fast in conveyances that are not completely closed conductors—for example, riding on motorcycles, in open convertibles or soft-top cars, in airplanes with fiberglass bodies and in metal-covered jet planes with appreciable window area (as in the cockpit of a jet fighter plane). Similarly, astronauts go on space walks outside their spaceships, and so on. The point was (and it was a minor one) that there are plenty of common activities in which one is exposed to electric fields of this type that are much larger than those coupled into the body at ground level below typical power lines.

References

1. W. R. Bennett Jr, *Health and Low-Frequency Electromagnetic Fields*, Yale U. P., New Haven, Conn. (1994).
2. R. K. Adair, Phys. Rev. A **43**, 1039 (1991).
3. See, for example, C. F. Stevens, Biophys. J. **12**, 1028 (1972); A. A. Verveen, L. J. DeFelice, Prog. Biophys. Mol. Biol. **28**, 189 (1974).
4. J. L. Kirschvink, Phys. Rev. A **46**, 2178 (1992); see discussion of figure 1. Also see the reply by R. K. Adair, Phys. Rev. A **46**, 2185 (1992), and R. K. Adair, Bioelectromagnetics **14**, 1 (1993).
5. A. V. Prasad, M. W. Miller, E. L. Carstensen, Ch. Cox, M. Azadniv, A. A. Brayman, Radiat. Environ. Biophys. **30**, 305 (1991). L. A. Couton, A. T. Barker, Phys. Med. Biol. **38**, 347 (1993). A. V. Prasad, M. W. Miller, Ch. Cox, E. L. Carstensen, H. Hoops, A. A. Brayman, Health Phys. **66**, 305 (1994).
6. P. Brodeur, *Currents of Death: Power Lines, Computer Terminals and the Attempt to Cover Up Their Threat to Public Health*, Simon and Shuster, New York (1989).
7. E. R. Adair, "*Currents of Death* Rectified: A Paper Commissioned by the IEEE-USA Committee on Man and Radiation in Response to the Book by Paul Brodeur," IEEE-USA, New York (1991). J. R. Jauchem, J. Clin. Epidemiol. **45**, 1137 (1992).
8. P. Brodeur, The New Yorker, 7 December 1992, p. 86.

WILLIAM R. BENNETT JR
Yale University
New Haven, Connecticut

Retooling the Tevatron for Top Performance

I am puzzled why the 1994 subpanel of DOE's High Energy Physics Advisory Panel led by Sidney Drell has proposed a US contribution to the CERN Large Hadron Collider (see PHYSICS TODAY, July 1994, page 51), whereas a similar Drell subpanel in 1990 pointed out that the LHC energy is about a factor of 3 too low "to elucidate [with confidence] the nature of electroweak symmetry breaking."[1] The new Drell subpanel not only endorses the LHC but recommends a US contribution to it of $400 million over eight years!

If $320 million of that $400 million were given to Fermilab instead, the Tevatron energy could be doubled and its luminosity increased to 10^{33} cm^{-2} sec^{-1} or more.[2] With such an upgraded Tevatron at 4 TeV in the center of mass, over 2000 top-quark events could be produced per day. We would have a T-factory, equivalent to the present Cornell B-factory, which would open up a new field of physics. Also, there would be a chance of finding clues to electroweak symmetry breaking: Heavy Higgs particles of mass up to approximately 300 GeV decaying into two vector bosons could be seen. The D0 detector, which now has a hole through it for the main ring, could be made state of the art, and a third detector could be designed for C0. In addition, fixed-target experiments could be done with a primary beam of twice the present energy. Ever higher-energy upgrades are under consideration.[3] All this would be more exciting and more cost effective than trying to fit in with the 1500 European physicists already planning to use the LHC. It could be completed five years before the LHC, at one-tenth the cost, and it would reverse the present decline of American high-energy physics.

A very important fringe benefit of such a Tevatron upgrade is that it would be an ideal injector for a future 20-TeV ring that could do the physics that the Superconducting Super Collider was to do, but at a fraction of the SSC's cost. Fermilab would then have antiproton beams almost as intense as proton beams, and there would be no need for two rings of magnets, as was necessary for the SSC. The number of magnets would be one-third that of the SSC. I have seen estimates of around $1 billion for the magnets and $200 million for the tunnel. Besides, in the Illinois site proposal for the SSC, the State of Illinois pledged to cover tunnel costs. There would be no need to go through the additional costs and new layers of management connected with creating a new, large laboratory from scratch.

Perhaps the Drell subpanel operated under the rigid assumption that the next accelerator above the LHC energy *must* be an international enterprise. From a world point of view it would be wasteful of resources and money to build a new high-energy physics laboratory from scratch at some unknown location. (We should have learned this lesson from the SSC.) The Tevatron is an existing national accelerator and laboratory. As is the usual practice, other countries would contribute to the new, large detectors and the experimental program in proportion to their participation.

Not only is the Drell report being misused to promote the LHC over the physics that we Americans would normally be doing at that time, but it is being used to promote linear colliders over hadron colliders. For example, *Science*, in a report on the Drell subpanel's recommendations,[4] said that "nearly all physicists agree that the next step after the LHC should be a long, straight linear collider, a larger version of the one now operating at SLAC." One can guess whom the *Science* reporter talked to. I bet the reporter was not told that a proton collider uses known technology at known cost, whereas no one knows how to build an electron–positron collider of reasonable cost and of high enough energy to produce the Higgs particles that could be produced by the SSC or its equivalent.

Almost all the American physicists I know would prefer the first of the following two choices:

▷ the Fermilab program I have just described

▷ giving the equivalent funds to the LHC, followed by an international linear collider project of too low an energy, as described in the box on page 1397 of the *Science* report.

References

1. S. Drell *et al.*, report of 1990 HEPAP Subpanel on SSC Physics.
2. G. Jackson *et al.*, "Conceptual Pre-design for a Tevatron Upgrade to 2 TeV Beams and Luminosity >10^{33}," Fermi Natl. Accelerator Lab., Batavia, Ill., 25 March 1994, unpublished, available from Jackson.
3. J. Strait *et al.*, "Tevatron Energy and Luminosity Upgrades Beyond the Main Injector," to be published in Proc. 8th Mtg. of APS Division of Particles and Fields, 2–6 August 1994.
4. Science **264**, 1397 (1994).

JAY OREAR
Cornell University
Ithaca, New York

Seeking ν Oscillations with Old Reactors

Bertram Schwarzschild's news story on anomalous cosmic-ray data and neutrino oscillations (October, page 22) nicely reviews the status of the atmospheric neutrino puzzle and mentions several high-energy experiments that have been proposed to shed light on neutrino oscillations in the critical parameter range

$\Delta m^2 \approx 10^{-2}$–$10^{-3}$ eV^2.

It is useful to recall that low-energy neutrinos from nuclear reactors may compete with high-energy neutrinos from accelerators as sensitive and cost-effective probes for oscillations in this parameter range. Inasmuch as reactor neutrinos have energies of only about 5 MeV, about 1000 times smaller than those at Fermilab or CERN, the base line required to achieve comparable sensitivity is only about 1 km, about 1000 times smaller than for high-energy experiments. Accordingly, detector size and price tag for a reactor experiment are much more modest. So is the lead time for an experiment.

Reactors are pure electron-antineutrino ($\bar{\nu}_e$) sources. Reactor experiments would probe the "disappearance" of the $\bar{\nu}_e$, thus shedding light on the oscillations $\nu_e \longleftrightarrow \nu_\mu$, one of two possible modes that might explain the atmospheric puzzle.

There are two such experiments in preparation, each using a detector of about 10 tons. One is near the San Onofre nuclear power station in California, and the other is near a station at Chooz in France. These experiments will be capable of deciding conclusively whether there are $\nu_e \longleftrightarrow \nu_\mu$ oscillations. A positive result would explain the atmospheric puzzle and, more generally, establish that neutrinos have mass.

FELIX BOEHM
PETR VOGEL
California Institute of Technology
Pasadena, California

Sound Reasoning on Materials and Moduli

The following statement in Ray Ladbury's news story (October, page 17) on the 9 June Bolivian earthquake is incorrect: "Because olivine is less dense than spinel of a similar temperature, the speed of sound would drop as it passed through olivine." The speed of sound in olivine is lower than in spinel, but not because olivine is less dense. The relevant relationships are

$$V_p = \{(K + 4/3\mu)/\rho\}^{1/2}$$

$$V_s = (\mu/\rho)^{1/2}$$

where V_p and V_s are compressional and shear wave velocities, respectively, K is bulk modulus, μ is shear modulus, and ρ is density.

Note that ρ is in the denominator, so a decrease in ρ alone would increase velocity. I recognize that this is counterintuitive. Reconciliation with intuition follows from the fact that in most situations where we compare the velocities of sound in materials, the difference in the moduli is even greater than the difference in density. Materials of greater density usually have much greater moduli. I emphasize this point to my students and feel it worth emphasizing here.

GARY L. KINSLAND
University of Southwestern Louisiana
Lafayette, Louisiana

National Ignition Facility Funding Foul-up, Fixed

In the recent summary of the 1995 Congressional R&D budget actions (October, page 59) Irwin Goodwin mistakenly refers to the proposed National Ignition Facility as "a massive \$10 billion" project. In fact, the correct total project cost estimate for the NIF is \$1.1 billion in as-spent dollars, including contingency. That figure is based on a detailed conceptual design study[1] submitted to the Department of Energy by a multilaboratory team consisting of scientific and engineering staff from the inertial confinement fusion programs at Lawrence Livermore National Laboratory, Los Alamos National Laboratory, Sandia National Laboratory—Albuquerque and the University of Rochester. The project cost has been validated by independent cost estimators commissioned by the DOE.[2] Indeed, if funded by Congress, this seven-year project (FY 1996–2002) would be a significant investment by the US in inertial fusion energy technology and high-density physics.

References

1. Natl. Ignition Facility Conceptual Design Rep., NIF-LLNL-94-113, L-16973-1, Lawrence Livermore Natl. Lab., Livermore, Calif. May 1994.
2. Independent Cost Estimate—Natl. Ignition Facility, contract no. DE-AC01-94PR10016, Foster Wheeler USA Corp., Englewood, Colo., May 1994.

JEFFREY A. PAISNER
Lawrence Livermore National Laboratory
Livermore, California

(The writer is project manager of the National Ignition Facility.)

Resolving Near-Field Microscopy History

The news story "Near-Field Optical Microscopes Take a Close Look at Individual Molecules," by Graham P. Collins (May 1994, page 17), was of particular relevance to us, since our group at the IBM Zurich Research Laboratory was the first to build an NFO microscope. We feel that the report presents an incomplete and in some aspects erroneous view of the development of NFO microscopy. Our claim is based on published literature from an entire decade (the 1980s) that was not cited in Collins's report.

In particular:

▷ With the NFO microscope that we (in particular Dieter W. Pohl, W. Denk and Urs Dürig) built in 1983 and operated from then on, we obtained and published images showing details 20 nm in size,[1,2] somewhat better (and earlier) than the "unprecedented optical resolutions" of 50 nm cited by Collins. The Cornell group reported a resolution of the same order a few years later using a similar setup.[3]

The instrument that we developed at that time already possessed all the essential features found in present NFO microscopes. (Compare figure 1b of reference 1 with figure 1b of reference 4.)

▷ The "first scanners of this type" were etched quartz crystals whose facets formed highly pointed tips. They had an optimal angle of apex (close to 45°), were aluminum coated and could be prepared to form an extremely small aperture at the very apex. They were used as optical probes in our NFO microscope.[1,2] The micropipette technique, which Collins also describes as being used in the "first scanners," was introduced in 1986 by the Cornell group.[3]

▷ We are not aware of any comparison between our quartz probes and the optical fiber probe cited in Collins's report. The claim to have found an implementation with throughput "four orders of magnitude greater than those in previous designs" hence awaits to be substantiated.

▷ "Apertureless NFO microscopy" also was already conceived and demonstrated at our laboratory back in the 1980s, with U. C. Fischer as the main investigator.[5]

A fair and complete historical perspective on NFO microscopy should certainly include the 1928 proposal of E. H. Synge[6] and the 1972 microwave work of Eric A. Ash and coworkers,[7] as Collins's report appropriately did. It nevertheless remains the case that the way to present-day NFO microscopy was paved by the experimental work of the 1980s, in particular by our conception and successful demonstration of a complete NFO microscope.

References

1. D. W. Pohl, W. Denk, M. Lanz, Appl.