5. D. Larbalestier, G. Fisk, B. Montgomery, D. Hawksworth, PHYSICS TODAY, March 1986, p. 24.

JOHN R. FANCHI
3/87 *Littleton, Colorado*

THE ASSOCIATE DIRECTOR OF THE SSC CENTRAL DESIGN GROUP REPLIES: It would appear that John R. Fanchi has not taken fully into account the quantitative aspects of the points to which he speaks. Before addressing those points I would like to make it clear that we who have been working on magnets for the SSC have been as excited as anyone about the discovery of new superconducting materials that may have properties that would be very useful for accelerator magnets. Work has been undertaken at high-energy physics laboratories to explore possible applications of these materials and to contribute toward a better understanding of their properties. Furthermore, the SSC Central Design Group is providing modest support for an R&D program by an experienced group of materials scientists and engineers to investigate possible high-energy physics applications of the new materials. However, contrary to the view expressed by Fanchi, it is my observation that as the weeks have passed, the theoretical and experimental experts have become more and more conservative in their projections of the time scale on which the materials might be ready for use and even on the question of the degree to which they may be useful for accelerator magnets.

Now to address Fanchi's points directly. A higher field for the SSC would indeed permit a smaller tunnel, but to reduce the circumference to about one-quarter its designed value would require 4 times the present magnetic field intensity, and that would involve forces about 16 times the present value. In turn, this would require a much heavier magnet structure, if containment is feasible at all. (The present forces already tax normal insulation materials to their limit.) Calculations show that this smaller configuration would cost more, not less.

Fanchi speaks about critical magnetic fields as high as 40–55 T at liquid helium temperature. But producing such a field in anything like an economically feasible magnet design would require very high current densities in the coils. At the moment the current densities that the new materials can support are not as great as those we are achieving in niobium–titanium in the present SSC magnet design. There is no evidence that the new materials can support even the presently quoted currents except at very low temperatures (in the vicinity of that of liquid helium), and I doubt that even the most unbridledly optimistic of those scientists working on the new superconductors woud predict that they would support four times the current density achieved in niobium–titanium.

The enormous progress in elementary-particle physics in the past decade has been built upon the development of an extremely complex infrastructure of physicists, technicians and engineers. One cannot put that complex animal in a holding pattern for a decade or two, to be reawakened at will. Its current high level of capability would quickly decay and diffuse. It would be foolhardy to invite such a process for the sake of gaining an improbable economic advantage that would, at best, be small and that would require, in any case, that a set of materials have properties nature may or may not have bestowed on them.

Research and development of the new superconducting materials should be supported at a level that fits the problems, their tractability, and the number and quality of people available to work on them. That probably will require tens of millions of dollars a year. However, I believe that it is scientifically sound to take simultaneously the next logical step in pursuit of an understanding of the structure of matter and of the forces through which it interacts. I further believe that it would be a sad abdication of our position of scientific and technical strength for us to do otherwise. Fanchi himself says that he embraced that view one year ago—before the new superconducting materials were discovered. I can see no reason to revise that judgment.

EDWIN L. GOLDWASSER
Lawrence Berkeley Laboratory
9/87 *Berkeley, California*

CALIBRATING A BUBBLY UNIVERSE

The proposals for "calibrating with bubbles" in Roger N. Shepard's letter (April 1987, page 114) will not work, at least in the sort of universe most cosmologists think we inhabit.

He proposes inspecting redshift diagrams of galaxy surveys for elongations of the voids as a way of testing the distance scale. However, these diagrams are totally independent of the distance scale, since their units are velocity (redshift) and angles.

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Perhaps if methods for determining distances without using redshift are developed that are very reliable at large distances, we will be able to reliably know the cosmic distance scale.

Any region we can imagine surveying completely in three (or even two) dimensions is so small that the light travel time across it is a small fraction of the age of the universe. Therefore the tests of changes in the geometry of space–time or in the large-scale clustering of galaxies that Shepard proposes would merely show noise. Some studies using narrow, deep "pencil" surveys have been under way for years, trying to accumulate enough data to reliably perform tests related to the ones he proposes, but based on galaxy counts.

Adrian L. Melott
University of Kansas
5/87 *Lawrence, Kansas*

Shepard replies: I assume in my proposal that galaxies manifest redshifts determined by their recessional velocities (at the times their observed energies were emitted), that the distances between the galaxies' locations then and Earth now increase monotonically with those redshifts, and that galaxies are distributed isotropically in space (at any given time). I do not assume knowledge of an independent distance scale or of properties of early galaxies (as is required for inferring distances via "galaxy counts").

I proposed that we estimate distances by monotonically transforming redshifts to render features in the redshift diagram consistent with spatial isotropy. We can remove systematic elongations (or contractions) of intergalactic voids along their radial directions from Earth by differentially contracting (or expanding) corresponding regions of the redshift scale. (We can also conformally map the transformed diagram to a curved manifold, to ensure that perimeters of concentric circles around Earth increase with their radial distances in the nonlinear way that renders the implied smaller scales of the earlier universe consistent with distances in the resulting mapping.)

Present redshift surveys may not extend sufficiently in angle or redshift to constrain the required transformation. I suggested only that cosmic distances might be established in this way "eventually."

Roger N. Shepard
Department of Psychology
Stanford University
9/87 *Stanford, California*

ETHICS FOR FUTURE PHYSICISTS

The APS directed-energy weapons report in the May 1987 issue (page S1) is certain to bring to mind in many readers the ethical problems that scientists who do SDI and other weapons research must face. They will remember these problems even more sharply if they flip ahead a few pages in that issue to read the budget report (page 59) and note how much of the 1988 science budget is allocated for military R&D. While SDI poses special and immediate concerns for the scientific community, the underlying issue behind the SDI debate is that of science's role in weapons research in general. The scientific community seems to have divided into three camps: those who oppose weapons research and won't do it, those who favor it and those who are not in favor of it but who do it anyway because a lot of good research can be done with the money. Soon there will be another group, however, composed of scientists who are undergraduates now and who just don't care as long as there is prestige and money involved in what they are doing.

This is extremely evident here at the University of Dayton. The university's department of physics and the University of Dayton Research Institute's applied physics labs are closely tied in with the R&D labs at Wright–Patterson Air Force Base. With a few exceptions, all junior or senior physics majors at Dayton work on projects for the Air Force through UDRI and Wright–Patterson. Engineering and chemistry majors are also accepted for defense-related work, either through the research institute or the base or through one of the many businesses in the Dayton area whose sole reason for existence is Wright–Patterson.

Whether it is unethical for these young scientists to work for the Air Force is a matter of opinion. But what is not so much a matter of opinion is the attitude with which students approach this work. The "get ahead" ideal that has infested many other undergraduates has also afflicted physics majors. No one at Dayton so much as considers whether DOD research is the sort of work a scientist should pursue. Some are doing the work for the money. Many are doing it for the prestige involved and the benefits such work gives to their resumés. Certain jobs under certain researchers at UDRI and Wright–Patterson seem to carry a sort of honor with them, supposedly because these researchers need brighter assistants for their work. The bright student who gets one of these "special" jobs does not give any consideration to the fact that the research he is so proudly working on will ultimately lead to the destruction of other people.

Because this sort of attitude seems so prevalent, and because the scientific community is still debating the ethics of DOD research, professors of physics should attempt to instill in students an ethical concern for their work. They do not need to turn students away from DOD research, for at this point in history a national defense is needed and therefore defense research needs to be done. But students should be encouraged to consider the implications, beyond personal gain, of the jobs they are taking.

Christopher Graney
5/87 *Dayton, Ohio*

QUASAR MISINTERPRETATION

We wish to correct any misunderstanding that may have resulted from John Kierein's letter (March 1987, page 112). Kierein's letter implies that we had detected relative proper motion between the quasars 3C 345 and NRAO 512, and that we had estimated the parallax distance of one of them. In fact, we detected *no* relative proper motion and obtained *no* parallax distance. In the first instance, for the "cores" of the quasars, we set only a firm upper bound of 20 microarcseconds per year in right ascension and 50 microarcseconds per year in declination. In the second, we set a lower bound of 15 kiloparsecs, valid only on the assumption that the two quasars are at sufficiently different distances from Earth. Thus, in a strictly logical sense, our results prove nothing about whether or not quasars are "local."

Norbert Bartel
Thomas Herring
Michael Ratner
Irwin Shapiro
Harvard–Smithsonian Center for Astrophysics
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5/87 *Westford, Massachusetts*

ARE THE UCS'S SDI SURVEYS BIASED?

Since the PHYSICS TODAY letters to the editor column seems to have developed into a forum for opinions regard-