shifts. Moreover, it has been related by Edward Witten to the underlying theory of quantum chromodynamics in the limit of light quark masses and a large number of colors. Further, the Skyrme solitonic approach has been derived explicitly by Yitzhak Frishman and collaborators in a two-dimensional reduction of QCD with any number of colors. The writers of well over a thousand papers have taken the model sufficiently seriously to discuss its applications in many aspects of particle and nuclear physics.

For these reasons we think it suggestive that the Skyrme model predicts that Σ, the quark helicity contribution to the nucleon spin, vanishes for light quarks in the limit of a large number of colors. Neither of these assumptions is exact for the actual physical situation. However, the available experimental data from the EMC, followed by that from the Spin Muon Collaboration and the Stanford Linear Accelerator Center, lie within the likely accuracy of these approximations. The Skyrme model can thus provide an example of a limit in which the so-called spin crisis can be explained. We urge others, as well as ourselves, to strive to overcome the model's imperfections and relate it to the more familiar constituent quark model, which did not prepare us for the EMC result.

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Jaffe replies: I apologize to my friends for my patently incorrect assertion that "no one takes it [the Skyrme model] very seriously as a way out of the spin crisis," and for my having associated my negative remark so closely with their stimulating paper (which I have read with interest and refer to often). I should have said "I do not take it very seriously as a way out of the spin crisis"—a phrase that I would apply at one level or another to all of the models on the market.

Exciting physics almost always spawns controversy and, as I tried to indicate in my article, theorists continue to debate the origins of the spin crisis passionately. Meetings on QCD spin physics are enlivened by debates among adherents of the Skyrme model, gluonic anomalies, chiral quark models, and so on. There is no satisfactory model of hadron structure: Quark models do not adequately describe chiral symmetry; the Skyrme model and other models based on chiral dynamics do not give an adequate description of many quarkish features of hadron structure. Richard Feynman used to classify theorists and their models as those who worship "the quarks" and those who worship "the pion." I think it best to view them all as toy models, useful for gaining insight into phenomena but dangerous to take too seriously.

This is not the place to debate the virtues of the Skyrme model at length. Briefly, I have two reasons for not taking it very seriously as a solution to the nucleon spin problem.

First, the problem lies in a flavor average channel, where the model has particular difficulties. The model cannot be formulated in a world with only one quark flavor—although QCD has no fundamental problems with such a world. In addition, the model predicts that the mass of the flavor-average pseudoscalar meson ($M_{\eta'}$) should vanish, and the mass of the flavor-average baryon (M) should go to infinity in the same limit (that of an infinite number of colors) that it predicts $\Sigma \to 0$. In the real world $M_{\eta'} > M_N$.

Second, the model is most successful describing a world of only up and down quarks. Extensions to include strangeness have been problematic, but the appearance of strange quarks in the nucleon is at the heart of the spin problem.

I agree with Brodsky, Ellis and Karliner that these reasons should spur us not to abandon the model but rather to make further efforts to understand and improve it. I look forward to many lively debates on this subject in the future.

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APS Statement on EM Fields Debated

The lively controversy regarding possible health effects of environmental electromagnetic (EM) fields has prompted the American Physical Society to go on record (as reported in the *New York Times* on 14 May 1995) as saying that "conjectures linking cancer to power line fields have not been scientifically substantiated." Further, the APS has taken a policy position arguing for elimination of research in this area on the grounds that using funds to combat "a threat which has no persuasive scientific basis" leads to the neglect of "more serious environmental problems."

One might be tempted to dismiss the statements of a group whose expertise is so far afield from the medical issues they are considering. However, given the many contributions of physicists to solving problems in biology, it is important to give full consideration to the APS assertions. The issues that need to be addressed can be framed in two questions:

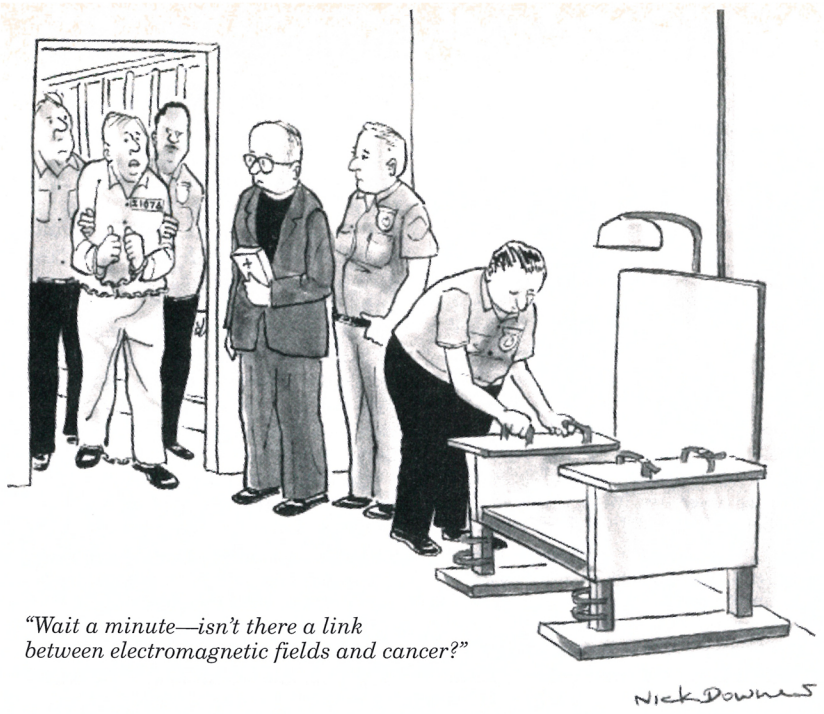
▷ Are the physicists aware of relevant work in biology that contradicts their conclusions?

▷ Is the highly simplified model of a cell that is critical to their conclusions relevant to processes in cells that can be affected by EM fields?

Our answer to both questions is no.

For a start, the authors of the APS statement do not appear to know of the work carried out by several biological laboratories that shows that weak EM fields have significant effects on biochemical reaction rates, including changes in protein biosynthesis.[1] The changes caused by EM fields are similar to the response of all cells to stress ("the stress response"), whereby cells synthesize stress proteins to control damage brought about by environmental stimuli. In other words, cells interpret EM fields as a noxious stimulus, and there is strong evidence that the fields stimulate the synthesis of stress proteins. It is obvious that if the cells are compromised or overexposed, the stress response may not be able to compensate. One cannot simply overlook these scientific developments because they have not been published in physics journals.

The physicists seem not only to have ignored the published biological evidence, but also to have based their conclusions on calculations using a highly oversimplified theoretical model of a cell, not on real cells. Their theoretical creation has as its only structure a cell membrane, whereas a true cell contains many vital structures. This is a critical issue because the membrane may not be the most relevant part of the cell with regard to the stress response. (Recent measurements on DNA, for example, have shown that the double helix has a very high electronic conduction,[2] and bases can be made to flip out of the double helix.[3] These new properties may be involved in interactions with EM fields.) Further, the physicists' oversimplification continues in that they have conceived of their model membrane as a simple uniform boundary layer, whereas an actual true cell membrane has multiple structures.



"Wait a minute—isn't there a link between electromagnetic fields and cancer?"

Calculations based on simple physical models that do not reflect complicated systems must be suspect. Extrapolations to the disease process are even more suspect, since that situation is far more complex.

The situation we are witnessing is not new. Probably the best-known example of a respected physicist using an idealized model to make correct calculations but reach wrong conclusions was Lord Kelvin's attempt in the 1860s to estimate Earth's age. In the controversy surrounding the subject, some geologists had correctly proposed that Earth was billions of years old. Drawing on Fourier's laws of heat transfer, Kelvin calculated that Earth was probably about 100 million years old, and certainly not more than 400 million years old. To be any older, he reasoned, Earth would have to have lost enough heat to have cooled into a solid mass—but it still had a molten core. Kelvin's conclusion was wrong because his model was incomplete; it did not—and could not at that time—take into account the then-unknown heating effects of radioactive decay.

Where does that leave us? Biologists have found significant effects on cells due to even weak EM fields. Admittedly we cannot be sure if there is a health risk associated with exposure—but at the same time, we are in no position to deny this possibility, especially in light of the consistent link between EM field exposure and childhood leukemia found in epidemiological studies. Consequently, it might prove disastrous to cut research efforts in this area while the jury is still out. We encourage physicists to continue their work in this area, refine their models, and help biologists to answer the necessary questions. However, they should be aware of their limitations, and above all, not place themselves in the position of policymakers on issues that are vital to the American public.

References

1. See, for example, M. Blank, R. Goodman, *Bioelectrochem. and Bioenerg.* **31**, 27 (1993); **33**, 109 (1994). See also Adv. Chem. **250** (1995), which is devoted entirely to the issue of biological interactions and mechanisms associated with EM fields.
2. D. Cleary, Science **267**, 1270 (1995).
3. S. Klimasauskar, S. Kumar, R. J. Roberts, X. Cheng, Cell **76**, 357 (1994).

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The American Physical Society's ill-advised policy statement on cancer and electromagnetic fields as reported in PHYSICS TODAY (July 1995, page 49) prompts me to write this letter. The review by William R. Bennett Jr. (April 1994) and the many letters in response to it (January 1995) should be commended but they missed two basic points in cellular biology: (1) the environment operates on an organism on a cellular basis, and (2) the electrochemical equilibrium of a cell governs the transport of many substances across the cell membrane.

Just as researchers presented confounding results in early studies of dioxin, one of the most toxic substances known to man, current EM studies present confounding results. Workplace studies on EM issues in the US have been presented that are as convincingly negative as the recent Swedish studies are convincingly positive. It seems unconscionable to suggest that, because all previously proposed links between cancer and EM fields have been shown to be unfounded, more money should not be spent on research. We have not reached a level of certainty in our knowlege of either molecular biology or cellular ecology to accept such a suggestion.

Electromagnetic field theory is basic physics, but it is also basic biology. How much of an EM field is necessary to temporarily short out a human cell? How does even a weak EM field affect the electrochemical equilibrium of a cell? To what extent does an EM field affect the allosteric transitions that allow proteins to mediate the movement of metabolically necessary substances within the human body? These questions are ones as much of physics as of biology. We are all scientists with the common goal of understanding our physical and biological world.

Other single-science organizations have made statements they have come to regret. The American Association of Petroleum Geologists came out against continental drift in the early 1970s. Two decades later, I fear that our statement on the role of EM fields and cancer may come back to haunt us.

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HAFEMEISTER REPLIES: Blank and Goodman are incorrect. As chair of the APS Panel on Public Affairs's Subcommittee on Power Line Fields and Public Health, I can say categorically that the APS has not "taken a public policy position arguing for the elimination of research" on the possible health effects of extremely-low-frequency electromagnetic fields. The APS statement is silent on the merits of ELF-EMF research budgets.

Blank and Goodman seem to have misquoted the APS statement's third paragraph, which discusses litigation and mitigation costs—not research funds. The APS paragraph reads as follows: "These unsubstantiated claims, however, have generated fears of power lines in some communities,

leading to expensive mitigation efforts, and, in some cases, to lengthy and divisive court proceedings. The costs of mitigation and litigation relating to the power line–cancer connection have risen into the billions of dollars and threaten to go much higher. The diversion of these resources to eliminate a threat which has no persuasive scientific basis is disturbing to us. More serious environmental problems are neglected for lack of funding and public attention, and the burden of cost placed on the American public is incommensurate with the risk, if any."

Clearly, the APS is very concerned about the consequences of people overreacting to unfounded fears—namely, the present cost of between $1 billion and $3 billion per year for mitigation and litigation and a possible cumulative future cost of more than $250 billion if a proposed national standard of 2 milligauss is imposed.[1]

Blank and Goodman have concluded that physicists are not aware of "relevant work in biology that contradicts their conclusion." However, the APS subcommittee is indeed aware of ELF-EMF-related work in biology, and agrees with most reviewers of this work that the biology does not support a conclusion that power-frequency fields of the magnitude encountered in residential and most occupational settings pose a hazard to humans. The subcommittee considered epidemiological studies, biomedical–biophysical experiments, physical mechanisms and mitigation and litigation costs, and it widely consulted the biomedical community, the APS division of biological physics, and the reports of about ten interdisciplinary (biological, medical, epidemiological and biophysical) review studies on the ELF-EMF issues.

None of those studies determined that society should spend funds on mitigating common ELF-EMF levels. For example, the American Medical Association concluded that "most studies of magnetic field effects in children, workers, and other populations do not meet accepted scientific criteria in terms of accurately measuring past exposure, identifying comparable test and control groups, and accounting for potentially confounding factors. Findings of studies are inconsistent in terms of whether a risk exists, what conditions might be related to exposures, and risk magnitude. Positive studies indicate, for the most part, that the associated relative risks are low."[2] Further, an interdisciplinary Oak Ridge study concluded that "there is no convincing evidence in the published literature to support the contention that exposures to extremely low frequency electric and magnetic fields generated by such sources as household appliances, video display terminals, and local power lines are demonstrable health hazards."[3] It also should be noted that the various interdisciplinary studies determined that there is a major problem of replicating the results of the positive studies.

Blank and Goodman also have concluded that the model of a cell used by physicists is highly simplified and not relevant to actual processes in cells that can be affected by EM fields. Physicists generally accept that the internal electrical fields caused by thermal oscillations and natural processes in the body are much larger than ELF-EMF-induced electrical fields in the body. This does not prove there are no other mechanisms that might cause cancer, although no such mechanisms have yet been found. Nor does the APS statement rule out this unlikely possibility.

As for Blank and Goodman's comments about Kelvin and Keyes's comments about continental drift, of course one can find examples of scientists having erred. In fact, I would also raise the issue of "cold fusion"; some 200 papers have claimed a positive finding, but not many scientists take them seriously. I see a parallel between ELF-EMF and cold fusion, in that in both cases there is a lack of a viable mechanism and of consistent empirical evidence in support of the hypothesis. I predict that the ELF-EMF experimental evidence will continue to crumble after further research.

Science is full of honest debates, but when we are confronted by the prospect of paying out billions of taxpayer dollars for mitigation and litigation purposes and of spreading widespread fear in the citizenry, we must exercise broader responsibility than wasting our research resources on small, very unlikely and hypothetical pathways to death.

References

1. For details and specific references, go to http://www.calpoly.edu/~dhafemei on the World Wide Web. For an up-to-date review of ELF-EMF issues, see J. Moulder, K. Foster, Proc. Soc. Exp. Biol. Med. **209**, 309 (1995).
2. American Medical Association, Council on Scientific Affairs, *Effects of Electric and Magnetic Fields*, Chicago (1994).
3. Oak Ridge Associated Universities, *Health Effects of Low-Frequency Electric and Magnetic Fields,* Oak Ridge, Tenn. (1992).

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Review and Reveal the Reviewers

To ameliorate such peer-reviewing problems as reviewer procrastination and asymmetrical application of the golden rule, I suggest two possible approaches that would incorporate desirable feedback procedures without necessarily sacrificing anonymity.

First, editors not only should strongly emphasize the importance of reviewers adhering to the requested two- or three-week review response time but also should inform reviewers that if a review is not received within a specified interval (four–six weeks seems more than generous), then (barring rare extenuating circumstances) that reviewer's name will be disclosed to the author of the manuscript. This tactic by itself would certainly reduce average review time.

Second, two lists of reviewer names should be published by each journal at the end of the year. The first list should contain the names of those reviewers who were dilatory (by the above criterion) during the year. The second list should contain the names of reviewers who met their obligations in timely fashion during the year. Publication of such lists at the same time of the year by many journals would enable journal editors to be better able to recognize and avoid using slow reviewers whenever appropriate. Even if the first approach outlined above were deemed too draconian, this second approach would certainly encourage reviewers to eschew tardiness.

In addition, I suggest that when an author submitting a manuscript is known to be a reviewer on the slow list, the reviewer for this manuscript be chosen from the same list! Such selection would also be desirable for manuscripts submitted by authors who have published in a given journal but have since rejected several requests from the journal to serve as a reviewer.

Widespread implementation of the above proposals might initially reduce the supply of willing reviewers, an undesirable result. But this is unlikely to be the case for the steady state once the procedure becomes well known and reviewer lists are routinely published. Although implementation might weed out some of the worst offenders, especially if they do not expect to ever publish again themselves, it should cause most slow reviewers to mend their ways and begin to meet their professional responsibilities with