private papers, Kiel), written by Army physicists in consultation with Heisenberg and the other project scientists, proves that the German researchers were working with an estimate of 10–100 kilograms, comparable to the Allied estimates at this time. Heisenberg may have sounded confused at Farm Hall, but that proves little about what he knew or did during the war. Heisenberg and his colleagues knew how to build a bomb in principle by February 1942 at the latest, and it is unlikely that they would have forgotten so soon.

I respectfully suggest that Rose consult my book, especially since I believe he is writing his own book on this subject. If he remains unconvinced I am willing to send him copies of the relevant documents.

MARK WALKER
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Legislating Science Funding Levels

It is sad but increasingly apparent: A scientist has to be politically savvy to be scientifically successful. Leon Lederman's Reference Frame column "The Privilege—and Obligation—of Being a Physicist" (April 1991, page 9) will have received thousands of sympathetic ears, I am sure, but haven't we heard this kind of plea more than once before? I am quite suspicious of the notion that a cohesive political strategy representing the interests of all subfields of physics, to say nothing of the interests of individual institutions, groups and investigators, can be formulated and executed effectively.

I hereby propose a *legal* alternative to Lederman's admirable *political* course. We must keep our message and tactics simple. Start with legislation to mandate that funds are committed to basic science and technology research at a *per capita* rate comparable to that of our major economic competitors (notably Japan). Such a legal course has several advantages. First, it's simple and forceful. Many a politician who might sniff privately at the idea of giving more funds to science may find it politically unwise to object publicly. Second, it is a uniting path of action, around which all disciplines of

science can coalesce. (Let us trust our intelligence and worry about how to divide the pie later.) Third, knowing the world is competitive, the assumption that the flexible term "*per capita* rate" will take off on a upward trajectory among the global economic powers is not an unreasonable one. Thus this approach will save us much repetitive work.

As is always the case for an unpolished idea, this alternative may also have its simplistic shortcomings and undoubtedly suffers from a lack of sophistication. However, as in most human enterprises, the first small step is often the most important one, despite the usual groping, staggering and occasional self-doubts.

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An Issue of Neglect for Astronomy?

It was with great anticipation that I opened my April 1991 copy of PHYSICS TODAY, but my heart was filled with great sadness as I scanned the table of contents. Why, with many distinguished women astronomers and astrophysicists out there, were women not represented among the authors in this special issue on astronomy and astrophysics?

I do not know how articles are chosen for such an issue. Surely some of them must be solicited from the authors, and perhaps some women were asked to contribute to the issue and did not. If the latter is true, then I must fault those women, but I strongly suspect that none were asked.

Time and again I have seen statements from The American Physical Society and the American Astronomical Society, member societies of the American Institute of Physics, that more women must be encouraged to consider careers in physics or astronomy. But where are their role models in PHYSICS TODAY?

The very fine PBS program "The Astronomers" prominently features some of our brightest and best women working on the cutting edge of science. What a fine example PBS is setting for young women in our secondary schools. But PHYSICS TODAY continues to fail in this respect.

The New York Times has a "critics' choice" once a week featuring young successful people in the arts. Why doesn't AIP follow this example? Praise need not be limited to young female physicists, but they are certainly out there among the young men. We need to encourage all talented people to pursue careers in the physical sciences.

SHIRLEY W. HARRISON
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A glance at the April 1991 issue confirms the feeling of most solar physicists that we are the Kurds of astronomy. It is a pity you could not devote a little space to the rich scientific problems of our very own star.

HAROLD ZIRIN
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THE GUEST EDITOR OF THE APRIL 1991 ISSUE REPLIES: Astronomy and astrophysics have profited enormously from both recent and historical research contributions of women. Those of us who helped to organize the astronomy and astrophysics decade survey, from whose committee members the authors of the April 1991 issue were drawn, tried to include women at the highest levels. We were moderately successful. Eight percent of the members over 40 of the American Astronomical Society are women, while women constitute 11% of the total membership of the society. The percentage of women among the panelists of the decade survey was almost exactly halfway between these two numbers, namely, 9.5%. There were 2 women among the 30 panel chairs and members of the executive committee. As Shirley W. Harrison might have guessed, some very distinguished women astronomers declined to serve either as panel chairs or on the executive committee because of other commitments. When we presented the results of our study to the media, two of the five speakers were women, and when we described the recommendations in a symposium at the National Academy of Sciences, one of the five speakers was a woman.

Astronomy of the Sun is mentioned or discussed on five of the seven pages of my summary of the astronomy decade survey in the April 1991 issue (page 24). Two of the 14 outstanding achievements of the previous decade listed in the summary refer to solar astronomy, helioseismology (carried out with superb precision at Harold Zirin's Big Bear Solar Observatory) and solar-neutrino experiments.

The editor of PHYSICS TODAY—a widely respected female physicist—and I jointly selected the topics and authors for the April 1991 issue, based on what we believed to be of most interest to the magazine's readers.

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Theory Lack Shouldn't Prevent Publication

Philip W. Anderson has read my inmost thoughts! Although he refers to solid-state physics in his Reference Frame column "Solid-State Experimentalists: Theory Should Be on Tap, Not on Top" (September 1990, page 9), one thing he writes applies in general to most fields of physics, and most certainly to my field of experimental plasma physics: "Much more serious is the distortion of priorities, of communication and of the refereeing process that occurs when excessive weight is given to theoretical interpretation. We don't want to lose sight of the fundamental fact that the most important experimental results are precisely those that do *not* have a theoretical interpretation."

In spite of being an experimentalist, until recently I was (for 17 years) a member of an institute for theoretical physics. I often tried in vain to inculcate in my colleagues there the understanding that physics is not only (mathematical) theory but that there are such things as phenomenological models, which an experimentalist has to develop at first, simply as a strategy.

Let us assume you find during experimentation that a phenomenon that has been known for a long time suddenly shows some new features that disagree with the hitherto applied and generally accepted theory. This theory may have rested on a number of simplifying, even incorrect, insufficient or inapplicable assumptions that are at odds with the experimental arrangement. Of course this fact was also known to other scientists who had investigated the phenomenon, but some of them preferred to ignore it, since a number of experimental data agreed quite well with the theory.

In view of your new results you are forced to reconsider the old theory, and you develop a new, phenomenological model capable of explaining them. Of course you try to find a theorist who has time and interest enough to develop a new theoretical model. However, in view of the complexity of the system this will keep your theoretical colleague busy for several years. But in the meantime you want to publish proudly the results of your experiments together