ELECTION ISSUE
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PHYSICS AND SOCIETY

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Physics and Society is the quarterly of the Forum on Physics and Society, a division of the American Physical Society. It presents letters, commentary, book reviews and reviewed articles on the relations of physics and the physics community to government and society. It also carries news of the Forum and provides a medium for Forum members to exchange ideas. Opinions expressed are those of the authors alone and do not necessarily reflect the views of the APS or of the Forum. Contributed articles (2000 words maximum, technicalities are encouraged), letters (500 words), commentary (1000 words), reviews (1000 words), and brief news articles, are welcome. Send them to the editor: Art Hobson, Physics Department, University of Arkansas, Fayetteville, AR 72701, 501-575-5918, fax 501-575-4580, bitnet AHOBSON@UAFSYSB. Typist: Sandra Johnsen. Layout: Page Perfect of Fayetteville.

LETTERS

James Randi

The issue of science vs claimed psychic powers is not yet dead. Uri Geller, the Israeli magician who claims to be able to bend spoons and keys with the power of his mind—as well as many other fantastic tricks—is suing the 1989 Forum Award winner, James Randi, for $15,000,000! To date Randi has had to lay out almost $200,000 of his own money to defeat Geller's suit; now Randi is broke and needs financial assistance to continue his defense. Besides imperiling Randi, who received his Forum Award for his tireless pursuit of scientific quacks and frauds, this case cuts to the heart of our freedom to discuss scientific results with which we do not agree. If Randi loses, any scientist whose results are criticized or refuted in public could sue his critics, and possibly win.

The recent example of cold fusion, with accompanying threats to bring suit against its detractors, serves as a reminder of what can happen even in the physics community. In fact, Randi received his award from the APS the morning after the 1989 cold fusion presentation; then-president Val Fitch remarked, when Randi stepped forward, that he had not believed until the night before that *physics* needed the kinds of defense Randi had recently provided for the life scientists, but, Fitch said, he had been convinced.

The Forum Award citation to Randi read: "For his unique defense of science and the scientific method in many disciplines, including physics, against pseudoscience, frauds and charlatans. His use of scientific techniques has contributed to refuting suspicious and fraudulent claims of paranormal results. He has contributed significantly to public understanding of important issues where science and society intersect." Before cold fusion's "discovery" many of our colleagues wondered why the APS had selected a magician for a prestigious scientific award; after Randi discussed his challenges to homeopathic medicine and fraudulent experiments in France, there was no dissent about our choice of Randi.

Geller has stated that "I brought [bending spoons] into the world. No one was bending with the power of the mind before me." During his Forum Award lecture Randi bent a spoon with little apparent evidence that mechanical forces were at work, but he took care to explain that no supernatural powers were involved, only his skilled hands. Magic shows are fun; but fraud and pseudoscience are dangerous, which marks the difference between Randi and Geller.

If your want to help defend James Randi, and incidentally your own First Amendment rights to speak out against pseudoscientific fraud, you can send a contribution to: c/o Robert Steiner CPA, P.O. Box 659, El Cerrito, CA 94530.

Checks should be made payable to the James Randi Fund.

Even more than money, Randi tells us he needs to find physicists who once believed that Geller had paranormal powers but who have been convinced, one way or another, that Geller was a garden variety faker. If you are such a physicist, contact us; we'll put you in touch with Randi.

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Nukes In the Gulf

In his Commentary (July 1991), Lawrence Cranberg argues that use of tactical nuclear weapons against Iraq would likely have reduced total property damage and loss of life. The rationale here is that the shock of being a nuclear target would immediately have caused Saddam Hussein to surrender. He cites the eight days between the bombing of Hiroshima on 6 August 1945, and Japan's surrender on 14 August 1945, in support of his view.

I too believe that fewer lives (both Japanese and Allied) were lost in WW II by using nuclear weapons to shorten the conflict, but there my agreement with Cranberg ends. He is incorrect factually in stating that the US gave Japan a "massive public alert" in advance of the bombing. This was not done, to avert prisoners of war being moved into the cities, to preclude improved defenses around the targets, and to minimize our own "loss of face" in case the bombs fizzled (1). Further, Cranberg's implication that the destruction of Hiroshima and Nagasaki and Japan's surrender bore a cause-effect relationship is debatable. Allied conventional forces really won the war; at best the nuclear weapons pushed its termination forward by some months.

But historical analogies (which usually have some flaw) and technical support for a position (which is the Forum's customary pattern) are not at the heart of Cranberg's position. As his prose indicates (calling the opposition "issue-seeking demagogues and easily-misled scientific illiterates"), his statement is political, a value judgment. As such, it may be contested by other value judgments.

Since World War I, we have been told that poison gas is a more efficient and sometimes more humane way to prevail on the battlefield than by blowing the enemy to pieces. Yet, most nations recoil with horror at its use; we have a phobia against gas, thanks in part to media-formed public opinion which Cranberg condemns. I happily accept the ban on gas, even if its use has some merits, just as I will praise the rejection of any weapon. Psychologically, it is important for people to know that at least some forms of mayhem are proscribed.

Regarding the Gulf War, we simply do not know that Iraq would have capitulated quickly under nuclear attack. But whatever Iraq's reaction, it is likely that the US would have been condemned by virtually every nation on this globe. Not only would the image of the US as a nuclear bully have been established (in violation of the spirit of the Non-Proliferation Treaty (2), as well), but the nuclear genie would have been let out of its bottle, making it all the more likely that these weapons would see use in other conflicts. Nuclear policy is

properly based upon long-term considerations, and abstention was the proper policy in the Gulf.

1. Letter to L. Badash from Richard Hewlett, chief historian of the AEC and DOE, 1 November 1980. See also Richard Hewlett and Oscar Anderson, *The New world, 1939/1946* (Pennsylvania State Univ., University Park, 1962), 369, for the interim committee's position that the bomb should be dropped without warning.
2. On 12 June 1978, Secretary of State Vance stated: "The US will not use nuclear weapons against any non-nuclear weapons state party to the NPT—." See Arms Control *and Disarmament Agreements* (ACDA, Washington, DC, 1982), 87.

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Response:

My source on A-bomb warnings to the Japanese is Richard Rhodes, *The Making of the Atomic Bomb*, Simon and Schuster, 1986, 737.

Where Badash and I really part company is that he would allow public policy to be governed by "phobias" and "genies," and by subjective guesses about what is "psychologically important." But if physicists are going to have useful things to say about physics and society, it will be by encouraging rational consideration of imaginable alternatives, and not by supinely accepting existing states of mind. For example, had anyone publicly discussed the effect of a few tactical nukes launched against Saddam by Kuwaiti aircraft?

If scientists are to be trusted to be "on tap" rather than "on top," they must use their creativity and professional skills to suggest options (1), and not to preclude them in pursuit of private agendas and thereby try to dominate policy debate. If Professor Badash has a private agenda that requires him to "praise the rejection of any weapon," he may particularly deserve his membership on the Forum's Executive Committee, but he is in no position to lecture anyone about making "political" or "value judgments."

There was virtually no public discussion of the nuclear option at a time when the Center for Defense Information was predicting we would suffer 45,000 casualties in the Gulf. When phobias and genies exert so great an influence on public policy as to smother open discussion even among scientists, we scientist simply are not doing our jobs.

The fact is our military-political leaders have been doing splendidly at putting genies back in bottles with no help at all from the Forum—for example, century-old pseudoscientific socialism and twenty-year-old Vietnam pacifism-defeatism, both of which godfathered the birth of the Forum. Is it time to put the Old Forum itself back in the bottle, and give the New Forum on Education a clear field?

1. Letter to L. Cranberg from the Navy Member of the Joint Chiefs, R.G. Brodsky, Captain, USN, December 2, 1991. "The military members of the Chairman's Staff Group have had several discussions inspired by your articles. While the consensus has not always been in consonance with your viewpoints (particularly on the subject of nuclear weapons), you have stirred thoughtful debate. That, in itself, can be counted a breakthrough during these busy times."

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Controlled Fusion in Space

Controlled nuclear fusion with lower compactness/output ratios than the ones described by Maglich and Holt (July 1991) may eventually be developed by going above Earth's atmosphere. Small (0.1 gm) projectiles could be shipped out into space, accelerated toward Earth, and made to spread out, home in on and impact-ignite fusion microexplosion in (2.5 gm) pellets at spacecraft or power plants in near-Earth space. The projectile carriers would then rapidly decelerate away from Earth, pick up fresh projectiles, and repeat the cycle. About thirty should suffice for continuous power generation.

Impact-ignition should occur at relative velocities of 4,000 km/sec, and perhaps even at 450 km/sec with properly-designed projectiles and pellets. An initial (600-2000 tonne) projectile carrier could be propelled (and its projectiles spread out) by small (25 kg, 0.25 kiloton) fission bomblets (as in the Orion project), and subsequent (500-700 kg) carriers by fusion microexplosions impact-ignited by projectiles from other carriers differing sufficiently in velocity, e.g. when travelling in the opposite direction. The former could be lightened considerably if smaller bomblets could be developed.

After release from its carrier, a projectile could be steered by thrust-producing surface ablation induced by external laser pulses. The power generated in space could be used for spacecraft propulsion, or be beamed, e.g. by 1x1 km^2, 2.4 GHz, 10 GW microwave antennas, to 10x10 km^2 antennas on Earth for ground consumption.

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ARTICLES

On Taking Horses to Water

John H. Gibbons

[*Editor's note*: John Gibbons is winner of the 1991 Szilard Award for physics in the public interest. He is Director of the US Congressional Office of Technology Assessment (OTA), Washington, DC 20510-8025. The award citation reads "—for leading and greatly strengthening the OTA, an institution that has produced balanced, thoughtful, and influential assessments of public policy issues dealing with science and technology." Gibbons, according to a nominating letter, "has over the past decade led and greatly strengthened an institution that has become known for its balanced, thoughtful, and influential assessments of public policy issues dealing with science and technology." The following article is based on his lecture at the Forum's award session at the APS Spring Meeting, 22 April 1991, in Washington, DC.]

I was deeply touched by being selected for this year's Szilard Award. First of all, it is particularly gratifying when my colleagues remember me despite the fact that I have been away form the laboratory bench for longer than I would care to admit! Suffice it to say that today there are 21-year-old graduate students who were born the year I last turned on an accelerator! Second, you should know that my shift from experimental physics to technology assessment (TA) was made not orthogonally, but by finite differences, thanks to the support and accommodation of Alvin Weinberg, then my Director at ORNL. He orchestrated the creation of early forms of TA (mostly of issues in energy and environment) at the Lab in the late 1960s and early 1970s, and I had the opportunity to lead that work into such brand new areas as analysis of energy demand and efficiency of use.

Some years later, in the course of setting up the Energy, Resources, and Environment Center at the University of Tennessee, I gained an education and practical experience in the complexity of public policy-making with respect to Science and Technology (S&T). A year and a half in the executive branch of government, as first director of the Office of Energy Conservation, enabled me to participate in the practicalities and complexity of political compromise and decision-making. I returned to the University life convinced that if I ever returned to government, it would have to be in the legislative branch!

During the 1970s, I became increasingly concerned about the paucity of *effective* ways for decision-makers, especially our "citizen governors"—the elected members of Congress—to access technical information. When OTA's Congressional Technology Assessment Board invited me to become Director, I was an easy target because, ever since my transformation that began at Oak Ridge National Laboratory (ORNL), my view of the importance of TA had grown from considering it "interesting" to considering it "imperative."

Since coming to OTA twelve short years ago, I have encountered nothing but confirmation that our ability to govern ourselves wisely hangs ever-more heavily on the ability of those that govern to have access to, as James Madison put it, "—the power that knowledge gives—."

In preparing for this talk, I had a chance to become better acquainted with Leo Szilard. His extraordinary innovations in science, his technology foresight, and his passionate concern about the special responsibilities of scientists as citizens make him a man of the century. His activities—from the early 1930s (when he foresaw nuclear chain reactions), to his many actions in World War II (such as drafting the letter that Einstein sent to Roosevelt), to his postwar efforts to put brakes on nuclear arms and ballistic missiles—reads like a novel. His mounting frustrations with the seeming inability of society to recognize and deal more directly with the perils of the times led him to write the allegorical tale "The Voice of the Dolphins." In that tale, scientists, frustrated with their inability to reach politicians and to influence society, capitalized upon the high intelligence of dolphins after learning how to communicate with them. The dolphins helped develop a new low-cost source of food (which mercifully was also an excellent birth control chemical), and some of the revenues from the sale of this food (Amruss) were devoted to purchasing a number of television stations around the world and operating them for public service.

One of the major television programs carried by the Amruss stations was devoted to the discussion of political problems. The function of "The Voice of the Dolphins," as this program was called, was to clarify what the real issues were. In taking up an issue, "The Voice" would discuss the several possible solutions and would indicate what the price of that particular solution might be.

It is for this reason and many more that I believe Szilard should be the Patron Saint of OTA!

OTA's mission is akin to Szilard's fervent hopes for better communication between science and society. While it carries out studies at the request of House and Senate committees of jurisdiction, OTA's results are fully available to the public. Our task is to address both unanswered questions and unquestioned answers about the interplay among science, technology, and governance. The range of subjects and issues covered is as broad as S&T itself. This means that our small agency must constantly reach out and network with various experts and stakeholders to assure the quality, completeness, and fairness of our analyses. In the end, we try to elevate and focus on the political debate surrounding socio-technical issues without taking partisan sides or becoming outright advocates for a particular solution. Thus, we must be sensitive to, and understand, the political process while simultaneously not being swept into it. We offer findings and options, but take great care not to advocate a particular political decision. We must understand very clearly Victor Hugo's observation that "science has the *first* word on just about *everything*, but the *last* word on *nothing*."

Hugo notwithstanding, science does lie at the very heart of humankind's aspirations to achieve physical amenities and security, to satisfy the uniquely human aspirations of discovery and exploration, and to enable people to be wise stewards of resources. But, like it or not, humankind and technology are simultaneously (and probably inherently) not only the height but also the depth of creation. Inevitably, even science has its downside because it cannot be either considered or understood separately from technology. However, science must be treated and governed differently from technology. The different ways that governance needs to be applied to these two intertwined enterprises still confounds us.

Another feature of Szilard that pertains to OTA is his commitment to share the insights and experience of science with the public. A democracy cannot long survive without the undergirding of an informed electorate. Szilard was a kind of Johnny Appleseed—he constantly moved around (intellectually and physically), incessantly creating ideas and theories and then spreading them, planting them in the minds of others. He was also a reflection of Paul Revere in his mostly

frustrated attempt to alert citizens, colleagues, and leaders to the perils before them. At OTA, we not infrequently feel the same pangs of concern and frustration of seemingly inattentive audiences, but the alternative of not trying is simply not acceptable.

Szilard lived in extraordinary times. There was a cascading, urgent need for scientists to speak up, and to devote a substantial measure of their energy to the broader public agenda. What about today? Are the issues and opportunities any less? I venture to suggest not. Reflect for a moment on the pace of change (mostly driven by S&T advances) and issues such as conflict resolution, population growth, environmental change. The bottom line becomes clear: S&T's place in providing new and better options for society—for better or worse—has never been so profoundly urgent.

"The Dolphin's Voice" still needs to be heard, but OTA's experiences imply one important modification to Szilard's vision: The voice of wisdom must arise from the effective synthesis of not only the natural sciences perspective but also of other persuasions—including the social sciences and humanities. But that is a matter that merits another day's discussion!

On behalf of all my colleagues at OTA, which includes a full measure of people who were originally trained in physics, I am profoundly grateful for the honor you bestow upon us today.

Symposium: Effects of Low-Level Electromagnetic Radiation on Living Systems

Physics and Society presents here three articles based on an invited session sponsored by the American Association of Physics Teachers at the April 1991 APS meeting in Washington, DC. Readers might also want to see the October 1990 *P&S*, for three more articles on effects of low-frequency fields based on a symposium at the April 1990 APS meeting, and also the January 1991 *P&S*, for several letters written in response to the October 1990 articles. *Editor*

Health Effects of Low-Level Electromagnetic Fields: Paradoxes and Problems

Kenneth R. Foster

The controversy about possible health effects of low-level electromagnetic fields raises several paradoxes, about the interpretation of scientific evidence as well as the role of science in risk assessment. My comments pertain to the link between 60 Hz magnetic fields and childhood cancer risk, which has been suggested by several epidemiologic studies (1-7); similar comments would apply to numerous other health claims about weak electromagnetic fields.

The epidemiologic evidence does not directly point to "fields" as causative agents (8). In the studies cited, the reported associations between "exposure" and cancer risk were small, near or below the level of statistical significance. Paradoxically, the associations were strongest (if such a world could be applied to such weak data) when the "exposure" was judged in terms of a "wiring configuration code," based on the proximity of the childrens' houses to neighborhood distribution lines or other electrical equipment. The associations between cancer risk and magnetic fields measured in the homes were smaller and, in most cases, not statistically significant. If the associations reflected a causal effect of "fields" the opposite should have been observed.

In fact, neither the wiring configuration code nor point field measurements in the home can reliably indicate a person's actual exposure to power-frequency magnetic fields. Studies using recording dosimeters show that a person's actual exposure varies widely, due to household appliances and other local sources, and this variability far exceeds that associated with the different wiring codes or point measurements in the home.

Some people have argued that the wiring configuration code is a better measure of "exposure" than direct field measurements in the home. But this claim cannot be evaluated without knowing what parameters of the exposure are important. An alternate possibility, equally hard to prove, is that the reported associations were spurious, due to some slight bias in the studies. The question remains what the epidemiologic studies imply about cancer risk from electromagnetic fields. Not much, I believe.

The fields implicated by the epidemiologic studies are small compared with natural sources of "exposure." If the epidemiologic data do reflect some real risk from 60 Hz magnetic fields, they imply that the threshold for detectable increase in risk is about 2 mG, but the earth's magnetic field is 500 mG. As a person moves about his body will experience time-varying magnetic fields that far exceed the hazardous level of 2 mG, although the dominant frequency components are considerably below 60 Hz.

If the hazard resulted from electric currents the magnetic fields induced in the body, even greater paradoxes arise. A simple calculation shows that a 2 mG, 60 Hz magnetic fields induces electric fields within the body of roughly a microvolt per meter. But the naturally occurring electric fields (from the EEG, ECG, and other bioelectric sources) are roughly a million times stronger—and in precisely the same frequency range (9). One might argue that the body is somehow "tuned" to 60 Hz, but the degree of tuning needed to overcome a noise to signal ratio of a million to one must be astonishingly high.

There is a gross discrepancy between thresholds for observable effects predicted by biophysical theory, and the field strengths implicated by the epidemiologic evidence. There are, of course, many physical mechanisms by which electric or magnetic fields can interact with biological systems. These include field-permanent dipole and field-induced dipole interactions (both electric and magnetic) and membrane excitation (10-11). The study of these interactions has, of course, been a longstanding preoccupation in science.

These interactions occur in the presence of random thermal fluctuations, which implies that thresholds must exist for fields to produce observable effects. Such thresholds, where they can be calculated, invariably exceed, by many orders of magnitude, the field strengths implied by the epidemiologic evidence.

These have been many well-established biological effects of electric and magnetic fields (9). For example, humans can detect external 60 Hz electric fields of about 10 kV/m, apparently due to vibrations of hair on the body; alternating magnetic fields of about 200 Gauss will elicit visual sensations (magnetophosphenes) in humans, from induced currents in the retina (12). These effects require field strengths far above those implicated by the epidemiologic studies, and

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moreover have no obvious relations to hazard.

To be sure, some organisms respond to much weaker fields. For example, sharks and rays can sense DC electric fields below 1 microvolt per meter in the water in which they swim (13) by means of specialized sense organs (the ampullae of Lorenzini). Some bacteria are passively oriented by the earth's magnetic field, by virtue of small magnetic particles contained within the cells (14). These effects have conventional explanations, and have no apparent connection to hazard.

The bioeffects literature is very noisy, with many unexplained or nonreproducible phenomena, and often speculative.

The bioeffects literature is very noisy, with many unexplained or nonreproducible phenomena, and often speculative. Many biological effects have been reported from electric or magnetic fields, which have no apparent explanation, and which frequently cannot be independently confirmed (9). Thus, the experimental facts themselves are frequently uncertain.

Also, the bioeffects literature is awash with speculation, sometimes by prominent physicists, about possible mechanisms by which weak fields might produce biological effects. Typically these speculations are purely formal (with no way to put numbers in the theory), ignore important effects such as thermal agitation, or can be criticized on obvious theoretical grounds. Adair has more to say about this elsewhere in this issue. The microwave bioeffects literature has suffered from similar confusion for many years (15).

A recent controversy illustrates these difficulties. In 1987 Liboff et al reported a strongly frequency-dependent effect of weak alternating magnetic fields on the uptake of calcium by lymphocytes (16). The investigators cited this effect as evidence for cyclotron resonance of calcium in cell membranes, an explanation that can be criticized on obvious theoretical grounds (17). Prasad et al could not confirm this finding, in a study with improved methods and a blinded design (18). Curiously, these investigators did observe a small "effect" in their initial experiments. Subsequent analysis uncovered a subtle cue that broke the blinding of the study. When the experiments were repeated with more rigorous controls, the effect disappeared. Did the original investigators overlook a subtle artifact? Or did the followup study neglect some important condition for detecting the effect? Interested readers should look up the original papers and draw their own conclusions.

A second example is the growing literature on possible teratogenic effects of pulsed magnetic fields. Nearly two dozen studies have been reported, that vary widely in quality, exposure parameters, and animal model. Some of these reported effects, and other did not, even in the same biological systems and under similar exposure conditions.

One reviewer concluded: "—we cannot clearly relate an increase in the incidence of abnormal embryos resulting from exposure to pulsed magnetic fields to any patterns of pulse frequency, field intensity, pulse shape, or rate of change in the intensity. —Until the important variables in pulsed magnetic fields are determined and the mechanism of effects is identified, it may not be possible to extrapolate such effects to humans." (19)

In short, these biological phenomena are neither predictable nor understandable in terms of mechanism nor demonstrative of risk to humans. To what extent is this merely scientific noise—and how can we tell?

Much confusion arises, in part, because of the nature of the research itself. Many (most!) bioeffects studies are exploratory in nature, seeking to identify new phenomena. An investigator will often report an "effect" on the basis of some statistically significant difference between the exposed and control groups of animals. Whether this difference arises from a direct effect of the exposure, from a subtle uncontrolled variable, or from a statistical false positive, requires inferences that are difficult to make in early stages of research.

Adair has noted the importance of "sifting and winnowing" in science, which occurs as scientists come to understand new phenomena through hypothesis-testing research. For many reported biological effects of electric and magnetic fields, this process has barely begun.

Resolution of the Issues

These paradoxes raise quite different questions, and their resolution must proceed at different levels.

1. Is there hazard from exposure to weak electric or magnetic fields? This question pertains to hazard identification, an initial stage in the process of risk assessment (20). These issues will most likely be resolved through the kinds of evidence normally brought to bear in risk assessment: The most relevant evidence comes from epidemiology, followed by certain kinds of animal screening studies. Relevant, but still more difficult to relate to humans, are standard in-vitro tests (e.g. the Ames test). Electric and magnetic fields have routinely failed standard tests for primary carcinogens or tumor promoters.

Most of the bioeffects studies that Carstensen describes (9) are more basic in nature, with no clear connection to risk. Such studies could provide an "anchor" (in Bailar's expression (21)) to guide risk research and help predict the conditions under which a hazard might be present. For ionizing radiation, such an anchor is the understanding that biological damage is associated with the production of free radicals in tissue. No such anchor has yet emerged for weak nonionizing fields (and none may exist).

2. What is one to make of the many reported biological effects of electric or magnetic fields? Obviously, each reported effect needs to be considered individually. Some will turn out to be real and interesting; other will vanish upon further examination. Many reported effects, I believe, will eventually be clarified on the level of biology, through better-conceived or better-conducted followup studies. Meanwhile, it is important to avoid pseudoproblems: The question is not how "fields" might have caused any reported effect, but rather (as with any scientific evidence) what can be reliably concluded from the data, and how strong an experimental case an investigator has made for his or her conclusions.

In their fine book, In Search of Safety, Graham et al point out that risk research is notably contentious and murky (22). Controversies about possible hazards of weak electromagnetic fields have continued for decades over a shifting range of issues, with little or no demonstrable benefit to public health. As our society turns its attention to increasingly subtle hazards in the environment, similar issues will doubtless recur, and the process of "sifting and winnowing" will become more difficult and contentious. How can it be more efficiently carried out? Needless to say, our society has real and urgent health problems, and phantom risk is a very expensive diversion indeed.

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Biological Response to Weak ELF Electromagnetic Fields

C.J. Montrose and T.A. Litovitz

Initially stimulated by a group of epidemiological studies, the question of whether low-level extremely-low-frequency (ELF) electromagnetic fields can cause observable biological effects has become the focus of a scientific, societal, political and, at times, a media debate. Despite declarations that it cannot happen (1), experimental evidence exists associating exposure to weak ELF electromagnetic fields with such phenomena as transcriptional changes, increased bone-cell proliferation, enhanced trans-membrane calcium ion transport, elevated rates of abnormal chick embryo development and heightened activity of certain enzymes.

The point is that one can no longer simply dismiss electromagnetic-field-induced changes in cell functioning with the assertion that there is no credible evidence for their existence. The debate must now shift to questions such as whether the existence of electromagnetic-field-induced bioeffects imply any biohazards and how very weak ELF electromagnetic fields can affect cells that are immersed in an electrically noisy biological environment. It is on this latter point that physicists can and should be expected to comment constructively.

This issue has been addressed by Adair (2), who dismisses the experimental record, and argues that thermal fluctuations lead to local electric "noise" fields with rms values some 100 to 10,000 times larger than the external fields that have been reported as altering cellular behavior. In this context, he concludes that "—it does not appear to be possible for weak external ELF electromagnetic fields to affect biological processes significantly at the cell level—."

Yet the data make it clear that they do. There are two obvious possibilities. One can attempt to argue that Adair's calculation of the thermal fluctuations is off the mark by many orders of magnitude, and that the externally applied fields are not weak by this standard. Alternatively, granting his estimate of the noise fields, one must propose a means by which weak exogenous fields can influence cell behavior when the cell has evolved in such a way as to function normally in the presence of the much larger endogenous fields. Or stated another way, how can the cell discriminate against the large thermal-noise fields in order to respond to the weak externally-imposed fields?

Coherence requirements

Litovitz, Mullins, and Krause (3) have suggested that an important element in the cell's discriminating against thermal noise involves the temporal coherence in the exogenous field. They studied the effect of introducing partial incoherence into the ELF field on the specific activity of ornithine decarboxylase (ODC), a highly inducible enzyme required for DNA replication and cell proliferation. Their initial studies indicated that four-hour exposure to 100-mT, 55-Hz, 60-Hz, and 65-Hz fields effected a roughly twofold enhancement of ODC activity in logarithmically-growing murine L929 fibroblasts.

A random component was then added to the signal; specifically, the frequency was shifted between 55 and 65 Hz at selected intervals t = 0.1, 1.0, 5.0, 10, and 50 s. At each switch in frequency a random delay time dt << t (dt in the range 0 to 50 ms) was added to insure that successive intervals were incoherent with respect to one another. Measurements of ODC activity for four-hour exposures showed no increase for t £ 1.0 s and essentially the full twofold enhancement for t ≥ 10 s. For t = 5.0 s, the ELF field produced an intermediate effect—an increase by a factor of about 1.5.

Essentially similar results were found when developing chick embryos were exposed to sinusoidal ELF fields during the first 48 hours of incubation. When the frequency of the impressed field was maintained for longer than 10 s, a doubling of the abnormality rate from about 8% to 16% was noted. When incoherence on a time scale £ 1.0 s was introduced, no difference between the exposed and control populations could be detected; the abnormality rate for both was about 8%.

It is clear that some type of time correlation for about 10 s is essential in the field-induced enhancement of both ODC activity in L929 cells and abnormal development of chick embryos. Cells must carry on a kind of signal processing that allows them to disregard fields that are incoherent on time scales shorter than this. But ten seconds of signal averaging is not sufficient for detecting a 60-Hz signal that is a factor of 1000 weaker than noise; the gain in signal-to-noise is only about a factor of 25. Moreover, the field at any point is the sum of the exogenous field and the fluctuating

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noise field. Since there can be no question that this field exhibits temporal incoherence on a scale much less than 10 seconds, the question remains—how does the cell discriminate against the thermal noise field in order to respond to the exogenous field?

Biological cooperativity: a coincidence-detection mechanism

In many instances, chemical agents do not affect cell functioning unless their concentration in the vicinity of the cell exceeds some critical threshold value. When these sharp thresholds have been observed, biologists have interpreted the data in terms of the *principle of biological cooperativity* : more than one of the cell's membrane-integral receptor proteins must be simultaneously activated to induce a response. Sharp thresholds have also been observed in studies of ELF field-induced effects. This idea that a multiplicity of cell signal receptors are simultaneously activated suggests that a mechanism analogous to *coincidence detection* could be operative in discriminating against fields from thermal fluctuations. We assume that the direct "target" of the electromagnetic field is the assembly of neutralizing "counter-ions" in the immediate vicinity of the cell surface, and that the resulting motion of these ions produces effects at the membrane that are transmitted to the cell interior where modification of the biochemical reaction pathway is effected. A plausible supposition is that the ionic motion affects the binding of ligands to the roughly 100,000 receptor proteins (sensors) that are integral to the cell membrane. Binding ligands causes the production of intracellular effector molecules (second messengers) within the cell; the net effect is the transducing of the extracellular signal into an intracellular one. Cooperativity is required in such processes in that "more than one intracellular effector molecule must [simultaneously] bind to some target macromolecule in order to induce a response" (4).

One can no longer simply dismiss em-field-induced changes in cell functioning with the assertion that there is no credible evidence for their existence.

Because the average spacing between receptors is on the order of 100 nm to 1000 nm and the Debye screening length (roughly the range over which a given ion is not shielded from other ions) is about 1 nm to 10 nm, localized charge density fluctuations in the neighborhood of a given receptor will not influence motion of charges near other receptor proteins. Thermal noise fields thus are prevented from producing intracellular effects. Conversely, impressed ELF fields are *spatially* coherent over the cell surface and therefore produce charge density variations that are correlated at various receptor sites in the membrane. Consequently, they produce the required number of effector molecules to initiate a cytoplasmic response. This biological coincidence detection scheme allows the cell to be exquisitely sensitive to very weak *spatially correlated* electromagnetic fields while discriminating against the much stronger but spatially random (on the relevant distance scale) thermal noise fields.

In summary, we have presented a hypothesis that is consistent with the extant data that accounts for the sensitivity of cells to external electromagnetic fields that are several orders of magnitude weaker than endogenous thermally-driven noise fields. The idea is that cellular response to a field requires simultaneous activation of several membrane sensors, thereby enabling cells to discriminate against spatially incoherent thermal noise while maintaining susceptibility to correlated external signals.

Is this the way that cells respond to electromagnetic fields? Does the combination of spatial and temporal coherence of impressed fields defeat the protective mechanisms that cells have evolved to enable them to function in the presence of electrical noise? Perhaps; but the difficult experiments to test this suggestion are only now being designed. Whether they will yield persuasive results is an open question. However, to the extent that public health may be an issue, research on field-induced bioeffects must not be allowed to be obstructed. We must guard against both of the prevalent forms of electrophobia: Type A, typified by an unreasonable fear of electromagnetic fields and Type B, characterized by an unreasonable fear of knowledge about the effects of electromagnetic fields.

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Electrophobia Revisited: Are Biological Effects of Weak ELF Fields Possible?

Robert K. Adair

Much of my talk was devoted to material reviewed in *Physics and Society*, October 1990, and published in *Physical Review A*, **43**, 1039 (1991), which shows that effects at the cell level of weak electromagnetic fields in the environment are very much smaller than the natural thermal (kT) agitation and, therefore, such fields cannot be detected by cells let alone affect them deleteriously. I will not recount that material.

However, I will comment on some matters not covered previously. In particular, I discuss kT limitations on membrane rectification processes, I comment on the experimental record and the paper presented by Litovitz, and I criticize a much-quoted epidemiological study of occupational hazards and show other epidemiological data that shows clearly that childhood leukemia is not caused or promoted by environmental electromagnetic fields.

Rectification

In my previous papers, I did not discuss explicitly mechanisms such as those described by Weaver and Astumian (*Science* **247**, 459 (1990)) which addressed effects of internal fields (in tissues) much larger than those induced by external environmental fields (in the air around the biological system). By and large, the internal electric fields will be about 10^{-7} times as large as the inciting external fields. Since these mechanisms are interesting and important I reviewed the broad topic of thermal noise limits on the rectification of currents by cell membranes and state thermody-

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namic constraints which I believe may apply generally to the W&A mechanisms.

According to this rectification hypothesis, a very small increment in the voltage across the membrane induced by the negative (positive) half-cycle of an ELF electric field across the cell enhances the transmission through the membrane of some ion which is then quickly removed from the cell interior by processes in the cell. Since the ion is removed quickly in the interior of the cell, the ion current does not reverse during the positive (negative) part of the cycle and each following cycle adds another increment of ion transfer for periods of many minutes—or "over the whole lifetime of the cell."

Since the rectification mechanism must not operate on noise voltages—violating the second law—the diode must be imperfect and allow a certain back-current and thermodynamic principles require that the current-vs-voltage relation (i vs V) be described by the Nernst equation:

$$i = I_0(e^{Vq/kT} - 1) \overset{Vq \ll kT}{\rightarrow} I_0 \left[\frac{Vq}{kT} + \frac{1}{2}\left(\frac{Vq}{kT}\right)^2\right] \tag{1}$$

where $V = V_0 \cos(wt)$ is the incremental voltage across the membrane, q is the charge carried by the ion, and I_0 is equal to the maximum back-current through the diode. The mean DC rectified current is then

$$\langle i \rangle = I_0 \frac{1}{4}\left(\frac{V_0 q}{kT}\right)^2 \tag{2}$$

and the rectified current is proportional to the square of the voltage—as the classical square-law rectifier and is smaller than the AC current by a factor $\approx V_0q/2kT$. We consider some numerical results for a small field in the tissue of 0.01 V/m. Taking $V_0 \approx E_i \cdot 1.5\,r \approx 3 \times 10^{-7}$ volts, $V_0q/kT \approx 2 \times 10^{-5}$, and for doubly charged ions, q = 2e, then for r = 10 mm, a canonical value for the radius of the cell, and with other reasonable parameters, the rectified current across the membrane will be less than one ion every 15 minutes.

If the rectified current is to constitute a signal, the charge so transferred must not be much smaller than the charge transferred through thermal fluctuations in the ion flow across the membrane. But the instantaneous rms noise current, $i_{kT} \approx I_0 \approx 10^{-12}$ A, or about 1750 ion pairs per second and will vary (like Brownian motion) as the square root of the time. With that noise drift the rectified ion transfer would be larger than the mean noise fluctuation only after about 100,000 years!

Since this characteristic time varies approximately as $(V_0q)^{-4}$, changes in the parameters can sharply reduce this time and I consider only that rectification effects on cells of fields in the tissue smaller than 0.01 V/m are precluded by thermal noise considerations.

The experimental record

As of last November (1990) when I served on a committee that reviewed the National Cold Fusion Institute at the University of Utah, there were 90 experiments from 10 different countries, that "showed" effects attributed to cold fusion. But no one of the experiments was robust; none reported a well-defined signal from processes that were independently replicable. And few of use would bet that cold fusion is real.

Similarly, there are perhaps 200 laboratory experiments, reported over a period of 15 years and from all over the world, that claim to show effects that are caused by very weak low frequency electromagnetic fields. Again, not one is robust; not one has been independently replicated in a scientifically acceptable manner. A number of these concern results from measurements on avian eggs. Ted Litovitz has now presented another from a Catholic University group he heads.

Since the results reported by Litovitz have not been published, my criticisms must necessarily be cursory and imprecise (and, perhaps, subject to misunderstandings), but if the results are advertised on the basis of what may be insufficient information, they must be subject to criticism on that same basis.

Litovitz made measurements based on the protocols of "Project Henhouse," a set of similar experiments designed to measure possible teratological effects of low-level pulsed magnetic fields on chicken eggs. The experiments were conducted by six different groups around the world according to procedures defined by the Navy. Four groups found no effects and two groups found effects on eggs exposed to 10 milligauss pulses 500 ms wide generated at a rate of 100 per second. The intensities of these pulses are at least two orders of magnitude less than that which I claim are masked by kT noise at the cell level and perhaps three orders of magnitude below that where biological effects have been definitely established.

The incidence of teratological aberrations in avian eggs is known to be a very sensitive function of the egg genetic, temperature, and handling history; hence egg teratological data is very noisy at best. As a consequence of such difficulties, many (most?) experts in such matters criticized the Henhouse experiment severely and considered the experiment ill-designed, ill-advised, and discounted the results.

In brief, the CU groups claimed to have found a dose-response step-function in teratological effects; there were few defects in the eggs exposed to fields below 10 milligauss but many defects were found, at a constant level independent of the field strength, for greater fields. And they claimed that they saw effects from 60 Hz fields which disappeared when the phase of the excitation was changed randomly every few seconds.

They reported that they examined about 2000 eggs in the course of their experiment and found about 500 with defects. Hence, this Herculean effort resulted in only about 500 events—for a process with very large and ill-defined noise. Were their results other than that from sporadic noise fitted ingeniously to ad hoc hypotheses? I think not.

Litovitz attempted to construct a physical basis for his conclusion that the eggs were sensitive to such very weak fields. In particular, he stated that Johnson-Nyquist noise describes only temporal, but not spatial fluctuations and that the fields he used are coherent over the whole cell membrane hence escape J-N noise limitations. I consider this wrong in general and in particular. In particular (as emphasized to me by Charlie Bean), the membrane with a resistivity of $r \approx 10^6$ Wm, electrically clamped internally by conducting cytoplasm and externally by conducting electrolyte, then has an RC time-constant of $e_0 r \approx 10^{-5}$ s, which insures that the J-N noise fields act uniformly over the whole membrane over times relevant to the experiment. Litovitz dismissed the unusual dose-response function that he supposedly observed as "typical of coherent effects" without further elucidation. Though physical properties of materials do change in a discontinuous manner as a function of state functions such as temperature when a phase change occurs, the relevance of such to threshold responses to magnetic fields is hardly transparent.

And my comment about the ad hoc nature of his explanations

was confirmed by Litovitz's explanation of why his results-cum-hypotheses did not fit the (negative) Finnish Henhouse results (which some critics found the only properly controlled experiment) though his model was arranged to fit the other experiments. The Finns hatched their own eggs from their own small colony of about 20 hens and 4 roosters—and then controlled the handling of the eggs with great care—while the other groups, and the CU group, bought their eggs commercially and could not control their early history. Litovitz says the Finns got the "wrong" results because they had *insufficient genetic diversity*!

Epidemiology

The techniques of epidemiology are outside of a physicist's natural area of competence, but the results—numbers, ratios, statistical significances—are not. Indeed, since physicists deal with much the same kind of data as epidemiologists in situations where, unlike epidemiology, the data is clearly falsifiable and errors are punished, physicists seem to have learned to deal better with the meaning of statistical significance than many scientists in fields such as epidemiology. Much of the epidemiological data that purports to demonstrate carcinogenic and leukogenic effects of weak electromagnetic fields, a physicist finds laughably inadequate. I discuss one such study that has had an impact on public policy.

An EPA draft report spends several pages discussing the work of Samuel Milham concerning the mortality statistics of Amateur Radio Licensees in California and Washington and concludes that "The study points to excess risks—especially for myloid leukemia—."(page 3-52). A table (page 3-51) lists causes of deaths and observed and expected deaths where the expected deaths are taken from American Experience Mortality rates for caucasian men corrected for age. A total of 33 categories is included (where there is some double counting). I list a few in Table 1.

Table 1. Data on a few of 33 mortality categories for radio amateurs*

Causes	Observed	Expected	Ratio(%)
All	**2485**	**3478.9**	**71**
All Cancer	741	837	89
Leukemia	89	72	123
Myloid	18	12.9	140
Acute	**15**	**8.5**	**176**
Chronic	3	3.5	86
Unspecified	0	0.9	0

*Only the categories in boldface are statistically significant.

Since the death rate for the subjects is much less than for the control population, the subject set is clearly quite different than the controls. Nevertheless, a count of 15 deaths from acute myloid leukemia among the subjects when 8.5 (better, 9.1 by allocation of the unspecified) were expected among the controls, judged the most negatively significant of 33 comparisons, is taken as supporting the view that weak EM fields are dangerous and national policy should be adjusted.

If any single measurement initiated electrophobia, it was the work of amateur epidemiologists Wertheimer and Leeper, who reported results that they interpreted as indicating that children in Denver who lived near local stepdown transformers were about three times as likely to contract childhood leukemia, a rare but often fatal disease, as children who lived a few houses down the string. Their distance classifications were subjective—and not done blind. Hence their data was subject to bias and their internal statistics showed clearly that their results were fatally flawed by those biases. Though later, similar, studies showed smaller effects—and none firmly excluded a null result—an acceptance of the set of results must lead to the oft-stated conclusion that most childhood leukemia today is caused by electromagnetic fields in the environment.

However, epidemiologist Philip Cole noted that if this were the case leukemia would be rare among populations subject to very little electromagnetic fields, and tested this result by comparing past and present childhood leukemia rates in Connecticut (which has the oldest cancer registry) with increases in power use in the US (an adequate surrogate for Connecticut use). The graphs of Fig. 1 show the number of childhood leukemia cases per 100,000 childhood years from 1940 to the 1980 along with the national power usage in megawatt-hours per capita. (The Connecticut power use variation cannot be easily extracted but should be almost identical to the national trend.)

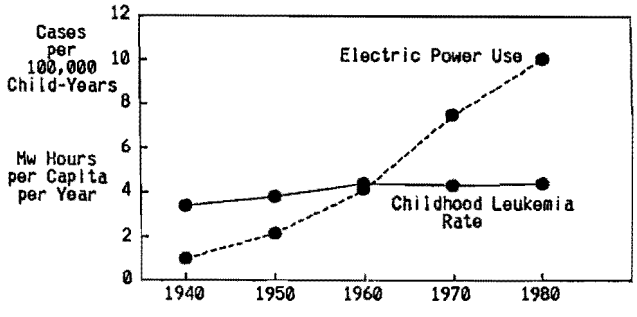

Figure 1. Cases of childhood leukemia per 100,000 childhood years in Connecticut, and power use in the US in megawatt-hours per capita from 1940 to 1980.

Early (1940) deficits of about 20% in leukemia cases are about that to be expected by diagnosis and reporting failures noted in the initial years of the survey. Since 1960, the reported leukemia rate is constant. But from 1940 to 1980, the electricity use increased by a factor of 12 and the usage increased by a factor of about 2.5 from 1960 to 1980. Hence, childhood leukemia is certainly not caused by electromagnetic fields in the environment—and the poor-quality studies that show otherwise are simply wrong.

FORUM ELECTIONS

We print, here, the backgrounds and statements of the candidates for this year's Forum elections.

Marc H. Ross (chair-elect)

Professor of physics, University of Michigan, and senior scientist in the Environmental Assessment and Information Sciences Division, Argonne National Laboratory. PhD University of Wisconsin, 1952. Co-directed the 1974 APS study of efficient energy use. His research concerns the technology, behavior, and economics of energy use. Current projects include: 1) a model to generate scenarios of industrial activity and energy use, 2) an analytical description of fuel use by automobiles, depending on elementary variables relating to the vehicle and driving, and 3) analyses of policies to improve environmental quality through new technology.

Statement: The Forum is doing a good job informing the physics community about socially important physics-related issues, through *Physics and Society*, invited sessions, special studies, and short courses. But there is more the Forum should be doing for the next generation.

Students and young physicists should, in choosing and directing their careers, consider what they might do with their special skills to help society at large. The Forum should work with other groups to help develop information on physics careers, such as statistical and anecdotal information on the social roles that physicists play, and contacts for further information.

The Forum should work with others to help stimulate long-overdue improvements in introductory college physics. Some individual physics teachers are doing a lot, but many of us at colleges and universities need to reconsider our goals and methods so that we address the needs and gain the interest of those who are not on the narrow track to a physics PhD. A part of the reform, and it can be a minor part, is to include discussion of the relevance of physics to other fields and to societal concerns.

The Forum should also encourage the physics community to reach out to society. The Forum has been relatively effective and needs to continue its work in the area of improving the understanding of modern weaponry. Another challenge offered to physicists was presented in the excellent September issue of *Physics Today* on school education. Some important projects are underway, but a satisfactory program of eduction in science and mathematics in the U.S. is a distant and difficult goal that more of us should address.

Finally, the Forum can help physicists consider new socially-important research areas. The environment is a major research area needing the physicists' kind of thinking and methods. The Forum should work with others to develop information on current environmental research problems that may challenge physicists' imagination.

Peter D. Zimmerman (chair-elect)

Senior Fellow for Arms Control and Verification, Center for Strategic and International Studies, and Research Professor of Engineering and Applied Science, George Washington University. Senior Associate, Carnegie Endowment for International Peace, 1986-89. Fellow at the Arms Control and Disarmament Agency and technical adviser at the START Negotiations, 1984-86. Professor of Physics, Louisiana State University, 1974-87. Steering committee, UN Commission on Arms Control Education. PhD, Stanford University. Forum service: secretary-treasurer 1983-87, chair of awards committee 1989, first chair of committee on APS fellowships 1982, organized several invited sessions at APS meetings, member of Forum ICBM study and of APS Committee on Education. Current research interests include use of Landsat satellite imagery to detect concealed underground nuclear tests, directing CSIS study on global monitoring and verification for the 1990s, director of US/German/Indian study of ways to manage the proliferation of guided missiles. Performed satellite survey of Kuwait oil well fires for the Kuwait Petroleum fire-fighting task force.

Statement: Participation in Forum activities opened many new doors for me, and led directly to the opportunity to serve at ACDA. The Forum is probably responsible for my career making a 90 degree change from medium-energy nuclear experiments to its new focus on the study of issues at the interface of physics and public policy. Such a reorientation is not the goal of most Forum members, but the Forum, through the studies it has organized, has given many physicists the chance to participate in nationally recognized research on arms control and on energy and environmental policy. The Forum must continue to provide a way for members outside Washington and not connected to the major centers of policy research to meaningfully contribute to the national debate and to receive appropriate recognition at their home institutions.

As Chair-Elect and Chair I would seek to start at least one new study each year, trying each time to widen the participation to new investigators.

As the Cold War fades, some of the Forum's traditional focus on strategic arms control should shift to other concerns such as environmental policy, American competitiveness and high-technology manufacturing, scientific fraud and ways to handle it, and multi-lateral arms control initiatives designed to establish an "orderly new world" rather than a New World Order based primarily on American military dominance. These areas, not yet the subjects of Forum studies, provide a rich field of research where physicists have much to contribute.

Anthony V. Nero, Jr (vice-chair)

Senior Scientist in the Indoor Environment Program, Lawrence Berkeley Laboratory. Research activities have included health effects of energy technologies, forms of nuclear power, and indoor pollutants, especially radon. PhD, physics, Stanford. Spent a year in the non-proliferation bureau of the US Arms Control and Disarmament Agency. APS Fellow. Served on POPA as chair of the the subcommittee on studies, and on the Forum's executive committee, as well as on its awards committee and fellowship committee, and has periodically arranged APS symposia. Consultant to various national and international scientific bodies, author of *A Guidebook to Nuclear Reactors*, co-edited *Radon and its Decay Products in Indoor Air*, and written numerous articles for such periodicals as *Science*, *Technology Review*, and *Scientific American*. Received the 1989 Leo Szilard Award for his work on radon, indoor air pollution, and nuclear power.

Statement: The physics community, through its research, makes

a fundamental and long-term contribution to the future of our society and has a corresponding capability and responsibility for considering important technological issues and for influencing associated societal choices. During recent decades, physicists have taken the lead on important questions in arms control, energy, and the environment, with the Forum making essential contributions by raising issues, airing them in APS sessions and in a now-expanded newsletter, exploring them in groups organized around certain topics (often leading to publications), and encouraging the APS to examine them. These successes illustrate how the joint efforts of capable and dedicated scientists can yield substantial results and influence.

Given this experience, I believe that the Forum should press forward on several fronts. First, as a result of recent events relating to the USSR and Iraq, new questions and perspectives have arisen in arms control and energy and the environment, some deserving (and amenable to) examination by Forum efforts. Secondly, I believe this Forum ought to cooperate with the new Forum on Education to examine the degree to which energy and environmental topics might be used in precollege curricula to stimulate interest in science and mathematics, which might in turn lead to more critical and effective public examination of societal issues. Finally, I suggest that we in the Forum ought to examine to a greater degree, not only well-defined technical topics, but the underlying question of how to develop conceptual frameworks for making effective decisions on difficult technological issues that ultimately affect the way we, our children, and our children's children will live.

David Bodansky (vice-chair)

Professor of Physics, University of Washington. PhD, Harvard, 1950. Research interests: experimental nuclear physics, nuclear astrophysics, and energy and environmental policy. Co-author of *The Energy Controversy: The Fight Over Nuclear Power* (1976) and *Indoor Radon and Its Hazards* (1987). Fellow of APS and AAAS, member of POPA 1985-87, participant in Forum energy study.

Statement: The Forum provides a bridge between the normal physics activities of APS members and their interests in societal problems. It has used a number of mechanisms for this, including the newsletter, invited APS sessions, symposia and short courses, and studies and publications. These activities should be maintained and expanded. They have two main audiences—the APS membership and the public. The Forum should provide information to each. Overall, the Forum has done a good job and should try to continue to progress along the same general lines.

Specific future Forum projects will be determined primarily by the enthusiasms of the membership, but Forum officers can be helpful in lending support. In addition, they can take the lead in fostering activities in areas which they believe to be important and of general concern. The rotation of Forum officers should make it possible to have a wide spectrum of interests represented over a short period of time.

My own interests lie primarily in energy and related environmental problems. I believe that the preparation of the Forum's *Energy Sourcebook* was an excellent step in this area. However, it can be viewed more as a first step than a completed program. The understanding of virtually all of the topics treated in this volume is undergoing continual development, for example, the potential of conservation and the prospects for utilization of nuclear fission, biomass, and solar photovoltaics. Periodic updates and extensions of the material would be useful, either through revision of the entire volume or through further publications on individual subparts. These and other energy topics should continue to be addressed through workshops, APS sessions, and Forum-sponsored publications. Of course, interests in one area, in this case energy, should not preclude attention to other important issues.

There are clear overlaps between the concerns of the Forum and POPA and, in some areas, between those of POPA and the AAPT. The Forum should continue to explore possibilities for cooperative efforts with these and other groups with common interests.

Caroline L. Herzenberg (secretary-treasurer)

Physicist at Argonne National Laboratory. PhD, physics, University of Chicago. Taught at Illinois Institute of Technology, the University of Illinois at the Medical Center, and California Sate University at Fresno, and has worked as a research scientist at ITT Research Institute. Fellow of the APS and the AAAS. Chaired the APS Committee on the Status of Women in Physics. Member of Forum for over 10 years, has served on the nominations committee. She has organized and chaired a variety of sessions dealing with science and society issues at both APS and AAAS meetings.

Statement: I have spent a number of years serving variously as an officer and member of the executive committee of other scientific organization, both local and national, and I believe that I have suitable experience for conducting the work of secretary-treasurer. I feel a strong commitment to the goals of Forum, and would be glad to take this opportunity to help to contribute toward advancing Forum's activities.

Alvin M. Saperstein (secretary-treasurer)

Professor of physics, member and former chair of Executive Board of Center for Peace and Conflict Studies, Wayne State University. Initiator and director of co-major Program in Environmental Studies. Published research in theory of land warfare, non-provocative defense, arms control, chaos and theoretical models of international relations. Fellow of APS and AAAS. Teaching, new courses, and publications in elementary and advanced physics, science and public policy, energy and environment, international security and technology. Presented professional, public, radio and TV programs. Current interests: Chaos theory and its implications for science and public policy; interrelations between science, technology, and public policy as related to international security/arms control and energy/environment issues; revamping and rejuvenating the teaching of elementary physics, its applications and implications to science and non-science students.

Statement: I bring the experiences of the past 28 years working primarily within the context of a large, multifaceted, midwestern, urban university, facing many issues from a perspective different form that of many of those currently on the committee. Yet, I am not parochial, having a solid international perspective via having lived and worked (research and teaching) for extended periods abroad. Here and abroad, I have dealt with local, state, and federal governments, inner-city and suburban schools—instituions, children, staff, and issues—as well as with the "usual" university research, service and teaching issues.

As a long-term Forum member who previously served on its executive committee, I am familiar and sympathetic with its "ideology." I believe this to be the provision of a non-ideological Forum wherein the physical sciences' impact upon societal problems can be demarcated and illuminated, to the benefit of both the general society and the physics community. I believe our role is to teach (often requiring us to learn), not to proselytize, formally and informally, to all ages and segments of our society. In this most

"degreed" society in history, the level of common knowledge of science—both for its own sake and for its application to the common well-being—is appalling. Hence I suggest that the Forum, while continuing and expanding its publications, special studies, and APS sessions, all of which are basically "adult education," should consider close cooperation with those elements of the AIP family which are seeking to improve school physics education.

My value as a potential secretary-treasurer should be enhanced by my service as the treasurer of the founding board of UCAM (and a participating member of many similar organizations) as well as terms as treasurer and president of a large residential cooperative corporation. These experience have made me familiar with the creation, presentation, and adminstration of large and small budgets for both "pragmatic" and "ideological" organizations.

Peter Lindenfeld (executive committee)

Professor of Physics, Rutgers University. PhD Columbia University. Guest Scientist, University of Paris-Sud (Orsay) 1970-71, Kyoto University, 1982. Developed and taught courses "Physics and Public Issues, Science and Social Science of Energy," and numerous courses for high school teachers. Numerous publications on superconductivity and related areas, "Radioactive Radiations and their Biological Effects" (AAPT prize for module-booklet), "Introduction to nuclear fusion power and the design of fusion reactors" (AAPT, with J.A. Fillo), "Measurement of optical absorption by calorimetry and analysis of a solar collector" (AAPT apparatus prize and paper in Am. J. of Physics), Warren I. Susman Prize for Excellence in Teaching, Millikan Medal of the AAPT.

Statement: Education has always been a major item on the Forum's agenda, and this should remain so even as a second Forum (on Education) begins its existence. Continued strong advocacy for education which emphasizes societal concerns will be particularly important. There is a need not only for specialized courses, but also for reform of the introductory physics courses, which have tended to become more and more abstract and remote from the real world. More generally, in spite of widespread interest by students and faculty there is normally no place whatsoever in our pattern of scientific education for the examination and analysis of public issues. I believe that the Forum can play a vital role in this area through the development and listing of appropriate materials for courses, colloquia and workshops.

Arthur Charo (executive committee)

Senior Analyst in the International Security Program, US Office of Technology Assessment (OTA). AIP Congressional Science Fellow, 1988-89. Member, AIP Committee on Public Affairs, and selection committee for AIP Congressional Science Fellowships. PhD, Duke University, 1981. Research in high-resolution spectroscopy of transient molecular species and weakly bound molecular complexes using molecular beam techniques. MacArthur Foundation Fellowship in International Security, 1985. Fellow, Center for Science and International Affairs at Harvard University, 1985-1988 working on technical aspects of strategic defense. Author of *Continental Air Defense: A Neglected Aspect of Strategic Defense.* Worked on technical and policy aspects of arms control and the civilian and military uses of space at OTA. His OTA publications include an analysis of the National Aero-Space Plane program, technologies for monitoring compliance with the START Treaty, and methods to monitor limits on sea-launched cruise missiles. Currently working on remote sensing technologies and options for the proposed Earth Observing System program.

Statement: The Forum exists for two reasons. First, it allows APS members to share views on issues in science, technology, and public policy. The Forum does this by organizing APS invited sessions and through *Physics and Society*. Second, it contributes to the public debate on controversial issues that have a significant technical component. Recently, for example, the Forum sponsored studies on the future of land-based strategic missiles and on US energy policy.

The Forum is a unique resource. Its members are mostly physicists, its officers are volunteers, and its publications are not perceived as partisan. None of the other professional associations that work at the interface of science, technology, and public policy combine these attributes. The Forum can therefore be an effective voice for physicists who wish to inform a public debate through objective, technical analysis. Unfortunately, the Forum currently has little visibility, even within the physics community.

The effectiveness of the Forum could be increased by soliciting invited talks and journal articles by persons who may not be APS members and by publishing more of the Forum's work in *Physics Today*. The Forum should also coordinate more of its activities with the APS Office of Public Affairs, which has been particularly effective in conveying the thoughts of APS members to the press and to Congress on topics ranging from the wisdom of building Space Station Freedom to the appropriate government response to claims of cold fusion. To broaden its audience, the Forum should also seek to jointly sponsor colloquia and Congressional seminars with non-partisan organizations such as the AAAS and the National Academy of Sciences.

Lisbeth Gronlund (executive committee)

Social Science Research Council-MacArthur Foundation Fellow in International Peace and Security, and senior visiting scholar at the Center for International Security Studies, University of Maryland. Post-doctoral fellow, program for defense and arms control studies at MIT, 1988-90. Research interests include restrictions on further qualitative developments in strategic weaponry, particularly short time-of-flight nuclear weapons; confidence-building and transparency measures for military R&D; and ballistic missile proliferation in the developing world. PhD, Cornell University, 1989. For the past three years, helped organize international meetings that have brought together young scientists working on security, energy, and environmental issues from the US, USSR, China, Europe, and developing countries. Served as a graduate student on the executive committee of United Campuses to Prevent Nuclear War and was actively involved with Cornell's chapter. Co-organized the national SDI pledge, which resulted in several thousand university scientists and engineers pledging not to accept SDI research funding.

Statement: I see the role of the Forum as three-fold: 1) to educate the public and policy-makers about technical aspects of public policy issues; 2) to educate the APS membership about public policy issues and, more broadly, to facilitate an ongoing dialogue about the responsibilities of scientists and their role in society; and 3) to encourage and enable APS members to become actively involved in addressing science and society issues.

All of these roles could be enhanced if the membership of the Forum were increased—and I would want to work on doing this, perhaps through targeted mailings and solicitation at meetings. Increased membership would also provide increased revenue, which would give the Forum greater flexibility in organizing sessions at the APS meetings since money would be available to pay travel costs and, if appropriate, honoraria for invited speakers. In gen-

eral, it would be useful for the Forum to sponsor more sessions intended for a general physics audience at APS meetings other than the Spring Meeting.

Jill Wittels (executive committee)

Vice President of Engineering, Loral Infrared and Imaging Systems; AIP Corporate Associate; former APS Congressional Fellow; former Senior Visiting Scientist to the Soviet Academy of Sciences. Served on APS committees, including the Forum, Status of Women in Physics, Fellows Selection, and committee to evaluate the Congressional Fellows program.

Statement: In the course of serving on several APS Committees I have found that there is considerable room for expanding the perspectives and the effectiveness of APS through more involvement of physicists from industry. For example, in the activities of the Forum in arms control and military technology there is too rarely information and understanding from the industrial developers of weapons systems and technology. As an increasing number of physicists find employment in industry rather than an academic or research laboratory setting, it is past due to increase the use of this resource in APS activities.

While spending time as a visiting scientist in the USSR in the late 1970s, I developed a keen interest in the international transfer of technology, which I carried into service as a Congressional Fellow. Although my involvement in policy issues became indirect after I moved into the industrial complex, I have continued to follow these issues and work on them through APS where possible. I would welcome the opportunity at this time to renew more active participation through the Forum's sponsorship activities and to bring to APS some of the industrial perspective.

NEWS

Forum Councillor's Report

Dear Forum Friends:

It has been my pleasure to serve you on the APS Council these past four years. I am much heartened by the willingness of you physicists to serve the APS and the Forum, and all for free. I feel most blessed just to know you all. The APS-Forum is a unique group that has made many useful contributions. I look upon the Forum as the thin-blue-line, watching for applications of physics to society here, calculating an energy effect there, using logic on stable nuclear force structures over there, hopefully softening the more harmful edges of the blade of physical laws. With the winding down of the nuclear arms race, the nation and the Forum will have more time to examine the other issues. Let's get on with it! The Forum is the second largest (4,217) of the APS Divisions and Forums, second to the Division of Condensed Matter Physics (5,409). When the International Physics Group becomes a Forum, we will then be third.

The toughest decision over the four years was the vote last week to move to College Park. I am all in favor of the move, but the sticker price ended up being more than I had expected. On balance, it should be a good thing. I voted yes (but not for the extra funds), in spite of the price. I suppose the extra cost amounts to $10 per member per year, going into equity for sure. For our kinds of issues, there are great benefits in being near DC. And having the American Center of Physics close by could help us (and the other divisions) get more organized and more active.

The APS has passed a couple of resolutions to warn of the high price of the Space Station, and to warn of the loss to little-physics funding. On the other hand, the Panel on Public Affairs (POPA) has not had a study since the Directed Energy Weapons Study finished in 1987. It is my strong feeling that POPA will have to take on smaller projects, run hearings in DC to get data, exist on modest funds, but produce good products. I think it can be done. There has been too much caution about accepting topics that are not very close to pure physics, and I think that POPA will have to grapple with some less pure topics.

Our Forum and Szilard winners this year (Kurt Gottfied and a Brazil Physical Society/Argentine Physical Society stop the bomb group) make us proud to be physicists, reaching out to fix what can be done. As you may know there is some pressure to endow the awards, but this will be difficult for the Forum because we have no allied industry to do that for us. I hope that our beautiful, symbolic statues will be acceptable to those who look for dollars in the bank. (On the other hand, if you want to put the awards in your will, we will accept that.)

The very good news is that I shall be more than replaced by one Dr. Barbara Levi, Ph.D. Stanford about 1970, formerly of Georgia Tech, Rutgers and Princeton, and now Senior Associate Editor of Physics Today. Barbara understands the complex physics and society issues, is very open-minded and fair, works hard, calculates and writes par excellance, and is a most wonderful person. Off into the sunset we go. Fondly to you, the Forum.

David Hafemeister

DeVolpi Punished for Attending Disarmament Meeting

From *Science*, 8 November 1991, p. 787: "The Department of Energy (DOE) has punished an Argonne National Laboratory physicist for participating in a non-governmental workshop on nuclear weapons disarmament. Through an intermediary, officials at DOE's Office of Arms Control (OAC) informed Alex DeVolpi, a specialist on arms control verification, that he has been barred from attending future DOE-sponsored arms control meetings and from authoring proposals to be circulated at these meetings, according to both a workshop participant and an Argonne official. DOE denies that DeVolpi is being blackballed.

"DeVolpi and two retired weapons experts from Lawrence Livermore National Laboratory (LLNL) attended an 18-19 October workshop on warhead storage and dismantlement sponsored by the Federation of American Scientists (FAS) and the Natural Resources Defense Council. Other weapons lab personnel at Livermore and Los Alamos National Laboratory had planned to attend, but OAC officials pressured them to pass up the workshop because it 'might be counterproductive to the president's arms control initiatives'."

From *Science*, 18 October 1991, p. 365: "The fact that the cold war is over doesn't seem to have registered with DOE officials, who have pressured scientitsts at LLNL to pass up a workshop on nuclear disarmament. Workshop organizers had invited three Livermore weapons researchers to participate. But the OAC told them that the agency would not pay their expenses to attend,

although it frequently does so for other conferences, says invitee Ray Kidder, a retired LLNL researcher who still works at the lab part-time.

"DOE officials defend the policy by claiming that the non-governmental workshop is an 'inappropriate' forum for DOE scientists to discuss nuclear weapons policy. OAC director Anthony Czajowski said that lab personnel wanting to 'do their own thing' has been a continuing problem for DOE. But Czajowski virtually admitted that politics might have played a role in the decision when he added: 'The workshop might be counterproductive to the president's arms control initiatives.'

"Workshop organizers are outraged. 'I don't think the current DOE administration understands that these guys aren't junior Navy officers who are supposed to do only what they're told,' says Frank Von Hippel, head of research at FAS."

Alex DeVolpi is a newly-elected Fellow of the APS, sponsored by the Forum. He has been an active Forum member for many years. He is also currently a member of the national council of the FAS.

Fellowships in International Security!

Stanford University's Center for International Security and Arms Control announces 1-year fellowships designed to provide experienced scientists and engineers an opportunity for study of technical aspects of problems related to international security, defense policy and planning, and arms control starting fall 1992. Fellows will have the opportunity to collaborate with social scientists and with visiting scholars, diplomats, politicians, and military personnel from the United States and other countries. Application deadline is February 14, 1992. For additional information contact David Bernstein, 320 Galvez Street, Stanford, California 94305-6165.

Book Review Editor Needed!

Do you like good books? At the physics-society interface? *Physics and Society* should be bringing more of them to Forum members' attention. We need a book review editor to suggest books and articles for review, to seek out appropriate reviewers, and to edit the reviews. If you are interested, please contact Art Hobson, Department of Physics, University of Arkansas, Fayetteville, AR 72701, phone 501-575-5918, fax 501-575-4580, bitnet AHOBSON@UAFSYSB. Lots of psychic income!

Join the Forum! Receive *Physics and Society*!

Physics and Society, the quarterly of the Forum on Physics and Society, a division of the American Physical Society, is distributed free to Forum members and libraries. Nonmembers may receive it by writing to the editor; voluntary contributions of $10 per year are most welcome, payable to the APS/Forum. We hope that libraries will archive *Physics and Society* ; Forum members should request that their libraries do this.

APS members can join the Forum and receive *Physics and Society* by mailing the following information to the editor or to the APS office:

I am an APS member who wishes to join the Forum:

NAME (print) _____

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Commentary

Editorial: Ozone

The ozone treaty of 1987 and 1990 was a triumph for the atmosphere and for rationality. Yet the ozone itself is in bad shape. Could we have acted sooner?

Chlorofluorocarbons (CFCs) were first synthesized in 1930. They were inert, non-toxic, non-corrosive, non-flammable, inexpensive, and they liquified under pressure. Such a chemical had many uses: universal coolant, spray propellant, foaming agent, and solvent. CFCs created the air-conditioning revolution that facilitated America's great shopping cathedrals, automobiling in cool comfort, and the southwestern population shift.

The CFC bubbles began bursting in 1973-74. Richard Stolarski and Ralph Cicerone were studying rocket emissions. They theorized that chlorine releases could destroy stratospheric ozone catalytically. Chemists Mario Molina and Sherwood Rowland wondered, quite independently of Stolarski and Cicerone's work, where all the CFCs go once they are released to the atmosphere. Where is the sink? It seems an obvious question, now, but nobody seriously asked it before 1974. They theorized that CFCs are broken down by ultraviolet radiation in the stratosphere, releasing chlorine.

The implications of the two hypotheses alarmed people. A classic environmental debate raged during 1974-78. The chemical industry argued that ozone destruction was only a theory, that CFCs should be presumed innocent until proven guilty. Environmentalists counseled erring on the side of safety and risking no further delays. Consumer pressures against CFCs played a role. In 1978, the US led a few other countries in announcing a ban on CFC spray cans. It was the first time a substance suspected of causing global harm had been regulated before the effects had been fully demonstrated.

The ban caused a temporary leveling of emissions. It would have been difficult to battle the larger problems, coolants and foams. Most Americans forgot about CFCs for eight years.

Fortunately, a British team had been routinely observing the Antarctic atmosphere since 1950. In 1977, they began observing a new trend: a temporary springtime decline in ozone concentrations. The decline deepened and broadened each year. So unbelievable were these measurements that the team delayed publication until 1985. Their report was greeted skeptically. In 1986, atmospheric chemist Susan Solomon organized an expedition that confirmed the "ozone hole" was real, and growing. The observations were entirely unpredicted. Any effects had been expected to be gradual, and worldwide. The following year, over 150 individuals representing 19 organizations and 4 countries organized the Air-

borne Antarctic Ozone Expedition.

The new data led to a theoretical understanding of the ozone hole. The key was an atmospheric subtlety, tiny ice crystals that form high above Antarctica during the polar winter. These provide surfaces on which solar-assisted reactions involving chlorine occur when sunlight reappears.

Meanwhile, as early as 1975 the UN Environmental Program began sponsoring research conferences that eventually led to the signing of the Ozone Treaty in September of 1987. The treaty called for a CFC production freeze, and 50% reductions by 1998. The 1990 London revisions called for 50% reductions by 1995 and phaseouts by 2000.

The Ozone Treaty was a giant step. In 1987, the ozone hole was not yet understood. Worldwide ozone depletion had not yet been observed. It was a treaty based on a scientific theory. And the treaty called not just for discussion or research, but for real cuts in a multi-billion-dollar per year industry. The US was among the nations leading the breakthrough. (Do read US negotiator Richard Benedick's *Ozone Diplomacy*, reviewed in these pages in October 1991.)

Although more can still be done, the verdict is now largely back in nature's hands. Her recent reports have not been good. The effects will get worse before they get better, with long-term effects for at least a century. Couldn't we have learned sooner, say during 1930-74? Could we have taken stronger action sooner, during 1974-87, when we did know the possibilities? We made an unconscious decision to accept the air-conditioning, and the risk. It was a bad bet.

As we enter the greenhouse century, what can we learn from ozone? We must look back, not to criticize the past but to live the future.

Who was minding the store during 1930-74? What was the ratio of "interested" to "disinterested" scientific study of CFCs? The next time you see a shelf full of the Physical Review, ask how does it happen that scientists can fill so many pages with such exquisite detail, yet for 44 years miss the fact that CFCs just might destroy the ozone that makes life (and research) possible? It was lucky for us that the British monitored Antarctic ozone levels, that Stolarski and Cicerone studied ozone, that Molina and Rowland studied chlorine compounds, that Solomon organized the 1986 expedition, and that the UN was available and interested. Any of these could have gone differently. What if they had?

What if there had been no public concern during 1974-78? What if the argument that CFCs should be presumed innocent until proven guilty had prevailed? Why did public debate subside after 1978? Why don't non-scientists know more, and express more concern, about these matters? Who is educating people about ozone? Are we physicists educating people about such matters?

On ozone, humankind acted more rationally than it has acted on perhaps any environmental issue. And we have been lucky—lucky to have had the work of Molina, Solomon, and others. Yet the atmosphere has suffered an enormous blow.

Atmospheric carbon dioxide is now 25% above its highest point in 150,000 years. A tenuous trace of CO2 and water vapor is crucial in controlling the planetary balance of radiant energy. Predictions are that CO2 doubling will occur around 2030. A 50-80% worldwide reduction in carbon emissions must occur during the next few decades, if greenhouse gases are to be restrained from rising above their present high level. But most of the world is on the verge of rapid industrialization, while along the highways and in the shopping malls of America, business continues as usual. We scientists mostly ignore the issue, both in the laboratory and in the classroom.

On ozone, despite unprecedented rationality and luck, we lost our bet with nature. We are now nearly 250 irrational years into fossil-fueled industrialization, and the temperature is rising. We had better hope for a lot of luck.

Art Hobson