DOES PHYSICS REALLY RULE OUT POWER-LINE CANCERS?

In his review article (April 1994, page 23) summarizing the significance of extremely low-frequency electromagnetic fields for human health, William R. Bennett Jr shows that the random thermal electric field in a tissue cell, as estimated by Robert Adair,[1] is "about 1000 times the internal electric field estimated to be caused by a power line" (about 20 μV/m), and he concludes that power-line fields are thus only a tiny addition to the thermal 60-cycle field naturally present in the cell. Adair's estimate is based on the well-known relationship for the thermal noise generated in a circuit by a resistive element, as applied to a cell.

The linear circuit-element model is not suitable for the determination of thermal fields in a cell, for several reasons. Cells are linked to neighboring cells of a tissue in a three-dimensional structure that does not resemble an electrical circuit, which is a one-dimensional, multiply-connected structure. For the transmission of low-frequency fields, tissue is more nearly a homogeneous, isotropic medium. Furthermore a cell is too small an element to maintain equilibrium thermodynamic electromagnetic fields at the cell temperature. Equally important, the thermal electric and magnetic fields in a system at thermodynamic equilibrium are thermodynamic properties that cannot depend on the medium's transport properties, such as its electrical resistivity, which is a component of Adair's model.

It is easy to prove that the thermally generated electric fields in human tissue are indeed much smaller than those caused by typical power-line sources. Suppose we consider human body tissue as a homogeneous, isotropic, electrically conducting medium of conductivity σ that is in radiative thermodynamic equilibrium with an external environment at a temperature T. In the environment, the electric and magnetic field intensities are essentially those of a vacuum blackbody radiation field and have spectral energy densities given by[2]

$$\left(\varepsilon_0 \frac{\langle E^2 \rangle}{2}\right)_\nu = \left(\frac{\langle B^2 \rangle}{2\mu_0}\right)_\nu = \frac{4\pi\,\nu^2 kT}{c^3}$$

in the limit of low frequencies $\nu \ll kT/h$, which is the case for 60-cycle power-line radiation. For a temperature T of 300 K, a frequency ν of 60 Hz and a bandwidth $\Delta\nu$ of 1 Hz, the calculated environmental rms electric and magnetic fields are 1.25×10^{-15} V/m and 4.18×10^{-20} gauss. If we consider the exchange of energy across the plane surface separating the environment from a tissue sample and use the conditions for refraction of electromagnetic waves,[3] we find that the electric and magnetic field intensities are the same inside the tissue as they are in the environment, as given above, provided the electric permittivity ε and magnetic permeability μ are $\varepsilon_0 + i\sigma$ and μ_0, respectively. This relationship holds even though the transmission coefficient of the interface is very small,[3] on the order of $\sqrt{\varepsilon_0 \nu/\sigma}$. Thus the electric field in tissue is 12 orders of magnitude smaller than Adair's estimate and certainly negligible compared with the electric field induced by power lines, even if a wider bandwidth is assumed.

A tissue sample warmer than the environment will not maintain a level of electric and magnetic fields corresponding to its temperature T unless the sample is optically dense. For this to be so, the sample dimension L must be a fraction of the skin depth (which is approximately $1/\sqrt{\mu_0 \sigma \nu}$) that is larger than the transmission coefficient:

$$L > \frac{1}{\sigma}\left(\frac{\varepsilon_0}{\mu_0}\right)^{1/2}.$$

For tissue, which has a conductivity σ of about 1 siemen per meter, L must be larger than about 1 cm. Thus animals would experience equilibrium fields at body temperature. Even if it were proper to consider a single cell as a circuit element, it is too small in length by a factor of a thousand to generate internally an equilibrium electric field, so Adair's calculation is not pertinent. (On the other hand, for a metal conductor, L is about 1×10^{-8} m, so ordinary circuit elements maintain thermal equilibrium electric fields within the circuit.)

If the field in a homogeneous medium is as small as calculated above, why are larger fields measured in resistive circuit elements? The reason is that low-frequency waves can propagate around the circuit with little loss, because the surface of the conducting elements is highly reflecting. The medium is one-dimensional rather than three-dimensional, and the energy spectrum does not go to zero at $\nu = 0$, as the three-dimensional blackbody spectrum does, but remains constant and independent of frequency, giving rise to higher fields at low frequencies than for the three-dimensional case.

It is not surprising that the thermally generated fields within tissue are essentially the same as those in the surrounding environment at the same temperature, namely those of a blackbody radiation field. The microscopic processes that give rise to these fields are reversible, and the field levels are independent of macroscopic irreversibilities such as electrical resistivity. Fields generated by power lines or electronic equipment are well in excess of the thermodynamic equilibrium values at ambient temperatures and are bound to exceed the thermal levels in tissue.

The thermodynamically generated fields are random in phase and direction, in contrast with the external field induced by power lines. In commenting on self-organization in living cells, Benno Hess and Alexander Mikhailov[4] point out that energy from external sources that is far from thermodynamic equilibrium, as is the field of power lines, can organize thermal fluctuations within cells. Whether this effect exists for power-line fields remains to be seen.

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JAMES A. FAY
Massachusetts Institute of Technology
Cambridge, Massachusetts

In his very interesting and informative article "Cancer and Power Lines," William Bennett discusses the electric field engendered in biological tissue by the 60-Hz external electric field caused by power lines. He invokes the ELF approximation to find the ratio of the amplitudes of these internal and external electric fields. Following Charles Polk, he states that a simple application of Maxwell's equations and the appropriate boundary conditions indicates that the internal electric field E_{int} is normal to the surface and many orders of magnitude less than the external field E_{air}:

$$|E_{\text{int}}/E_{\text{air}}| \sim \omega\varepsilon/\sigma$$

where ω is the angular frequency and ε and σ are the permittivity and conductivity of tissue, respectively. Bennett assumes ε to be the permittivity of free space ε_0 and σ to be 0.5 siemens per meter and finds the amplitude ratio to be approximately 7×10^{-8}. Subsequently in the article he uses this value to argue that electric fields induced in biological materials are negligible.

All this is somewhat disquieting, for there is no need to invoke the ELF approximation. The reflection and refraction of plane electromagnetic waves incident from a dielectric onto a plane conductor are well known and covered in many electromagnetic textbooks. For the case in which the electric field is in the plane of incidence some pertinent results can be summarized as follows:

▷ The transmitted wave propagates with a very small angle of refraction (less than 4×10^{-3} degrees for the values chosen by Bennett).

▷ The ratio of the amplitudes of the transmitted and incident electric fields is given by

$$|E_{\text{int}}/E_{\text{air}}| \sim 2(\omega\varepsilon/\sigma)^{1/2}$$

which reduces to approximately 1.6×10^{-4} for the values chosen. This ratio is about four orders of magnitude greater than that stated by Bennett and should replace 7×10^{-8} when one is discussing the magnitude of the internal field.

Since the transmitted wave propagates nearly normally to the interface, the normal component of the internal electric field is indeed very much smaller than the total internal field,

$$|E_{\text{int,normal}}/E_{\text{air}}| \sim 2\omega\varepsilon/\sigma$$

in approximate agreement with Bennett.

Surely, however, it's the total internal electric field induced in the biological material that should concern us, not just its insignificant normal component.

Of course I have assumed a particularly simple geometry for these calculations, and in practice the wavelength of the radiation is much greater than the dimensions of the biological material, so no doubt the insistence on more appropriate boundary conditions would vary these results. But would the application of more appropriate boundary conditions necessarily validate the calculations presented by Bennett?

DAVID N. PINDER
Massey University
Palmerston North, New Zealand

In his excellent review William Bennett makes a clear case against the leading candidates for a mechanism underlying a postulated connection between cancer and 60-Hz electromagnetic fields. Although his estimates for the strengths of ac magnetic fields inside cells are lower than those of some other workers,[1] the main point is that the energy imparted to any identifiable intracellular dipole is six to seven orders of magnitude less than thermal energies. This appears to be a compelling argument, yet a more tempered verdict may be in order, especially as some empirical evidence, while not definitive, does suggest such a link.[2]

So far no one seems to have considered a collective mechanism. There are over 2×10^{10} nitrogen atoms in human DNA, providing a direct coupling of magnetic fields to the genetic code. Oscillatory magnetic fields might induce, through the Einstein–de Haas effect,[3] twisting and writhing of DNA strands during cell division. My estimate is that, allowing for thermal factors that greatly diminish the response, only 3×10^7 base pairs, rather than the full 3×10^9 of the human genome, are required to obtain energy changes exceeding the strengths of topological bonds. Biological activity is extremely sensitive to geometric factors, so an actual bond need not be broken; it would be sufficient to disturb the conformation of a molecule at a sensitive stage in cell reproduction to induce an effect.

The coupling of an ac magnetic field to a paramagnetic medium in the presence of a dc magnetic field (such the Earth's field) can result in substantial changes in the induced magnetization, even when the strength of the driving field is small.[4] The magnitude and *orientation* of the dc field are more important than the amplitude of the driving field, precisely as has been observed in biological experiments.[5] Indeed, if the applied field is too strong, saturation will occur and the effect will go away. Reasonable estimates for the spin relaxation times of organic molecules in an aqueous environment[6] are commensurate with a strong induced magnetization at low applied-field strengths and frequencies.

From a mechanical standpoint, the equations of motion governing the response of a long chain such as a relatively free DNA strand are identical to those for a spinning top.[7] They are of third order, leading to a textbook instability.[8] These considerations suggest that one must give the Einstein–de Haas effect careful consideration before dismissing the biological effects, especially on the young, of frequent exposure to ac magnetic fields.

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ROGER BECKER
University of Dayton
Dayton, Ohio

One aspect of the possible initiation of cancer by power lines was omitted from William Bennett's otherwise excellent review. I refer to the generation of atmospheric ions and complex organic molecules by the corona discharges common around insulators and joints in high-tension lines, especially in wet and humid conditions. I would expect that very strange chemicals could be generated by the corona, especially in regions where atmospheric pollution is high. If these chemicals were directly introduced into the bloodstream via the lungs, even in minute quantities, they could cause cancer over a period of time. Can Bennett (or anyone else) offer any words of wisdom about this aspect of this important topic?

IVOR BRODIE
SRI International
Menlo Park, California

Accepting the article by William R. Bennett Jr as guidance on the question of health effects of electromagnetic fields seems to me analogous to accepting the advice of the village blacksmith on how to fix your Swiss watch. There is no doubt that Bennett's calculations are impressive. They are probably sound and correct as well. However, the question remains, Are they relevant to the question being addressed?

Bennett treats this question as though it were just another physics problem dealing with electromagnetic fields and the ordinary properties of matter. Presumably, then, it is much simpler than high-temperature superconductivity, since we don't really know yet how to calculate the observed properties of these superconductors. If biology is just applied physics of a straightforward nature, why isn't medical practice transparent, why aren't chronic diseases (such as chronic fatigue syndrome or Alzheimer's disease) well defined and treatable, and why isn't the mystery of consciousness resolved? Is it possible that biological systems are a little more subtle than is credited to them in Bennett's article? What, for example, does the fact that a lightning bolt may have a peak current of 10–20 kiloamps tell us about why electrical workers are known to have an increased cancer risk over "normals"?

There is some confusion in the public mind over the distinction between oscillating electromagnetic fields in the so-called diffraction zone and the fields representing radiated energy. Is this issue of vital importance in this controversy? After all, many professionals also have some difficulty sorting out that part of the total field responsible for the radiated energy. Moreover, of what relevance is the fact that the free-space wavelength of a 60-Hz wave is 3000 km? This field, nonetheless, does reverse its direction 120 times per second. Perhaps this fact and the day in–day out persistence of power-line fields are more significant factors in assessing possible health effects on the human body than is the wavelength of 60-Hz radiation.

Who today, physicist or otherwise, can objectively define states of ill health, let alone measure degrees of ill health objectively? Why then does Bennett treat the possible implications of epidemiological studies so dismissively? So they are less controlled and less objective than the laboratory-based studies so familiar to the physicist. Does this mean they have no value and even where public health may be involved we should ignore them? Ethical considerations prevent us from deliberately subjecting humans to field tests in the laboratory. However, many animal experiments indicate that there are deleterious biological effects from ELF magnetic fields. Animals can't talk and in any case objective testing is very difficult. But this does not mean that a problem does not exist.

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Bennett's calculations are no doubt useful in trying to analyze the problem, but they cannot be used to sweep the whole problem under the rug. Other superficialities are present in Bennett's article. For example, to imply that Paul Brodeur's articles in *The New Yorker* started this whole series of concerns is to be imprisoned by a narrow perspective. Admittedly the articles helped bring these concerns to popular attention, at least in the US, but they preceded Brodeur. And why were Brodeur's articles "sensational"? Because they exaggerated the truth or because they revealed possible truths that were unsuspected?

There are many ways to ill spend the public money. In my view, trying to get at the bottom of this particular health issue is not one of the major offenders. It is in the public interest to have the perspective and the expertise represented in Bennett's article as a contribution to dealing with this whole question. My concern is primarily that Bennett's oversimplification as well as his expertise be placed in perspective. The human organism is a very complicated nonlinear system, consisting in the conventional reductionist model of many highly nonlinear subsystems. Anyone familiar with the so-called butterfly effect will express strong reservations about deducing any simple conclusions about such a system from linear calculations and linear comparisons. Although I am a theoretical physicist with some Galilean reverence for the powers of deductive argument, I believe this is one circumstance where we are well advised to seek out the empirical evidence before drawing definitive conclusions.

LYNN E. H. TRAINOR
University of Toronto
Toronto, Ontario, Canada

William Bennett's interesting article did much to clarify and dispel many of the misconceptions concerning extremely low-frequency electromagnetic fields. Unfortunately one of Bennett's examples serves to reinforce a common misconception in ordinary electricity and magnetism. By way of introduction to his discussion of the $\mathbf{v} \times \mathbf{B}$-type electric field caused in, for example, blood flowing in an aorta by the presence of a magnetic field, Bennett mentions that because of the Earth's magnetic field "passengers in a jet flying across the country at 500 mph would experience a field of about 0.011 V/m." Presumably his point is that this $\mathbf{v} \times \mathbf{B}$ electric field is small as a result of the numerical values of v and B. In fact the field here is rigorously zero regardless of the values of v and B.

Consider just the conducting airplane itself moving through the magnetic field. It is well known that a charge redistribution will occur on the outer surface of the conductor so as to produce a uniform electric field $\mathbf{E} = -\mathbf{v} \times \mathbf{B}_{\text{Earth}}$ inside the conductor (including any cavity within the conductor). We can now insert the comoving passenger "for free"; all charges within the passenger experience zero net Lorentz force. According to the passenger the (nonrelativistic) fields will be $(\mathbf{E},\mathbf{B}) = (0,\mathbf{B}_{\text{Earth}})$. According to a person "in the lab frame," that is, stationary with respect to the Earth, the fields inside the plane will be $(\mathbf{E},\mathbf{B}) = (-\mathbf{v} \times \mathbf{B}_{\text{Earth}},\mathbf{B}_{\text{Earth}})$. The whole phenomenon is just the usual Faraday-cage shielding as seen by the passenger.

KENNETH R. BROWNSTEIN
University of Maine
Orono, Maine

William R. Bennett Jr's interesting examination of the possibly negligible relation between cancer and power lines attributes the charge on the Earth to "the combination of collisional ionization of air molecules by protons in the Van Allen radiation belt and the molecular photoionization" in the upper atmosphere. Without detracting from Bennett's analysis, it should be pointed out that his explanation for maintenance of the Earth's charge, variations of which were popular in the early years of this century, has long since been replaced by C. T. R. Wilson's suggestion[1] involving thunderclouds.

In 1887 F. Linss[2] noted that the conduction current carried by atmospheric ions would neutralize the bound charge on the Earth in a very short time. The relaxation time was later found to be on the order of 1000 seconds. After several explanations for the continued presence of charge on the Earth were demonstrated to be untenable, Wilson suggested in the 1920s that thunderclouds provide the principal supply currents. He pointed out that electrified clouds:

▷ extract negative ions from the more conductive upper atmosphere by attraction to the positively charged cloud tops

▷ lower negative charges to the Earth via lightning

▷ induce an upward flow of positive ions from the Earth by point discharge under the influence of the strong negative charges accumulated in the lower regions of thunderclouds.

In the years since, Wilson's hypothesis has been tested by many investigators, and there is now general agreement that it provides a satisfactory explanation as to *where* the current maintaining the Earth's charge originates. There is no similar agreement, however, about the mechanisms and processes by which thunderclouds generate this current; controversies continue among the proponents of various explanations[3] for thunderstorm electrification.

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CHARLES B. MOORE
New Mexico Institute of Mining and Technology
Socorro, New Mexico
BERNARD VONNEGUT
State University of New York, Albany

BENNETT REPLIES: Although several of the above letters are interesting and clarifying (such as the one by Charles B. Moore and Bernard Vonnegut), none of them alter the main point of my article, namely, that the magnitude of field exposure near ground level from typical power lines (and especially from urban distribution lines and transformer substations) is very small compared with the unavoidable exposure one gets from natural physical and biological sources. Hence exposure to the former fields alone cannot be regarded as a serious threat to health.

James A. Fay assumes that biological material at body temperature only reaches thermodynamic equilibrium through radiative processes. That simply is not even approximately true, and Fay's calculations are irrelevant. In this largely liquid environment, local thermal equilibrium is established primarily by collision processes. Even for $h\nu = kT$ at body temperature, the typical oscillator will have radiative lifetimes in excess of several seconds, whereas the lifetimes against collision destruction of the oscillator states will be on the order of a picosecond.[1] As can be seen from the principle of detailed balancing, these collision processes establish both Boltzmann and Maxwellian distributions in the local temperature. Thus the random thermal (Brownian) motion of charged particles in resistive material that produces Johnson noise will be well described by the Nyquist formula in terms of the local temperature (Robert Adair's assumption[2]). The linear circuit model is clearly an approximation, but it is a very reasonable one for treating isolated cells in the body electrolyte or even clusters of cells connected by gap junctions. The large membrane resistance isolates the inside of the cell electrically from the outside electrolyte. Because the fluids inside and outside the cell are highly conductive compared with the membrane, the membrane is encased by equipotential surfaces and is equivalent to a lumped resistor for the calculation of noise. Indeed, some authors have found noise measurement in a known bandwidth to be a useful method for determining cell membrane resistance.[3]

David N. Pinder is concerned that the transmitted electric fields from power lines at a biological interface in the Polk model do not agree well with those based on plane-wave refraction. He seems to feel that the high-frequency plane-wave approximation is more fundamental. Quite the opposite is true in the present case. The wavelength (5000 km) associated with power-line fields is infinite for all practical purposes, and the problem is nearly a static one. To an excellent approximation the electric and magnetic fields from the line decouple and are completely independent of each other. For example, the magnetic field is actually zero if no current is flowing in the line, whereas the electric field from the line remains nearly independent of load on the line. In the plane-wave approximation the magnetic field is assumed to be proportional to the electric field. There are no plane waves in the present case, and the problem is entirely a near-field, quasistatic one. Charles Polk's result follows directly from the basic continuity relations in this limit, and the approximation should be extremely good.[1] Here, the external electric field is closely normal to the surface of the body. Pinder's plane-wave model would have to correspond to an incident wave propagating parallel to the surface. The answer to Pinder's final question is simply "Yes." (A more appropriate calculation *does* give my result.) Nevertheless it is worth noting that my conclusions were not critically dependent on an attenuation factor as small as 10^{-8}. Even if the factor were 10 000 times larger, the induced fields would still be negligible compared with thermal noise at the cell level for most cases studied.

Roger Becker's comments on a possible collective mechanism for a diamagnetic interaction between the large number of nitrogen atoms in human DNA and external magnetic fields from power lines are highly speculative. Any motion induced by 60-Hz fields at the cell level will be strongly damped by viscosity effects. Few things are likely to have as large a collective magnetic interaction as a long chain of magnetite domains. For example, Joseph Kirschvink[4] estimated from his model of the problem that it would take more than 1400 milligauss from a 60-Hz field in the presence of cellular protoplasm to open an ion channel with a magnetite particle having a moment as large as 2×10^{-15} A m^2 (about 34 domains). Such fields are enormous compared with those from power lines. (Incidentally, I did not mean to imply in my article that biological interactions with static Earth-level fields are not well established in some cases; there was a misprint in my remarks on *Aquaspirillum magnetotacticum* bacteria in which the interaction energy with the Earth's field was printed as kT rather than 10 kT. However, there is no reason to believe interactions with such static fields are carcinogenic; evolution alone argues to the contrary.) As I noted briefly in my article, the early experimental biological results by Abraham Liboff and others that Becker cites as possible evidence for coupling effects of ELF ac magnetic fields in the presence of large Earth-level dc magnetic fields have not been consistent and have involved marginal signal-to-noise ratios. The required field relationships, "resonant" frequencies and "window effects" have varied from one paper to another by the original authors. More important, several recent attempts by independent investigators to reproduce these experimental results have failed.[5]

As Ivor Brodie suggests, there might conceivably be confounding effects resulting from carcinogenic ionization products produced by corona discharge from power lines. I did discuss that possibility in my book;[1] however, the only relevant data that I could find did not show any significant concentration under power lines of products such as ozone above normal background levels. Corona discharge would only be important from high-tension lines, and those are typically at least 30 meters in the air. In most instances the discharge products would be short-lived and blown away by the wind before reaching ground level. If there were an adequate concentration of pollution to produce worrisome "strange chemicals" as discharge products, it would probably be difficult to distinguish their effects from those of the pollutants themselves. It also should be noted that recent high-tension lines use groups of triangularly spaced wires to minimize corona discharge.

Lynn E. H. Trainor raises a barrage of "questions" that appear mostly to be statements of a personal point of view. I too think the human body is a remarkable and complex mechanism—certainly much more impressive than a Swiss watch and not likely to be explained completely in a simple, straightforward fashion by application of the basic laws of physics. Having agreed to that, one can either try to see what basic things may be said about the electromagnetic field–cancer problem in an objective manner or go on to some other topic. I did consider the epidemiological evidence in some detail in the introduction to my book[1] but was persuaded that such a discussion would be outside the scope of an article for PHYSICS TODAY. The statistical accuracy of the epidemiological studies is marginal and they are all very prone to systematic error (including the ones on electrical workers that Trainor mentions but does not cite). I did not mean to imply that Paul Brodeur started all current interest in the biological effects of electromagnetic fields, but he does deserve a lot of credit for stirring up panic on the cancer issue in the general public. The numerous exaggerations and misrepresentations in Brodeur's book[6] based on his *New Yorker* articles have been discussed in detail elsewhere.[7] The very title of his book states that there has been a "cover-up." His last *New Yorker* article on the subject[8] ends with the question "How many more cancers will it take?" (To do what, shut down the entire electric power industry?) Those are just a few examples of what I meant by "sensationalism." As I stated in my PHYSICS TODAY article, I most certainly did *not* conclude that no further research should be conducted on biological interactions with ELF fields. The question is, How much public money should really be spent on this problem? I, evidently, had vastly underestimated the recent expenditures at a mere billion dollars. While he was serving as science adviser to President Bush, D. Allan Bromley estimated that the present EMF–cancer scare had cost American society more than $23 billion since 1989!

Kenneth R. Brownstein is of course quite right in noting that the electric field inside a completely closed conductor is rigorously zero and that only an electric field of $-\mathbf{v} \times \mathbf{B}_{\text{Earth}}$ is seen in a reference frame traveling at velocity $\mathbf{v}$ through a uniform magnetic field ($\mathbf{B}_{\text{Earth}}$) equal to that of the Earth. Nevertheless there are many situations in which people do travel 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.

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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.

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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