in which it was born and by which it was nourished?

I suggest that such a wrenching action is unnecessary. The term "low" in "low-temperature physics" has meaning only when referred to a reference temperature; let us call it T_0. The early cryogenic engineers, not unreasonably, thought of T_0 as being around room temperature. But this temperature holds no special position in physics. Rather, T_0 must be defined as the characteristic temperature (or energy) that is identified with a particular physical phenomenon, often a phase transition. Then the *low-temperature physics* of that phenomenon is its study at $0 < T \lesssim T_0$. Some obvious examples of T_0: the Fermi temperature for a fermion gas, the Debye temperature for a phonon gas, the Curie temperature for a magnon gas, the energy gap Δ_0/k_B for a superconductor.

Let us not take the beef out of the hamburger.

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6/87

Fusion Community's 'Voice' at DOE

In Raghavan Jayakumar's thoughtful letter "Uniting the Fusion Community" (June 1987, page 11) he mentioned the need for "an elected or appointed group [to] develop an overall research strategy based on [a] review [of research concepts] and on its own collective wisdom," as well as for the "leadership of the fusion plasma community to begin a movement of consolidation and lead the researchers into developing a consensus on research approaches."

I wish to point out that such a group, performing essentially the suggested functions, already exists. It is the Magnetic Fusion Advisory Committee, appointed by and reporting to the Director of Energy Research in the US Department of Energy, which is the main administrative agency for US fusion research. This committee meets quarterly in public session. Over the years since 1982 MFAC has involved the fusion community extensively in such basic issues as DOE program priorities, detailed fusion program planning and the roles of the universities and industry. Various aspects of research in tokamaks (the leading concept) have been examined in detail: upgrade options for the Princeton Tokamak Fusion Test Reactor experiment and, currently, its usefulness for burning plasma physics using deuterium–tritium plasma, as well as US options for ignition experiments. After assessing the prospects of the Compact Ignition Tokamak, MFAC and its panel endorsed construction of the device (as did the DOE Energy Research Advisory Board). MFAC also examined progress in the open-ended, tandem magnetic mirror system, concluding in the face of declining budgets that it should be discontinued as the major alternative to the tokamak. MFAC and the community also examined the various alternative toroidal fusion concepts, and provided strong support for the stellarator and reversed-field pinch, as well as for compact toroidal systems. In technology, systems studies have been encouraged to define the fusion reactor end product, particularly in respect to necessary power density related to the mass of the fusion core.

These in-depth studies have involved scores of physicists, engineers and managers from the national laboratories, universities, industry, government agencies and the public. The findings are submitted to the Director of Energy Research and usually result in concrete action.

A word about Jayakumar's concerns for cohesion and outside support of major new initiatives. As mentioned, the CIT, a $300 million ignition experiment, is the major new US magnetic fusion device. It now has Administration funding support and has been favorably reviewed by Congress. Prospects for construction are bright because of unified community support. The fusion program seems to me to have a uniform purpose, not only for tokamak research but for research in alternative concepts, technology and basic fusion physics, even in the face of tight budgets.

We hope, as Jayakumar suggests, to keep the spirit of cooperation in the fusion community alive by continuing the processes I have discussed in this letter.

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7/87

Mirrors in Space: How Costly?

To avoid any misunderstanding regarding our closing statement in the debate on the APS directed-energy weapons study (PHYSICS TODAY, No-

continued on page 112

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vember, page 48), we produce here, at the request of Russell Seitz, the text pertaining to the cost of a space-based mirror. The original version, appearing on page 179 of the APS study on directed-energy weapons, released in April 1987, reads, "The largest high-quality space-weight mirror that has been fabricated in this country to date is the 2.4-m Hubble Space Telescope primary mirror, which took six years to fabricate at a total cost of approximately \$1.2 billion[1] in 1984 dollars." (Reference 1 is to the article by G. Field and D. Spergel, *Science* **231**, 1387 [1986], which makes clear that total system cost of the telescope deployed in orbit is implied.) The original version was modified, following discussions with Seitz (referred to in PHYSICS TODAY, November, page 53), to discourage misreading on anyone's part. The text published in *Reviews of Modern Physics*[1] reads: "The largest high-quality space-weight mirror that has been fabricated in this country to date is the 2.4-m Hubble Space Telescope primary mirror, which took six years to fabricate at a total system cost of approximately \$1.2 billion in 1984 dollars. Not including development costs, the primary mirror itself cost approximately \$5.5M."

Following a suggestion by Seitz, the *RMP* version also corrected the text relating to equation 5.1. Neither of these corrections has had a direct bearing on the conclusions of the report.

We take this opportunity to emphasize that further technological discussion of these and other points in the report should be based on the text published in *RMP*.

Reference

1. American Physical Society Study Group on Science and Technology of Directed Energy Weapons (N. Bloembergen, C. K. N. Patel, cochairmen), Rev. Mod. Phys. **59**(3), part II (July 1987).

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Remembering PSSC's Early Days

I read with interest the letters on "The PSSC Course in Retrospect" (April 1987, page 11). As I was involved to some degree in that undertaking I would like to add a few comments.

I saw in 1949 that there was too little science taught to elementary-school children. My wife and I originated the idea of starting a "science club" outside of school hours. There was an immediate and enthusiastic response.

I did simple experiments in basic classical physics—heat, light, sound, electricity and magnetism, and mechanics—with readily available materials. The children were encouraged to repeat some of the experiments with their own hands. The demonstrations were accompanied by explanations couched in nontechnical and nonmathematical language easily understood by the youngsters. There were many, many intelligent questions, which told me that the intelligence of these kids was not to be underrated. It also indicated in no uncertain terms that the hunger for scientific knowledge was very great.

We tried to interest the local public school authorities in this project but their response was less than cordial, or perhaps we didn't sell it hard enough. The sessions became unmanageably large, ultimately forcing us to abandon the project. The youngsters were disappointed—and so were we.

At this time I was an engineering research associate on the staff of MIT, working with Jerrold R. Zacharias and others on various projects in basic physical research. One evening we invited Zacharias and his wife, Leona, to our home for dinner, in the course of which I talked glowingly about the science club. As I described the venture we could see the professor's eyes light up, although beyond a few perfunctory questions he made little comment.

Not long afterward Zacharias invited me to participate in a group that met at the MIT Faculty Club and that soon became known as the Physical Science Study Committee. I attended these meetings after my regular hours at the laboratory, where, among other things, I worked on several ideas for devices that could possibly be made by high-school students with easily obtainable materials and simple tools. One such device was a triode vacuum tube in a glass decaffeinated-coffee jar.

At the PSSC meetings there was a good deal of erudite scientific discussion, and some disagreement; a certain amount of this talk was over my head, to the extent that I often felt small and out of place. Whenever I had the opportunity I expressed the feeling that what the committee was planning was too advanced for beginners in high-school physics—that it would have a negative effect because, simply, it was too advanced for *me*, and up to that time I had been working in various scientific fields for more than 25 years as an innovator and inventor of mechanical, chemical, optical, electrical and electronic devices, at Columbia University and at MIT. (I retired from the MIT staff in 1970 at the age of 69, after 28 years—in the Radiation Laboratory, in Basic Research and in the Research Laboratory of Electronics. I wrote a handbook on the techniques of making vacuum tubes and other evacuated devices; the latest edition was published by Addison–Wesley in 1965. It is still in demand. I am also the holder of US patent 2,099,349, issued in 1937, for a pioneering electrometer–pH meter.)

My small voice was more or less drowned out by the savants, with the result that I felt discouraged. If the proceedings and the output of the PSSC at that time fell short of stimulating me, I believed they would not attain the ostensible goal of reaching high-school students in the main. It was a worthwhile idea that missed the mark.

FRED ROSEBURY
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Apologies for a Publication Postponed

I would like to apologize to all whom I assured that the proceedings of the XVIIIth Winter School of Theoretical Physics, held in Karpacz, Poland, from 19 February to 4 March 1981, would be promptly published by Harwood Academic Publishers, London. I quote an excerpt from a letter from the publications editor: "The delay was due to a misunderstanding in-house." The proceedings appeared on the market at the end of 1986 as volume 4 of the series *Studies in High Energy Physics*, entitled *Gauge Field Theories—Theoretical Studies and Computer Simulations*, with me as the editor.

In all the haste of typing the 772 pages of material, the paper containing lecture notes by my colleague Jerzy Lukierski was regrettably omitted. The preprint containing his talk delivered at the school was issued in April 1981, as ITP–University of Wrocław preprint no. 534 under the title *From Supertwistors to Component Superspace*. I apologize to Lukierski for this omission.

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