Teller comments:

I remember having walked over with Fermi and others to Fuller Lodge for lunch. While we walked over, there was a conversation which I believed to have been quite brief and superficial on a subject only vaguely connected with space travel. I have a vague recollection, which may not be accurate, that we talked about flying saucers and the obvious statement that flying saucers are not real. I also remember that Fermi explicitly raised the question, and I think he directed it at me, "Edward, what do you think? How probable is it that within the next ten years we shall have clear evidence of a material object moving faster than light?" I remember that my answer was "10^{-6}." Fermi said, "This is much too low. The probability is more like ten percent" (the well-known figure for a Fermi miracle).

Konopinski says that he does not recall the numerical values, "except that they changed rapidly as Edward and Fermi bounced arguments off each other."

Teller continues: "The conversation, according to my memory, was only vaguely connected with astronautics, partly on account of flying saucers might be due to extraterrestrial people (here I believe the remarks were purely negative), and partly because exceeding light velocity would make interstellar travel one degree more real."

"It was after we were at the luncheon table," Konopinski recalls, "that Fermi surprised us with the question 'but where is everybody?' It was his way of putting it that drew laughs from us."

York, who does not recall the preliminary conversation on the walk to Fuller Lodge, does remember that "virtually apropos of nothing, Fermi said, 'Don't you ever wonder where everybody is?' Somehow we all knew he meant extraterrestrials."

Teller remembers the question in much the same way: "The discussion had nothing to do with astronomy or with extraterrestrial beings. I think it was some down-to-earth topic. Then, in the middle of this conversation, Fermi came out with the quite unexpected question 'Where is everybody?' The result of his question was general laughter because of the strange fact that in spite of Fermi's question coming from the clear blue, everybody around the table seemed to understand at once that he was talking about extraterrestrial life. I do not believe that much came of this conversation, except perhaps as a statement that the distances to the next location of living beings may be very great and that, indeed, as far as our galaxy is concerned, we are living somewhere in the sticks, far removed from the metropolitan area of the galactic center."

York believes that Fermi was somewhat more expansive and "followed up with a series of calculations on the probability of earthlike planets, the probability of life given an Earth, the probability of humans given life, the likely rise and duration of high technology, and so on. He concluded on the basis of such calculations that we ought to have been visited long ago and many times over. As I recall, he went on to conclude that the reason we had not been visited might be that interstellar flight is impossible, or, if it is possible, always judged to be not worth the effort, or technological civilization does not last long enough for it to happen." York confessed to being hazy about these last remarks.

In summary, Fermi did ask the question and, perhaps not surprisingly, issues still debated today were part of the discussion. Certainly the line of argument that York remembers became familiar[6,7] a decade later as the Drake–Green Bank Equation.

As for the date of conversation, the best evidence comes from the cartoon Konopinski mentioned. Drawn by Alan Dunn, it appeared in the 20 May 1950 issue of *The New Yorker*. Konopinski says that the cartoon had been fresh in his mind. A summer 1950 date also agrees with York's recollection that the purpose of his visit was preparation for a nuclear test that occurred in May of the following year.

* * *

I am grateful to Hans Mark and to the three surviving participants for their help. I also thank Helen Stark, a librarian at The New Yorker *who located the Dunn cartoon.*

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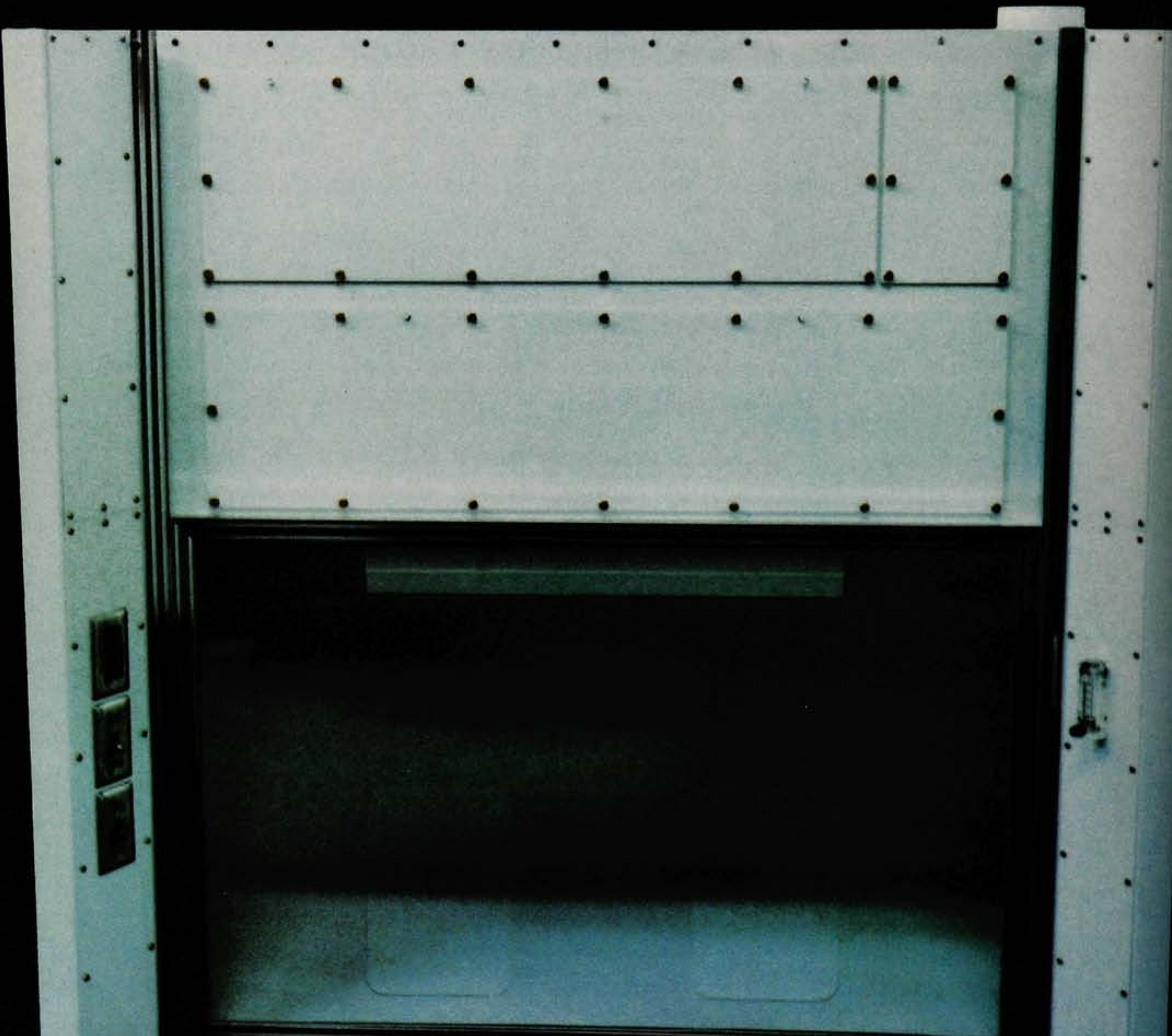
Noncrystalline semiconductors

In the absence of a single seminal paper outlining the study of amorphous hy-

All Radiological Laboratory Hoods Are Alike.



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NEW PRODUCTS FROM NPI • Call our Toll Free Customer Service Number • 1-800-821-0547 drogenated silicon from a historical perspective, it is perhaps natural that there are conflicting opinions concerning the development of this field. Unfortunately, the ambiguous wording of a recent article by Hellmut Fritzsche (October, page 34) seems likely to perpetuate and even extend some prevalent misconceptions. Fritzsche has written, "... Walter A. Spear and Peter LeComber of Dundee University in Scotland, and Ovshinsky, found that by attaching hydrogen or fluorine to the dangling bonds, one can reduce the number of defect states in the gap from 1 percent to 10^{-5} percent of silicon atoms." While this statement may be correct, it does not accurately describe the history of the development of the material, which we shall now amplify.

The first study on identified amorphous silicon and germanium deposited from the glow discharge decomposition of SiH_4 and GeH_4 was published[1] by the English group of R. C. Chittick and his coworkers in 1969. The material produced had radically different properties from that made by evaporation or sputtering, and so glow discharge was not at the time adopted as a preferred method of production. There is no reference in the published work to the presence of hydrogen in the films, although infrared absorption spectra and photoconductivity were measured. Preliminary demonstrations of large decreases in resistivity by two orders of magnitude on including PH_3 in the plasma—presumed to be phosphorus-doping effects—were reported. Despite the fact that the work was known to a wide circle of investigators (including one of the present authors, William Paul), only the Dundee group is known to have followed up directly on this work at Standard Telecommunications Laboratories (which, indeed, began[2] as early as 1955). After demonstrating[3] from field-effect measurements that amorphous silicon so produced possessed a low density of states in the band gap, they showed[4] in 1975 that the material could be doped n- or p-type by adding PH_3 or B_2H_6 to the SiH_4 plasma. Shortly thereafter it became known that D. E. Carlson and C. R. Wronski of RCA had achieved[5] similar doping effects independently in the same general period of time, as part of extensive company-confidential research on a-Si solar cells. These research efforts can justly be claimed as marking the beginning of the explosive growth of this field on an international scale. It has perhaps not been sufficiently recognized that these studies were bucking the conventional belief that doping was very unlikely, because every impurity atom would achieve its natural valence in an amorphous structure. Their authors certainly deserve the recognition they have been accorded.

Nevertheless, neither Spear and LeComber nor Carlson and Wronski recognized the role played by hydrogen in reducing the density of states in the gap. The Dundee group believed (with considerable justification) that they were employing a very gentle method of deposition that led to fewer incorporated defects than either evaporation or sputtering. This view is displayed consistently in their early papers, which make no reference to the presence of hydrogen in their films, and was defended at numerous international conferences. Similarly, the early RCA reports make no reference to hydrogen, which appears to have been recognized by them about the time of the Williamsburg Conference of 1976. It is to be noted that for the device development that was their goal, the primary role of hydrogen was not particularly relevant.

The first experiments on the *deliberate* introduction of hydrogen into a sputtering plasma to modify a similar material, amorphous germanium, by eliminating its dangling bonds were reported[6] by A. J. Lewis and his collaborators at a conference in Yorktown Heights, New York, in 1974. They showed that the systematic addition of hydrogen to a-Ge decreased the electron-spin-resonance dangling-bond density and increased the resistivity by several orders of magnitude, while also increasing the optical band gap and developing Ge–H infrared vibrational absorption. They then recommended the obvious: that a-Si produced from SiH_4 contained hydrogen. After the demonstration of doping in 1975, the same group doped a-Si:H n-type or p-type by adding PH_3 or B_2H_6 to an argon plasma sputtering Si, made p–n junctions, and otherwise demonstrated[7] a second method of fabrication of a-Si:H films. When Fritzsche and his coworkers demonstrated[8] from an evolution experiment that a-Si produced from SiH_4 actually did contain hydrogen, there was little doubt left about the primary importance of the H content.

In 1978 Madan, Stanford Ovshinsky and E. Benn claimed[9] improved properties for a-Si incorporating both hydrogen and fluorine. 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. In a recent review article, Madan explicitly states[10] that the philosophy of adding fluorine was derived from the success of the hydrogenation method.

Few reviewers have resisted the temptation to rewrite the history of this field so as to make it appear more logical and orderly, and so one often

continued on page 78

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.

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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 β 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.

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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.

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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