conference reported on fluorinated materials, an eloquent testimony to the interest in such alloys. Hiroyoshi Matsumura and his collaborators (Hiroshima University) corroborated[8] the improvements in both stability and defect density of amorphous-silicon-based materials that are directly attributable to the introduction of fluorine, while Shunri Oda and his group (Tokyo Institute of Technology) and Shinya Tsuda's group at Sanyo Electric Co both confirmed[9,10] our previously reported result that fluorine sharply improves the quality of amorphous silicon–germanium alloys to levels acceptable for use in devices.

Paul and Ehrenreich state, "Thus far, the effect of the addition of fluorine to the glow-discharge plasma has been insufficiently documented in the literature for a final conclusion regarding its efficacy." They also imply that even A. Madan has some doubts in this regard. We find it interesting that both Madan and Paul coauthored papers at the Rome conference on just this subject. A. H. Mahan, B. von Roedern and Madan found[11] that incorporation of fluorine in amorphous silicon–carbon alloys decreased the density of defect states and increased the photoconductivity. A recent paper by Paul's group showed[12] that fluorine provided "an order-of-magnitude improvement in the photoconductivity" of amorphous silicon–germanium alloys and presented as their major conclusion, "The effect of fluorine is beneficial." We defer to this objective source for the final word.

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The authors reply: The opening broadside notwithstanding, the letter by S. R. Ovshinsky and D. Adler does not address the historical perspective concerning the development of the amorphous-semiconductor field that we discussed in our letter.

We wish only to comment on the objections to our conservative opinion that any beneficial effects flourine may have are not yet well understood. The Ovshinsky–Adler discussion confuses the facts of the matter by failing to distinguish between the effects of incorporation of about one atomic percent of flourine in the film and the effects of flourine in the plasma.[1] The real cause of the observed improvements in photoconductivity and device stability remains to be established by experiment.

We are entirely in agreement with Ovshinsky and Adler that amorphous semiconductors have become increasingly important and that the contributions of Ovshinsky and his associates (including Adler and Hellmut Fritzsche) have had an important catalytic effect on their development. The latter point was already stressed[2] in an early National Materials Advisory Board study chaired by one of us.

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Proton storage ring

Your "Search and discovery" story (June, page 21) reported the news of the first operation of the new Proton Stor-

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age Ring at the Los Alamos Neutron Scattering Center. As the story said, the Proton Storage Ring will dramatically upgrade the brightness of the LANSCE source and, when it is fully operational, will add significantly to the nation's neutron-beam-research capabilities. We might add that since your story was published, Los Alamos has obtained initial funding for the construction of new and expanded experimental facilities at LANSCE, as was recommended by several national review committees.

The optimal development of large national facilities, such as pulsed neutron sources, for condensed-matter research depends on many factors, of which source brightness is only one. Others include experimental-support facilities, quality instrumentation, experienced staff and accessibility to the user community. For the benefit of those interested in using the pulsed sources at Argonne (IPNS) and Los Alamos, we would like to correct a numerical point and to expand on the story to provide a clearer picture of developments in the US pulsed-neutron-source program.

With regard to the numerical point, the article incorrectly states that the design performance at LANSCE will yield peak and average intensities 60 and 20 times higher, respectively, than those currently obtained at IPNS. The correct comparison, from a simple ratio of proton currents, energies and repetition rates, gives values of 34 and 13. These comparisons assume the use of uranium targets, which Argonne has been using since 1981 and which Los Alamos plans to be using in the near future.

It should be remembered that the relevance of peak- or average-intensity parameters depends on the type of experiment to be conducted. Moreover, the comparisons we are discussing can be altered by improvements that both laboratories plan to make in their accelerators, moderators, cold moderators and targets.

The focus of your excellent story was, of course, on the most recent developments at Los Alamos, but your readers should also be made aware of the facility at Argonne. The Intense Pulsed Neutron Source has operated as a national user facility since 1981. It has played a vital role in the development of a national following for pulsed-neutron scattering. Argonne's research experience and technical innovations have played an important role in the development of more intense sources, such as LANSCE and the spallation source ISIS at Britain's Rutherford Appleton Laboratory, and have made possible further improvements in IPNS as well as the development of better reflectors, cold sources, uranium targets, critical reflection of polarized neutrons and scattering instruments of many kinds.

Argonne and Los Alamos are committed to working together toward the full development of pulsed-neutron scattering research in the US. We believe that a healthy national condensed-matter-research program should provide *both* for full development in the 1990s of LANSCE as the most intense US pulsed source and for continued funding of the innovative, pioneering program at IPNS.

We hope that this letter provides some clarification of the picture of US pulsed-neutron-source developments drawn by your story.

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The magnetron

James E. Brittain's article "The magnetron and the beginnings of the microwave age" (July, page 60) is a well-written and valuable contribution to microwave history in many respects, but unfortunately it represents a miscarriage of historical effort in several areas. Clearly the article is intended to be a history of microwave technology, yet about 20% of the text and three of the five figures relate to the Albert W. Hull type of magnetron, which is far from a microwave device. It was considered mainly for very-low-frequency applications. In a paper on microwaves it deserves far less emphasis than that given it by Brittain.

On the other hand, whereas Brittain has overemphasized Hull's work, he has underemphasized, and in fact seems, to a surprising degree, unaware of, much early work done at RCA[1] on microwave magnetrons and on microwave technology in general. In the 1930s and early 1940s a group at the RCA research department in Camden, New Jersey, carried out research in the range from 3 to 10 centimeters. Unfortunately I do not have available data on publications by other members of the group, but my own efforts as a member resulted in the publication of 17 microwave papers, 9 of which were directly related to microwave magnetrons. Some of the more pertinent are listed in the reference below, and further references are obtainable from those listed. It would seem that Brittain was rather negligent in his literature search not to have noticed these articles. They appeared in leading scientific journals. Also, my work resulted in 71 US patents in the general microwave field, of which 28 related directly to microwave magnetrons. It is difficult to understand how any competent science historian could overlook such extensive work.

In addition, the sole reference that Brittain does make to our work at RCA Camden is incorrect. When he speaks of the radar tests that we made across New York Bay in August 1934 using a split-anode magnetron operating at 9 cm, he states that "the group led by Hershberger conducted joint experiments with RCA engineers." These tests were not jointly made. They were carried out solely by RCA personnel, Irving Wolff and myself, using transmitting and receiving equipment as well as other microwave instrumentation designed and supplied exclusively by RCA. W. Delmar Hershberger, who was a Signal Corps engineer at that time, had invited RCA to demonstrate its equipment to the Signal Corps Laboratories, but neither he nor any other Signal Corps personnel participated in the tests. They were only observers (see PHYSICS TODAY, October 1983, page 119).

The RCA work during the 1930s and early 1940s was wide in scope, including magnetron oscillators, detectors, modulators, power meters, wavemeters, attenuators and filters, as well as complete experimental radar and communication systems. This work is well documented by publications, reports, patents, photographs and devices now in my possession. In 1940 the emerging MIT Radiation Laboratory was supplied by us with two signal generators operating in the 3000–4000 MHz range, along with other instrumentation (see PHYSICS TODAY, June 1983, page 103). Such equipment was not available elsewhere at that time.

Brittain's article makes interesting reading for all of us who participated in the beginnings of the microwave age, but it is unfortunate from a historical standpoint that his coverage was not more comprehensive and more correctly distributed.

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BRITTAIN REPLIES: I am pleased that my recent article has stimulated Ernest G. Linder to share some of his reminiscences about the early work on magnetrons at RCA. Unfortunately, his assumption that I overlooked the publications by him and his colleagues in the 1930s is not correct. The bibliography cited in reference 9 of the article included five papers published by Linder, and I located all of them during my research. My notes even include biographical data on Linder. The space available for the article and editorial policy with regard to source notes made it necessary for me to omit much interesting material as well as most of the references that I consulted: An early draft of the paper contained 76 source notes but I was asked to reduce these substantially, and ultimately only 12 were retained. My file of research notes indicates that I read more than a hundred papers and scanned over many more than that. I must say that I was quite surprised to learn just how many papers were published on the magnetron prior to 1940. I now believe that a book might be written on the subject and I hope that Linder and other living pioneers will make available to scholars whatever records, reminiscences and unpublished documentary materials they have. Oral interviews also would be very helpful to historians of developments in vacuum-tube electronics and microwave engineering from 1920 to 1940.

With regard to Linder's comments on emphasis in my paper, I can only say that my intention was to try to identify and trace the social and intellectual threads from the Hull magnetron to the Boot and Randall magnetron. I simply do not agree with Linder's assertion that "clearly the article is intended to be a history of microwave technology." I can understand that a former RCA employee might feel that work there deserved more coverage. However, as I hope the paper suggests, magnetron work took place in many companies and many countries, and others may also feel that their work should have received more emphasis. Inevitably, selection of what to include and what themes to stress in a short paper becomes a matter for the author's judgment and no two historians are apt to reach the same decisions.

I am glad to have Linder's clarification of the 1934 experiments. My interpretation that he questions was based on my reading of a copy of W. Delmar Hershberger's unpublished manuscript entitled "History of radar before 1941" with which he provided me. This stated that "communication experiments are described by Wolff, Linder, and Braden and a number of these were conducted cooperatively by RCA and SCL at Fort Monmouth and Fort Hancock in the summer of 1934."