reported by the Russians for evacuees from the 3–7-kilometer zone around Chernobyl.) By comparison, the maximum allowable radiation level at the boundary of a nuclear dump is 25 millrems/year, and the allowable concentration of radon is 0.1 pCi/liter. Therefore I, for one, would not be distressed if some of my grandchildren happen to get jobs at the SSC site, even if it were built around a high-level nuclear dump!

HENRY HURWITZ JR
2/87 *Schenectady, New York*

THE CHAIRMAN OF THE APS STUDY GROUP ON LIGHT-WATER REACTOR SAFETY REPLIES: I don't honestly know what's bothering Henry Hurwitz, but I do feel that as chairman of the 1975 APS study group, I owe something to the fine group of people we were able to assemble for that pioneering effort, and therefore I need to reply.

It seems to me that Hurwitz raises an important point, the public impact of a scientific study, and ends up on the wrong side of it. Nearly all physicists recognize that much of the public has an inordinate terror of radiation, carefully nurtured by squadrons of lawyers and show business personalities parading as consumer advocates and troubled scientists. Some of us therefore spend a great deal of time speaking, testifying and otherwise trying to contribute to the education of the body politic, and indeed of the press. It is no secret that we have had limited success—Gresham's law is as applicable to ideas as it is to money—and radiophobia persists, with all the classic symptoms of a phobia.

Depressing though that may be, one right it does not give us is that of adjusting our scientific work to meet our public objectives. Hurwitz accuses us of "concocting" our results, which is a pretty strong (and completely improper) assertion. In fact, our references for what we did were clearly stated, and were the standard authorities, the National Academy of Sciences and the National Council on Radiation Protection. Everyone knew that the linear model favored at that time by both these authorities was a prudent upper bound on the effects of radiation, and we said so several times in the report. As it turns out, our estimated doses for the hypothetical case of complete containment failure are not all that far from the Soviet estimates of the dosage at Chernobyl, where it actually happened.

We've learned a lot in the 12 years since 1975, and the 1983 update of the 1972 Academy study that we used departs, for the first time, from the linearity hypothesis. The study of radiation goes on, and in 12 more years we will know even more. At each moment in time, responsible scientists should do their work with the best information available, and let the results appear as they must. What they should not do is concoct their results to fit a social objective. Yet Hurwitz accuses us of "sin" for not having done just that. *Honi soit qui mal y pense.*

HAROLD W. LEWIS
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6/87 *Santa Barbara*

'Fundamental' distinctions

I was rather amazed to learn from Sheldon Glashow's letter (December 1986, page 13) that we are rapidly approaching the time when fundamental physics will no longer be possible (unless funding is received for the Geotron or Lunatron). Once the SSC is built its corridors will echo the last pronouncements by humanity on the nature of nature. We truly live in awe-inspiring times.

What a baffling word: fundamental. It seems to have many false definitions. It certainly can't mean "essential to the existence of." If that were the case then solid-state physics and materials science would be fundamental to the creation of the SSC, and thus to its fruition, as the magnets owe their existence to the Bardeen–Cooper–Schrieffer theory and high-critical-temperature alloys.

One might argue that fundamental means "basic or primary." It might then be supposed that a fifth force of nature (or even the possibility of such a force) would be of fundamental interest. [See PHYSICS TODAY, October 1986, page 17.] But the instruments used in searching for supportive evidence of that force are reminiscent of a turn-of-the-century baron's basement or a 17th-century heretic's tower. The price tag of such investigations is certainly not as fundamental, in the sense that our trade deficit is fundamental, as that of the SSC.

Does fundamental mean "of most basic importance," as Glashow suggests? Then are not the axiomatic precepts of quantum mechanics fundamental? Then Alain Aspect's empirical proof of Bell's theorem and the work by a couple of groups resulting in the direct observation of quantized decay with Penning traps would lead to fundamental understanding. (And, as asked above, does this make ultrahigh-vacuum and laser technology fundamental?) Obviously such studies must not be fundamental by reason of scale, or else they are too fundamental (they lead to better philosophy as well as to better physics).

Mathematicians are much more precise about definitions (just ask one) than physicists and many of them see nothing at all fundamental about physics. Yet they too have trouble with a working definition of "fundamental" within their own discipline. Is a proof in the *Principia Mathematica* more fundamental than the solution to the four-color problem? (It is certainly more in line with reductionism.) In fact, a mathematician would probably feel that "fundamental" sounds too plebeian and use the word "elegant" in its stead.

Perhaps this semantic debate will save the physics community some money in the long run. As Henry Barschall pointed out (December 1986, page 34) humanities journals are must less costly to produce than physical science journals. *Reviews of Modern Physics* could combine with *Reviews of Metaphysics*, *Physical Review Letters* could shorten its title to *Review Letters*, and *Vacuum* could keep its name but drop its price.

Political science journals would not be excluded. Thus Glashow would have a more appropriate forum.

ROBERT D. BLACK
General Electric Co
1/87 *Schenectady, New York*

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Sheldon Glashow has a quick way with words, but he might take a little more time to contemplate the world around him. What my friend Pedro Echenique was trying to explain to him is the existence of emergent properties: properties that are reducible to simpler entities, but not deducible from them.

For instance, it is well known that one may make a computer from vacuum tubes, from semiconductor devices or from neurons. The fundamental physics of these objects is very different, yet the computer works in each case. Rigidity, life, consciousness are all emergent properties, not deducible from the individual atoms or quarks of which their components are made and in fact achievable in different ways. Chaos is exhibited by thousands of totally different physical and biological systems. Computer theory reveals other extensive hierarchies of complexity. The SSC will add to our knowledge of any of these fundamental phenomena no more and no less than they have contributed to particle theory, namely a broadening of our regrettably narrow intellectual horizons.

PHILIP W. ANDERSON
Princeton University
12/86 *Princeton, New Jersey*

GLASHOW REPLIES: Enough of semantics! In essence, Philip Anderson and I are in complete agreement. Big sciences like the mapping of the human genome or the SSC project, however important in themselves, must not squeeze out equally important smaller sciences, whether "fundamental," "emergent" or whatnot. The destruction of once triumphant American space and planetary exploration by the manned shuttle program shows that this can happen. I am a staunch advocate of the SSC because it will answer basic questions that cannot otherwise be addressed, and its successes will inspire our youth to choose scientific careers. I am even more an advocate of increased and stable funding for the entire American scientific establishment, without which this nation cannot long endure.

SHELDON L. GLASHOW
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QCD and gluon observation

Every new accelerator always adds some new information from "bread and butter"-type data. However, as the cost of construction of accelerators escalates, patrons who finance such projects tend to require much more sensational discoveries. Such pressure for sensational discovery becomes more troublesome when coupled with a (little noticed?) technical aspect of big-accelerator-based physics: As the power of big accelerators grows, the detectors used also need to become bigger and more sophisticated. Not only is it impossible to construct such big and sophisticated detectors to be nearly 100% efficient, it is also (almost?) impossible to correct the data acquired from such detectors to the degree that they can be considered to come from an ideal detector. But predictions of models and theories are always geared for ideal detectors. The way this dilemma is solved is that research participants put vast arrays of data on the inefficiencies and defects of the detector into computer programs, use Monte Carlo simulation methods to simulate events according to the models and theories being considered (still for an ideal detector), push those computer-simulated events through the above-mentioned computer programs to make the predictions of the models and theories as deficient as the real detector, and then compare the results with the data.

In other words, the published experimental data and their comparisons with models and theories are for the specific inefficient detector, not for an ideal detector in general. If an outsider wants to compare the data with his own models or theories, he simply cannot because his results are good only for an ideal detector. This aspect of big-accelerator-based physics increases the monopolistic power of the participants in big-accelerator experiments in interpreting the data and reduces the ability of outsiders to question critically their findings. Given this reality and the pressure to make sensational discoveries, there may exist both the motivation and the ability for some participants to exaggerate their findings.

An explicit experience of mine illustrates that the above-mentioned aspect of big-accelerator-based physics is not just a fear on paper. Many readers probably remember the sensational claim of the discovery of "gluons" and of the confirmation of quantum chromodynamics many years ago. To understand what the claim really was, a word about the true status of quantum chromodynamics is in order: Quantum chromodynamics is a theory of nonobservable and strongly interacting quarks and gluons; the theory does not have predictive power since it cannot be solved exactly or approximately. What the groups who claimed the discovery of "gluons" did was to implement some suggestions of quantum chromodynamics into a scheme with many additional adjustable assumptions and parameters and to fit the data of electron–positron annihilation. Data fitting does play a role in advancing our understanding, especially when a theory lacks predictive power, but we cannot claim the objective confirmation of a theory nor the discovery of nonobservable particles such as "gluons" from data fitting, precisely because of the existence of those additional adjustable assumptions and parameters not derivable from the theory. At that time I suspected that the data from electron–positron annihilation actually were very similar to the ordinary soft hadron scattering data and did not warrant the claim of the discovery of "gluons." My paper expressing this view received two kinds of response from the referees and the associate editor of *Physical Review D.* The first kind was that quantum chromodynamics was so firmly confirmed and the discovery of gluons so firmly established that unless I could provide firm evidence to show the invalidity of quantum chromodynamics (no one can disprove a theory that does not have predictive power!), the paper should not be published. The second kind of response was that because my discussion was for an ideal detector and could not be compared with the published data, which were not corrected and were good only for the nonideal detector, the