# letters

*continued from page 15*

reads about the Dundee and RCA work on a-Si:H in forms similar to Fritzsche's version cited above. While there may be nuances of this history of which we are unaware, we believe that we have accurately described the main events of its development.

### References


1. R. C. Chittick, J. H. Alexander, H. F. Sterling, J. Electrochem. Soc. **116**, 77 (1969).
2. H. F. Sterling, Standard Telecommunications Ltd, Report No. 1403–1955–99.
3. W. E. Spear, P. G. LeComber, J. Non-Cryst. Solids, **8–10**, 727 (1972).
4. W. E. Spear, P. G. LeComber, Solid State Commun. **17**, 1193 (1975).
5. D. E. Carlson, C. R. Wronski, Appl. Phys. Lett. **28**, 671 (1976).
6. A. J. Lewis Jr, G. A. N. Connell, W. Paul, J. R. Pawlik, R. J. Temkin, in *Tetrahedrally Bonded Amorphous Semiconductors*, M. H. Brodsky, S. Kirkpatrick, D. Weaire, eds., AIP Conf. Proc. **20** (1974) p. 27.
7. W. Paul, A. J. Lewis Jr, G. A. N. Connell, T. D. Moustakas, Solid State Commun. **20**, 969 (1976).
8. A. Triska, D. Dennison, H. Fritzsche, Bull. Am. Phys. Soc. **20**, 392 (1975).
9. A. Madan, S. R. Ovshinsky, E. Benn, Philos. Mag. **40**, 259 (1978).
10. A. Madan, in *The Physics of Hydrogenated Amorphous Silicon II, Topics in Applied Physics, Vol. 55*, J. D. Joannopoulos, G. Lucovsky, eds., Springer-Verlag (1984).



WILLIAM PAUL
HENRY EHRENREICH
*4/85* *Harvard University*


## Tokamak history

I have read Harold Furth's article on "Reaching ignition in the tokamak" (March, page 52) with great interest. I have found the overview of the work performed to date on tokamak ignition devices to be remiss. It completely omits the major design and development work performed by INESCO Inc on the tokamak ignition device, FDX, which was first proposed by Robert Bussard and Bruno Coppi in 1977 and completely funded by private sources ($17 million spent 1980–84). The results and descriptions of the design effort (PHYSICS TODAY, May 1981, page 17) were published extensively in scientific journals and presented at national and international conferences, national labs, as well as at universities, and even to the US Congress.

The engineering design studies performed by INESCO Inc defined[1] a wide range of phase space in which these highly compact, water-cooled copper tokamaks could potentially attain ignition and burn conditions. The feasibility of construction of low-aspect-ratio $R/a$ tokamaks that are capable of attaining ignition and high $\beta$ was demonstrated. The "steady-state" nature of the cooling systems allows these FDX tokamaks to be used in a long burn mode (greater than 10 sec), which would allow for equilibrium of the burn. In his article, Furth claims a 1-sec burn limit for subcompact tokamaks.

In addition, I would like to point out an error in reference 6. The correct reference is: S. N. Rosenwasser, R. D. Stevenson, G. Listvinsky, D. L. Vrable, J. E. McGregor, N. Nir, *J. Nucl. Mater.* **122 & 123**, 1107 (1984)—all INESCO Inc employees at the time of publication.

It is important to note that the reference details the advantages and viability of compact copper reactors such as the Riggatron, as well as subcompact ignition tokamaks such as FDX, in contrast to the allusion of relying on this reference to demonstrate the improbable future of copper reactors and the supposed practicality of superconducting tokamak reactors.

It is unfortunate that valuable work that has not depended on taxpayers' resources is totally ignored by a leading scientist who is solely dependent on the Federal tax till. Logic would have dictated that cost sharing between the Federal government and private industry would be encouraged and that scientists supported by the public would elicit private contributions that enhance the technical base of the fusion program. The opposite has proven to be the case, shattering my naiveté.

### Reference


1. R. A. Jacobsen, C. E. Wagner, R. E. Covert, J. Fusion Energy **3**, 4 (1983).



RAMY A. SHANNY
*5/85* *La Jolla, California*


THE AUTHOR COMMENTS: My article "Reaching ignition in the tokamak" cites Bruno Coppi as the chief advocate of the high-field approach to ignition, because Coppi introduced the basic idea as well as the most interesting variations.

During the late 1970s and early 1980s, a number of high-field ignition devices were proposed—among them the FDX, which Ramy Shanny mentions, and the ZEPHYR, which was based[1] on a major design study by the Max Planck Institut für Plasmaphysik in Garching, Germany. Generally speaking, the INESCO and Garching groups reached opposite conclusions, but both studies produced creative ideas and significant technical data. A more detailed history of tokamak ignition projects would have included an appreciation of both the FDX and ZEPHYR—and several others as well.

The main objective of INESCO's design work was to build a compact commercial D-T tokamak reactor (the Riggatron), which has its tritium-breeding blanket *outside* the magnet coils. My article did refer to this INESCO concept, because of its uniqueness. I regret the inaccuracy of the author listing in my reference to the INESCO work.

### Reference


1. C. Andelfinger, *et al.*, Z. Naturforsch. **379**, 912 (1982).



HAROLD P. FURTH
*5/85* *Princeton University*


## Sabine and acoustics

Leo L. Beranek provided us with a fascinating article on "Wallace Clement Sabine and acoustics" (February, page 44). It was revealing to learn how long and hard Sabine worked to come up with the first practical room-reverberation formula, and then to apply it in the design and construction of auditoriums in his era. He was truly a genius in acoustics.

However, I would like to suggest a modest correction involving the period of Sabine's graduate work at Harvard University. Based[1] on information in Dana Orcutt's biography of Sabine, the article states that, "At the end of his first year at Harvard (1887), he was awarded a two-year Morgan Fellowship . . .," and also that "During the next two summers he supplemented his fellowship stipend with employment at the Bell Telephone Laboratories."

The latter statement is in error, but only because the name of the company is out of place with the time of the events. After a bit of trivial pursuit, I found that telephone research in that period was conducted[2] for a number of years (starting in 1885) in a laboratory of the mechanical department at the American Bell Telephone Company (soon to become AT&T) on 141 Pearl Street in Boston. Sabine must have worked there. Quoting my reference source, "This was, in effect, the first formal organization in the continuous chain of research and development organizations leading to the present Bell Telephone Laboratories."

Having been employed by Bell Labs myself in its early days, I was well aware that the company was actually formed in 1925 as a distinct corporation in the transition from the old Western Electric engineering department at 463 West Street in New York City. As time progressed, the various laboratory divisions were relocated in New Jersey and other parts of the country. Finally, as a result of the Bell System divestiture in 1984, the company name has now been

modified to that as shown associated with my signature below.

### References


1. D. Orcutt, *Wallace Clement Sabine: A Biography*, Plympton, Norwood, Mass. (1933).
2. M. D. Fagen, ed. *A History of Engineering and Science in the Bell System: The Early Years, 1875–1925.* Bell Telephone Laboratories, Inc, 1975, p. 38; p. 54.



FLOYD K. HARVEY, RETIRED
*AT&T Bell Laboratories*
*Murray Hill, New Jersey*


*4/85*

## US–USSR scientific exchanges

I favor US–USSR scientific exchanges as a small but direct way to lessen East–West tensions. Within this context I question both the accuracy and the relevance of G. Truman Hunter's statement (December, page 15) that "Any Russian physicist allowed to come to the United States, or allowed to communicate with Americans, can be assumed to be completely dominated by the Russian government. . . ." If loyalty to one's government is meant, then the statement lacks significant content. If something more is meant—such as government briefing and debriefing, special indoctrination, and so on—then before sinister implications are drawn concerning the USSR only, I think one should first distinguish Soviet practice from that of the US.

However, the question of government domination of an individual is irrelevant. If scientific exchange is accepted as desirable, then the most important question is whether those participating are bona fide scientists—and even here there is no reason not to accept the sending country's criteria. When a visiting scientist lives and works in a host scientific community for a period of months, there are many opportunities for social and cultural interaction. The positive (as well as the negative) aspects of the host community can become apparent to the visiting scholar. A deeply ideological individual is as susceptible to enlightenment as anyone else—it's a matter of degree. Consequently, if an American is convinced his own culture is in some sense superior, then I should think he would be eager to let a visitor view it at close hand.

My own experience with Soviet exchange scientists at SLAC has been uniformly positive, and I believe that these visitors have returned to the Soviet Union with a much improved perception of what the American people are like and that this experience will have some small influence for good in improving US–USSR relations.


JAMES E. CLENDENIN
*SLAC*
*Stanford, California*


*1/85*

THE AUTHOR COMMENTS: Clendenin questions my statement that any Russian scientist in close contact with the US is completely dominated by the Soviet government. Why does the KGB closely supervise all culture groups and all Soviet citizens who work for the United Nations? Arkady N. Shevchenko's recently published *Breaking with Moscow* tells[1] in considerable detail how the KGB watches and controls *every* Soviet person.

I do not doubt that a scientist in another country has *some* opportunities for social and cultural interaction. But they are far fewer in Soviet-dominated countries than in America. I don't mind letting a visitor view American culture at close hand. What I object to is that visitor stealing everything he or she can get away with, and leaving as little as he or she has to. If Clendenin doesn't think that is the situation between the closed society of the Soviet Union and the open society of America, he is badly mistaken. I object to our giving the Soviets one iota of physics information that will help them to shoot down airliners faster, to improve the high-tech level of the infamous German slave wall, or to increase the jamming ability of radio and television broadcasts.

*Techno-bandits*, by Linda Melvern, David Hebditch and Nick Anning, details[2] only a few of the recent cases of Soviet spying and stealing of Western technology. The three-volume *Western Technology and Soviet Economic Development, 1917 to 1965*, by Antony Sutton, details[3] how the Soviets got much of their technology, including a fair amount by expulsion and expropriation. I do not deny that there are some excellent scientists and engineers in Russia, but they won't admit how much they have stolen. The way the Soviets rewrite history, they will soon be claiming to have invented French fries and Brussels sprouts!

Clendenin says, "Government domination of an individual is irrelevant." That is ridiculous! *Psychiatric Terror*, by Bloch and Reddaway, will give the reader a pretty good idea of how much dissent and change any Soviet scientist can make. One statement that the United States is good at anything, and the individual is done for. Clendenin should also read Valentin Turchin's letter (April 1985, page 104) titled "Fellow travelers"; Turchin is more eloquent than I.

PHYSICS TODAY has published more than two dozen letters in the past three years that further document the monstrous inhumanity of the Soviets and the selfishness of the few Americans who just want to enjoy their own pleasures and ignore murder, slavery, torture, deceit and anti-Semitism—all of which are the normal operations of Soviets, when needed for their purposes, and carefully hidden when they visit the US and try to get something they want for free.

Clendenin believes that Soviet exchange scientists will go back and have a small influence on improving US–USSR relations. What influence did Andrei Sakharov have when he simply wanted the Soviets to abide by the Helsinki document they signed? Ask the hundreds of other scientists and engineers who have voiced any criticism of Soviet government how nicely they have been treated by the Soviets.

Can Clendenin get any one of his Soviet visitors to sign a statement to be published in PHYSICS TODAY that the Soviet Union should allow more freedom of expression, information and travel without KGB supervision? The poor victim would never be heard from again! As to the small influence his returning visitors will have, I can quantify it for him. It is $10^{-99}$ in any units he wants to use.

### References


1. A. N. Shevchenko, *Breaking with Moscow*, Knopf, New York (1985).
2. L. Melvern, D. Hebditch, N. Anning, *Techno-bandits*, Houghton Mifflin, New York (1984).
3. A. Sutton, *Western Technology and Soviet Economic Development, 1917 to 1965*, Vols. 1-3, Hoover Institution P. of Stanford Univ., Stanford, Calif. (1973).
4. Bloch, Reddaway, *Psychiatric Terror*, Basic (1977).



TRUMAN HUNTER
*Oxford, Ohio*


*5/85*

## Science at GE

Your interesting article on Science at GE (December, page 61) contains a photograph of Nobel laureates Percy W. Bridgman, Harold Urey and Irving Langmuir (from left) at the opening of the GE Research Laboratory in 1950.

If I am not mistaken the author has missed another Nobel laureate in the picture; the man fourth from the left appears to be W. Lawrence Bragg.

Who is the gentleman on the right, I wonder?


JOHN G. JENKIN
*La Trobe University*
*Bundoora, Australia*


*2/85*

## Correction

**June, page 89**—The story on the recent ASP awards should have stated that Dale P. Cruickshank joined the Institute for Astronomy of the University of Hawaii in 1970. □