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LETTERS

High Quality Arms Control Post-Docs

This summer I had the nice opportunity to meet several of the current crop of young PhDs who have chosen to study and work on technical issues of nuclear arms control. I was impressed with their no-nonsense physics approaches to these complicated issues. A group of eight of these arms control post-docs will be joining other young post-docs from the USSR, UK, and China in Moscow this September for an international summer school on technical issues on arms control. This initiative, arranged by Frank von Hippel, Roald Sagdeev, and others, is trying to nurture the next generation of scientists who will dedicate part of their professional careers for paid or "pro-bono" work on arms control. I am hopeful that these eight high-quality physicists will be able to land jobs that can use their training.

After thinking a little about this issue, I drew up a list of physicists doing arms control work in the universities, in order to gain some perspective on their chances in academia. I arbitrarily defined an active physicist doing arms control in a university as one who has: (1) spent at least 1/4 time doing arms control, (2) published at least one technical paper on arms in the past 5 years, and (3) had a permanent or semi-permanent position in a university. My list has 34 names, 17 in physics departments, 3 in other departments, and 14 in centers, schools, or programs that do broader, public policy issues. This list is actually an upper bound since it has 4 retired members, and the PhDs of some members are in nuclear engineering, electrical engineering, chemistry, and energy studies.

Some observations: The names on the non-departmental list tended to be more well known, because they are very good and because they spend nearly all their time on arms control. Half the physics department members have served at least one year in government, and have been active in arms control since returning to their university. Only four physics department members are from the nominal "top ten" departments.

The average age is about 50, and only 1 or 2 seem to be less than 40. This is not surprising. Assistant professors in prestigious PhD institutions had better stick to their professional knitting if they are to get tenure. None of the 17 names on the physics departmental list had begun their careers with arms control physics as the main pole in their tent. In fact, all 17 had moved towards arms control after becoming full professors. I am under the strong impression that these departments are all glad to have one member who is active in teaching, publishing, and serving on arms control issues, but that they have not considered hiring a young assistant professor to do that kind of work.

My conclusions, which I address to US physics departments:

1. I am concerned that the present group of serious arms control professors in physics departments will not be replaced. Every department needs one to keep your university up to date.

2. When filling that tenure track position this year, take a look at the current crop of post-docs doing arms control. They come from particle physics, plasma physics, etc., they are capable physics citizens, and I bet they can do both physics and arms control physics in your department.

3. If you are a tenured full professor, and it seems that your career is on hold, please examine some of the societal applications of physics (arms, energy, environment) to see if you can get a second wind.

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Should Ethics Limit Scientific Research?

Liebe F. Cavalieri's article (April 1989) is an all-out attack on science, on technology, on material progress, on man. It deserves to be identified as such and condemned. While P. Roger Gillette (July 1989) is clearly bothered by that article's conclusions, I consider his response unsatisfactory.

Cavalieri exhorts us to guard science "against abuse and exploitation for commercial purposes that have little to do with either human needs or the acquisition of pure knowledge." He ignores that commercial enterprises satisfy human needs, that businesses earn profits by producing the things their customers value. He defends "pure knowledge" or "knowledge pursued for its own sake," catch phrases intended to refer to knowledge disconnected from and irrelevant to reality and human life. It is hard to imagine what such knowledge would consist of; what the advocates of these phrases mean is that knowledge should not be "corrupted" by its application to solving real-life, human problems.

Cavalieri attacks recombinant DNA technology essentially because it is a powerful tool. It has potential "applications to medicine, agriculture, and industry; its possible influence on ecological systems and future generations of humans is incalculable." That much is true; that is why I endorse aggressively *pursuing* recombinant DNA, not turning away from it. I hope that it leads to a cure for or the prevention of cancer, to increased food production, to advances in industry. Wouldn't these help satisfy human needs?

Cavalieri wants to increase the already-burdensome regulation of scientific research and technological innovation. Presumably ideas that are not compatible with his view of man would be banned. What else can be the meaning of his complaint that "....cries by scientists for freedom of inquiry seem banal, self-serving, and irrelevant?" And what is his view of man? Cavalieri tells us that we should have "labor-intensive instead of energy-intensive production," that we should seek a state without "a surfeit of material goods."

Cavalieri's argument that we must restrict technology because of the limits of the environment is a self-fulfilling prophecy. With his constraints on technology, our ability to support growth is indeed limited. But with technological progress, we can support a rising standard of living for a growing population.

In short, Cavalieri seeks to push man back to the middle ages, to a state where production is labor intensive, where there are few material goods to sustain and enhance life, where progress does not exist, where death and disease are rampant (but where nature is undisturbed). To accomplish this, he seeks a larger and oppressive role for government, which must ultimately result in a totalitarian state.

Gillette is correct in noting the connection between science and the satisfaction of "human needs and desires." His statement "to hinder science and technology is to limit the ability of our species to be truly human" begins to get to the essence of Cavalieri's vicious attack. But then Gilette tells us that "science and technology must be used for the *good of the planet as a whole*" (emphasis added). It is not clear what Gillette means here, but it certainly is not the satisfaction of human needs and desires.

Gillette accepts government regulation of technology, and he endorses a pragmatic cost-benefit approach (which Cavalieri rejects as too moderate). How these goods and evils are to be weighed is unclear. How, for instance, should a caveman have acted when he discovered how to sharpen a stone? Should he have been pleased that he could hunt more easily, or should he have turned away from his knowledge since "living beings" would be harmed?

While Gillette takes a more moderate position than Cavalieri, ultimately both agree on a fundamental point:: that morality is impractical, that ethics demands that man turn away from science and the rewards it offers.

Man needs a code of ethics. But, just as poison cannot be substituted for food, a false code of ethics cannot take the place of a proper, rational code. A proper morality *is* practical.

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Response :

Bennett Karp considers my response to Cavalieri's article unsatisfactory. So do I. No response on such a complex subject that would be publishable in Physics and Society could be fully satisfactory. Publishable statements will inevitably be oversimplified.

For example, my assertion that "science and technology must be used for the good of the planet as a whole" is incomplete. It doesn't specifically say that the good of each species of life and non-living matter on the planet must be defined, and the goods of each must be appropriately weighted to determine the good of the planet as a whole. I haven't done this; I don't think anyone has done it properly. However, the satisfaction of human needs and desires, appropriately defined, will be an important part of achieving the good of the planet.

The primary raison d'etre of government is regulation to further the welfare of the whole and of individuals. How goods are to be weighed against evils, whether for individual beings, individual species, or the planet as a whole, is another question for which no adequate answer yet exists. However, government regulation should be based on a consensus regarding the relative values of the various species and states of being in the global system. This consensus should be based on an understanding of the nature of the beings, and of the processes that can affect their existence, and a consensus regarding the ultimate meaning of existence.

Thus good government should be based on good moral principles, which in turn must be based on good information regarding what exists and what is possible, and on good intentions regarding the achievement of ultimate goals and purposes. Good results do not come from either good information alone or good intentions alone. I certainly do not believe morality is impractical or should be sidestepped. Ethical behavior is more complex than most people want to believe, and must be based on an understanding of the material world and of the transcendent purposes that direct its evolution and development.

Members of the human species differ from those of other species most significantly in that they possess greater freedom of thought, will, and action, and must therefore be guided by a code of ethics. They are capable of developing a code of ethics, and over the centuries have been doing so. To be effective, this code must be both idealistic and realistic. Without the ideal, there is no direction for movement; without the real, there is no movement in that direction.

P. Roger Gillette

Photovoltaics in our Energy Future

H. M. Hubbard and Gary Cook (July 1989) failed to mention the 1970s idea of using satellites to collect solar energy to beam down to earth, either as photovoltaically generated microwaves, or as reflected beams enhancing the solar energy received by collectors on earth.

This omission is presumably due to the continuing high cost of low-exhaust-velocity chemical rockets. Electric rockets, using for example solar-powered ion engines, should not have this problem, but their acceleration is very low and they cannot launch ground vehicles into space directly. A way of partially overcoming this limitation might be to send out such a rocket some distance into space and use it to accelerate a small mass to a very high velocity toward earth. This mass is made to home in on and (after separating from the reusable rocket) to impact a vehicle which has been lifted (e.g. by a relatively slow air-breathing ram-jet) to a point just above the atmosphere. The resulting rapid acceleration of this vehicle into orbit can be softened by spreading out the impact mass. These maneuvers are sure to involve many technical problems, but these should be no worse than the similar problems faced by even the most optimistic boost-phase ballistic-missile defense system.

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U. Cal, Weapons Labs, and Arms Control

Deborah Blum's article "Public Perspectives" (April, 1989) states "there is no working x-ray laser." Actually, two years ago a working x-ray laser driven by *another* laser was discussed in *Physical Review Letters*. I presume you meant to say "there is no working x-ray laser *driven by a nuclear bomb.*" Your statement then might be correct. Now it is just false.

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Response:

I did not give a generic talk on x-ray lasers. My entire talk was devoted to the Strategic Defense Initiative and efforts at Lawrence Livermore National Laboratory to develop suitable weaponry, including a nuclear-bomb-pumped x-ray laser. In that context, I think it is perfectly clear that "x-ray laser" refers only to that military device.

Deborah Blum

The Meaning of Quantum Theory

Holy smoke! Whatever gave you (editorial, July 1989) the notion that I don't think quantum mechanics is weird? I won a prize a few years ago for an article in *Discover* titled "Quantum Weirdness" (reprinted in my *Order and Suprise)*, and I have written in half a dozen places about the mystery of the EPR (which suggests an interconnectedness on a superluminal level) in half a dozen places. I agree with Feynman that QM is crazy, and I certainly regard it as a much more fundamental break with classical physics than relativity theory.

You have totally missed the point of my editorial (*American Journal of Physics*, March 1989, p. 203). It is that quantum weirdness does not justify a leap to the views of Wheeler and Wigner that the reality and mathematical structure of the external world is mind-dependent. The Schroedinger equation, as you know, changes in a completely deterministic way. It is only when measurement occurs that chance enters the picture, but it does not follow from this fact that the external world does not exist and have a structure independent of observation. It is *this* metaphysical solipsism my editorial attacked, and I would guess that 99 percent of working physicists agree with me. I even received a letter from Glashow saying he couldn't comprehend how anyone could find fault with my editorial, just as I have nothing in *your* comment to oppose.

Please, don't accuse me again of views I don't hold!

Martin Gardner
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Response:

The quotation marks around "not odd at all" in reference to Gardner's article were meant to indicate the article's general attitude toward quantum theory, rather than an actual quotation from the article. The quotation marks were thus misleading, and I apologize for that.

On the other hand, the drift that I get from carefully re-reading this particular essay of Gardner's is still that quantum theory is not odd at all. Maybe I am reading too much into such statements as (and here I do quote) "Quantum mechanics raises not a single fresh metaphysical problem." At any rate, I thank Gardner for the above clarification of his views.

Art Hobson

ARTICLES

Szilard Award Lecture: From Reactors to Radon - Toward a New Environmental Consciousness

Anthony W. Nero, Jr.

[Editor's note: The author is winner of the 1989 Leo Szilard Award for physics in the public interest. His citation was published in the April issue. The following paper is based on his paper at the Forum Awards session held 2 May 1989 at the Baltimore APS meeting. Anthony Nero, Jr., is at the Lawrence Berkeley Lab, 1 Cyclotron Blvd., Berkeley, CA 94720.]

Receiving the Leo Szilard Award is a great honor, and a great pleasure. For this I thank the Forum on Physics and Society and the American Physical Society. I am pleased that the APS recognizes the importance of work on such problems as arms control, energy, and the environment. The involvement of APS members in societal matters is natural in view of the social implications of science and technology.

For example, the APS undertook studies in the mid-1970's on such energy related topics as nuclear power and other means of producing energy. The major classes of energy production discussed then--using fossil fuels, nuclear reactions, and the sun—are the same major categories that we hear so much about in considering the greenhouse effect. However, the greatest opportunity for reducing CO2 emissions in the next several decades is undoubtedly to increase the efficiency of energy use. And this illustrates a major point in considering environmental issues in general, that it is important to look at the entire system and not to focus too narrowly, in this case on the energy production side; to do so may do little more than lead us to confirm our own preconceptions. I will return later to questions related to energy use, but I'd like first to turn to other illustrations of how we tend to think about environmental issues, beginning with nuclear power.

The central element of a nuclear power plant is the reactor and the associated elements for transferring heat and producing steam to run the electric generators. Beginning in the mid-1970s, such systems—and particularly those using ordinary ("light") water as the coolant—were subjected to systematic safety analyses that developed complicated "fault" trees to identify accident types and estimate their probabilities. The accident at Three Mile Island (TMI) demonstrated that our thinking about accident sequences and probabilities was incomplete—a fault in the fault tree approach--illustrating the difficulty of understanding everything that might happen.

However, TMI illustrates a broader fact about nuclear safety analyses—particularly the major "Reactor Safety Study" completed in 1975 by the Nuclear Regulatory Commission (NRC). These analyses tended to be misleading about which classes of accidents actually contributed most of the health risk. It was clear that the smaller accidents were much more probable, but this was interpreted to confirm the earlier presumption that really large accidents were so improbable as not to contribute much of the total risk from nuclear accidents. Even after modification in response to criticisms, the NRC's 1975 report gave the clear impression that most of the overall risk of death from reactor accidents came from small accidents that were much more probable than from the larger ones.

I recently went back to an analysis I performed in 1976 of the detailed content of the NRC report. This analysis led to exactly the opposite conclusion. Distinguishing between "early" deaths soon after an accident due to large exposures, and "delayed" cancer deaths estimated to occur long after, the detailed data of the reactor safety study imply that most of the risk of early deaths comes from large accidents in which at least 400 people are killed! And most of the delayed cancer risk arises from accidents in which more than 2000 die. Interestingly, the accidents that would cause a couple of thousand cancer deaths would cause no—i.e., zero—early deaths. These estimates contrast greatly with the impression left in the main body of the report, that most of the total risk is contributed by small accidents with minor consequences. I can only suggest that those who wrote the report tended to look at their results only in ways that would support their preconceptions, a common but dangerous way of examining environmental questions. They therefore missed the fact that what they had done actually contradicted those preconceptions.

The authors also did not specifically highlight the total risk arising from different classes of accidents in terms of the broad nature of failures that occurred. Even a crude analysis of their results indicates that the total risk was dominated, for each reactor type, by one or two kinds of very large accidents. This was not indicated in the report, which is consistent with the fact that the report did not point to large accidents as the major cause of deaths—reflecting, again, a preconception or myth that large accidents could virtually never happen.

Of course, in spite of the consternation it caused risk analysts (including me), the accident at Three Mile Island was in these terms a very small accident. There were no deaths due to acute radiation exposures and virtually no estimated cancer deaths (though those living in the vicinity still do not believe this). But it did indicate that large accidents could really happen.

Such an accident did occur in Chernobyl, which caused 31 deaths due to acute exposures, and is estimated to cause many thousands of cancer deaths, roughly fitting the picture just indicated. I should say that the Chernobyl reactor itself is not one to which the accident probabilities calculated by the NRC study can be applied directly, because of the reactor's peculiar design and operation (especially on the night of the accident). But, because of its large radioactive release through a breached containment, the Chernobyl accident does fit the pattern characteristic of the larger accidents examined in the NRC reactor safety study in terms of amount of radioactivity released and resulting deaths observed or estimated. Thus, in general, the accidents at TMI and Chernobyl have dispelled the myth characteristic for many years of nuclear proponents—that large accidents could never happen.

Of course, the power plant is only one part of a nuclear power system. Others of critical environmental importance are waste disposal facilities and facilities for fuel reprocessing and plutonium recycling should this occur. Consider first the issue of waste disposal, either as the residue from reprocessing of used fuel assemblies or as the disposal of this spent fuel without reprocessing.

In terms of developing a balanced picture of the various nuclear issues, let me point out that in contrast with waste disposal facilities, the power plant itself sits on top of the ground, has a hundred times the radioactivity in a waste disposal site and, similarly, much more energy to cause a dispersal of this activity. Although these observations do not constitute a complete comparison, all these factors suggest that the potential for reactor accidents is a much more serious risk than that from waste disposal, assuming moderate care. Or to put it another way, if we simply act sensibly it is hard to see how waste disposal could, even with substantial breakdowns, affect more than a local area, in contrast with other types of environmental problems or with reactor accidents.

The longevity of wastes adds a peculiar aspect to the issue, but this seems to me to be characteristic of all wastes, though we don't usually think in these terms. Chemicals and trash that we put in the ground will remain there a very long time and even the most benign gas from some points of view, CO2, can have vast global effects with time constants measured in decades to centuries. This is, I suggest, the same time scale for nuclear waste disposal—it takes about 300 years for the non-plutonium activity of the fuel to decrease by a factor of a million after which plutonium dominates the problem. This is therefore the period of primary concern for disposal of the other types of radioactivity. And the main issue for plutonium, by far, is its potential use in nuclear weapons—not the possibility of environmental contamination from disposal facilities.

This suggests how I would rank my concern about the major nuclear power issues: accidents in the middle, waste disposal needing attention but with much less potential for affecting the environment and, as the major issue, the use of nuclear materials for weapons that would purposely release destructive energy.

Which leads me to repeat a story. In the 1930s Leo Szilard had been thinking about the possibility of a nuclear chain reaction, largely because of his concern that it could be used to produce weapons. When fission was discovered in 1938, he immediately saw it as a basis for such weapons. He urged Rabi to press the need for secrecy with Fermi, but Fermi's response was "Nuts!" When they asked Fermi why, he said there was only a "remote possibility" that uranium emitted neutrons when it fissioned, in which case a chain reaction might be made. When Rabi asked what he meant by "remote possibility," Fermi said ten percent. Rabi remarked that ten percent is not a remote possibility if it means we may die of it.

Here we have a practical question of risk estimation and perception! I would have to agree that a ten percent chance of destroying ourselves or the world is a big risk. And even if nuclear power contributes only a portion of that risk, because the materials it uses can be diverted to use for weapons, then this possibility can be, and I think is, the biggest nuclear issue. Indeed, nuclear war would be the ultimate environmental catastrophe, compared to which the greenhouse effect, and certainly waste disposal, seem barely significant.

In 1978, while spending a year at the Arms Control and Disarmament Agency, I paid a visit to Serpukov, near Moscow, not in connection with their particle accelerator as one might suppose, but as part of the International Nuclear Fuel Cycle Evaluation. This was an effort to examine the way that commercial nuclear power might develop and, in particular, the degree to which different forms might be susceptible to misuse for production of nuclear weapons. Those meeting at Serpukov were members of the working group on fast breeder reactors, which would use plutonium from reprocessing to generate more plutonium, as well as power. There I found myself presenting the US paper on "proliferation resistance" to a group made up primarily of the heads of fast breeder development programs of various nations. Many observers thought it was fairly clear that the spread of plutonium recycle and breeders could widen access to materials for nuclear weapons, and this paper was intended to analyze the problem systematically. I have to say it did not get a sympathetic reception from this group. I guess I would have worried if it had.

In fact proponents of recycle had developed an effective metaphor for the current situation, where plutonium remains in the spent fuel. They said spent fuel repositories were like plutonium mines—not a nice thought. The proponents of reprocessing got a lot of mileage out of the "plutonium mines" picture, though it seemed fairly obvious that reprocessing and recycle would significantly increase access to plutonium. It seemed to me that there had to be some equally pungent description of the reprocessing/recycle scheme that would make the relative vulnerability transparent. The answer is obvious, once you know it: recycle constitutes a "plutonium river," a flow of plutonium in a form where it is much easier simply to dip into this stream to get weapons material than to have to extract it from a highly radioactive spent fuel assembly. Words can be very important and very misleading if all they do is reinforce a preconception or myth. And the potential for diversion of nuclear materials, especially in nuclear systems using advanced technologies, still constitutes in my view the most important issue of nuclear power. Furthermore, developing a perspective that includes all the nuclear issues helps provide a context for judging the importance of the individual issues and for deciding what we might do about them.

Since we are talking about nuclear weapons, I cannot resist mentioning another myth, that of an impenetrable shield against nuclear weapons. From the beginning, most of the scientific community viewed "Star Wars" as imperfect and expensive, so that selling the delusion of impenetrability to the US public was no more than the perpetration of a myth, perhaps to achieve other ends, such as the acquisition of a new and expensive weapons system (one that the Russians probably rightfully see as most effective in conjunction with a preemptive strike by us, not them). Regardless of the motives, what was sold was a myth, or perhaps more precisely a lie—a big lie. If you portray something strongly enough, people buy it. At present the administration is backing off the original Star Wars and selling a version that is no more than a moderately effective antimissile system, one that is probably most effective if used offensively. In terms of defense, a system, even if it works, that passes a few percent of thousand of warheads almost certainly means that you and I will be dead. What on earth has happened to the Arms Control and Disarmament Agency, if it is now supporting such armament!? Both the sword and the shield are arms; unless the shield is completely effective without the sword, it is a weapon itself. Again, one needs to look at the full picture, and not be deceived by the words or myth.

Now, I'd like to turn to the smallest and most important environment from a personal point of view—our homes. Here one can find a romantic, "star wars" approach to controlling exposures to radon and other indoor air pollutants. This may sound ridiculous, but it is definitely not a joke. There have been perfectly serious suggestions not just that high indoor concentrations of radon be reduced, but that the average be reduced by a factor of 10 or even 100 to 1000. This radical suggestion was made in a book on risk assessment where, as far as I can tell, the authors had not bothered to calculate the reduction factor required to reach their suggested risk limit of one chance in 100,000 of dying from radon exposures. I pointed out to the publisher that this could be achieved by staying outside and breathing only once an hour—in which circumstances I guarantee one will not contract lung cancer from radon or anything else. This book received good reviews which indicates some of the pitfalls of risk assessment. It also illustrates the danger of thinking one is wearing a white hat when pressing for radical reductions in estimated risks, whether from radon or from outdoor toxins. Trying to force new problems into preconceptions appropriate for other circumstances can lead to nonsense or, worse, diversion of effort and resources from the real problems.

This kind of difficulty is characteristic of indoor pollutants in general. An indoor environment such as the home is the site of exposure to a wide range of pollutants. And it is here that we spend most of our time, here that the concentrations of combustion emissions, radon, and organic compounds typically exceed those outdoors, often by very large factors, and here that measures to reduce energy use by decreasing ventilation rates might raise indoor exposures even more.

This potential influence of energy conservation illustrates that in considering the health risks associated with energy, one can't simply look at energy production. We have an energy system, and improvements in one respect may have negative effects in another. One has to look at the entire picture. As it turns out, we soon learned that ordinary energy conservation measures had a modest effect on indoor exposures compared with the wide variability already there, due mostly to differences in the source term, the rate at which the pollutants were emitted into the indoor atmosphere. Thus, although one shouldn't ignore the potential effects of energy conservation, it is not the most important issue in indoor air pollution.

Thinking about the indoor environment also suggests a broader issue. For the important environmental effects, we probably miss the point by considering these effects to be residuals, or side effects, of our activities. For example, when we use energy to heat indoor air or for other purposes, we also alter the global environment. Or, when we use resources to produce things, they end up in a waste disposal site. We are now enough people on this earth that it makes more sense to think of these transfers as primary effects, transformations within the overall system, and not as mere side effects. Taking this different point of view could result in a markedly different way of thinking about environmental and other issues. It may lead to our treating the earth a little more tenderly.

I'd like to turn for a moment to a one-to-one comparison of indoor radon with nuclear power, a comparison that generates a lot of feeling which in itself illustrates an important point. Speaking roughly, the range of annual indoor radon exposures is similar to the distribution of lifetime exposures to the world population from the accident at Chernobyl. In detail, there are differences, but the average exposure to radon is the equivalent of several Chernobyls per year, and the population receiving very high exposure from radon includes roughly as many people as were evacuated from around Chernobyl and who received comparably large doses—but only once—not every year. The point of this comparison is not to cause fear of indoor radon or to minimize the importance of Chernobyl, particularly to those who lived nearby. It is rather to suggest two things. First, the need to develop a perspective on the various sources of exposure, in this case to radiation. And second, where there appears to be some clash or contradiction in our perceptions or responses, the need to delve into and understand the premises for our understanding. Building an effective perspective typically requires modification or broadening of our premises.

This also becomes clear from considering the full range of risks that can be estimated for exposures, not only to radon, but also to other pollutants in indoor air, including cigarette smoke, volatile organic compounds, and asbestos. The estimated risk of premature death from average indoor exposures ranges from about 0.02% for asbestos to about 0.4% for radon. These factors are a hundred to a thousand greater than the levels of risk deemed to merit regulation in outdoor air, water supplies, or food. On the other hand, these indoor risks are entirely comparable to, or less than, other risks accepted as part of our daily lives, such as the risk of dying in an accident in our homes or in our cars. They are also similar to risks accepted in connection with exposures to toxic agents in industry.

What this indicates is that in considering how to think about indoor pollutants, we need to develop a broader perspective on environmental risks. And it illustrates the danger of trying to control indoor pollutants in the same framework that has been developed for outdoor pollutants. Developing a proper context for evaluation will avoid such absurdities as the above-mentioned requirement that we breath less (or wear space suits) to reduce the radon risk by a factor of 1000. It may also avoid the lesser, but still very expensive, goal added recently to toxic waste legislation, namely that indoor pollution levels be reduced to outdoor levels. This would probably require hundreds of billions of dollars over a period of many years to create an impermeable shield against radon from the earth—in effect the Star Wars of the environmental movement.

Living indoors, like driving a car or working in certain occupations, involves risks that we can control only to a limited degree. Basically they are part of the system, and trying to reduce them to insignificant levels means almost literally living in a vacuum.

All of this suggests several respects in which we need to change how we examine environmental risks: (1) In looking at one issue, it is important to look at related issues and, as in the case of indoor air, at the context, in order to develop a sensible perspective. Often we have to reexamine our premises and avoid making judgements based on some mythical picture. (2) We ought to recognize that in using energy or materials to improve our lives by providing various amenities, we are also causing broad changes in the environment or resources, so that associated effects have to be considered not as residuals, but as alterations of the basic terrestrial balance of resources and environment. (3) We need to consider issues in their full temporal dimensions: not just in terms of three-year paybacks, or a decade or two into the future, but in terms of longer times or time constants.

We may aspire to much but not at the expense of ourselves and the earth.

On a practical level, in terms of broad classes of environmental risks, we have to pay attention to more than regional environmental effects and recognize the importance of the microenvironments in which we live and work, and the global environment in which we have to live.

Leo Szilard said some things that are relevant and I here quote several of his ten commandments: (1) Recognize the connections of things and the laws of conduct of men, so that you may know what your are doing. (4) Do not destroy what you cannot create. (6) Do not covet what you cannot have. (10) Lead your life with a gentle hand and be ready to leave when you are called.

To the last might be added, "Treat the earth gently, so that we don't have to leave."

Forum Award Lecture: Fooling Some Scientists Some of the Time

James Randi

[Editor's note: The author is winner of the Forum's 1989 Forum Award for promoting public understanding of the relation of physics to society. His citation is given in the April issue. The following paper is based loosely on his lecture at the Forum Awards session held 2 May 1989 at the Baltimore APS meeting. James Randi, who lists his profession as "iconoclast," resides at 12000 NW 8th Street, Plantation, FL 33325-1406.]

More than a decade ago, the American Association for the Advancement of Science (AAAS), at the insistence of then-president Margaret Meade, admitted the Parapsychological Association (PA) to its ranks. Meade pointed out quite correctly that if the PA is, as it claims, a serious group of researchers actively looking for evidence of paranormal powers using proper scientific methods, it should be admitted. It is true that the group is searching for one replicable, properly conducted, correctly reported, experiment that will support the belief in ESP, psychokinesis, prophecy, or other supernatural powers that many of them share. But unlike all others affiliated with the AAAS, those in parapsychology have no such experiment to offer.

It appears that psychics and their supporters are playing a strange sort of game that requires them to believe, but not to test those beliefs. This is, in its way, a morality play in which they are the actors. They must be aware that any definitive tests of these matters have always resulted in failure, but the victims of this delusion, whether they are academically trained or mere amateurs, firmly refuse to be shown that they have duped themselves.

I am a professional conjuror by trade. That is to say, I trick and deceive people to make a living but I do it as an entertainer. This professional expertise allows me to detect flimflam when it is in operation. Along the way, I have discovered that scientists are uniquely susceptible to two kinds of deception. One kind is deception that may be practiced on the experimenter. The other kind, far more dangerous, is self-deception.

A few years ago, intending to test the ability of a well-publicized and well-funded parapsychology lab in St. Louis to differentiate between simple tricks and genuine psychic powers, I encouraged two young students of conjuring to offer themselves for testing. The lab director had already announced in the press that he did not intend to conduct his tests under strict control conditions, because, he pointed out, in parapsychology it had already been determined that subjects did not do well under those circumstances.

The two kids did very well, evoking from the experimenters very positive statements about their striking psychic abilities. When I tipped off the scientists at the lab, they tightened up their protocols, the observed miracles ceased, and the lab announced that they had known about it all along. This was an invented 20/20 hindsight, judging from the written "preliminary" reports that were issued from that lab just prior to my revelations. In spite of the torrent of disclaimers and reversals that ensued, my experiment was a resounding success.

John Hasted, physics professor at Birkbeck College, London, and John Taylor, mathematics professor at King's College, London, decided some years ago that mere children were not capable of performing tricks that would deceive adults—particularly well educated adults—when they saw former psychic star Uri Geller and his juvenile imitators do spoon-bending tricks. Both scientists wrote books and articles on the subject, fully supporting all the simple tricks they observed as genuine miracles. The fact that I and other conjurors duplicated these effects by admitted sleight-of-hand had no effect on their beliefs whatsoever. Only after a few years had passed did Taylor finally come around to admitting that he may have been deceived. Hasted still firmly believes he was not, and cannot be, deceived.

This attitude is similar to Sir Arthur Conan Doyle's attitude. He believed that two pubescent girls in 1917 had actually produced photos of fairies because, in his words, they were "of the artisan class," and thus too unsophisticated to decieve him. He did not live to learn that the two had been simply cutting figures from paper, coloring them, and photographing them in the glen near their home in Surrey, though his colleagues were well aware of the simple imposture. Until recent months, one of those deceivers was still alive in England, chortling over the scientists who went along with Sir Arthur simply because he was a celebrated figure of the day.

In the early 1900s, "N-rays" were discovered in France by Rene Blondlot, a respected physicist at the University of Nancy. These emanations were then observed, measured and marveled at by numerous scientists around the world, and articles verifying the experiments poured in to scientific journals. Those replicating the experiments were seeing results simply because those offering the original evidence were respected in their fields. Others, rightly skeptical, showed them conclusively that this phenomenon did not exist. Typically for the academic world, that was a signal for silence to envelop the subject.

In more recent times, "polywater" was able to momentarily entrance some scientists, but then was quietly forgotten. A new notion, cold fusion, is presently enjoying a certain notoriety.

Dowsing (divining for water, oil, or any other substance or object using forked sticks, wires, pendulums or various other whirligig devices) has been one of the most persistent delusions entertained by practitioners and scientists alike. I have extensive files of correspondence with a number of academics who claim the ability to dowse, but who simply refuse to be double-blind tested. Their reluctance is a puzzle to those who have not entered into such matters, but quite understandable to those who have become involved. Perhaps these unwilling enthusiasts are aware of the *New Scientist* article of a few years back that reported 100% success in a dowsing test until the experiment was repeated double-blind. The tally then became 48% positive, 52% negative, a not-unexpected result.

In Germany, certain physicists are currently enthralled by dowsing, which is second in persistence only to astrology in the field of crackpottery. The West German government has already spent 400,000 marks to test crowds of stick-waving enthusiasts with a very complex and expensive technology. It is believed that the German dowsers are able to detect "E-Rays," mysterious emanations of undetermined parameters that are supposed to be given off from unknown underground sources and are said to be a major cause of cancer. All over the Bundesrepublik, dowsers are being called in for consultation so that hospital beds and office desks may be moved to new locations to avoid this deadly radiation.

My recent investigation of the claims of a number of television faith-healers revealed that many modern M.D.s accept superficial evidence of faith-healing simply because they believe that divine intervention in such cases is not only possible, but common. To them, close investigation of these claims is blasphemous; their fervent belief is enough. Their colleagues think that the blasphemy is against science and not against heaven. It is the believers, rather than the unbelievers, who reach public attention through the media simply because their attitude is more popular and comforting. Thus more ammunition is brought against rationality.

When I have tried to force certain scientists into confronting the possibility that they may have fooled themselves or may have been deceived by subjects, they have very often retreated to a defence which is nothing more than an appeal to authority. They have thrown before me endless articles, papers, and books by noted authorities who have declared themselves in favor of various psychic, paranormal, and supernatural phenomena. Up until a few years ago, Professor John Taylor, Sir Cyril Burt, Dr. Walter Levy, and Dr. George Soal were among those they gleefully offered to refute my skepticism. It turned out that these authorities, at least, were depending upon feats of clay. Taylor listened to what I had to tell him and conducted further tests of his ideas about ESP and psychokinesis. Those tests were negative, and he wisely retired from parapsychology. Burt, in a widely publicized expose, was shown to have falsified and invented data concerning inherited traits. He had very strong prejudices which he wanted validated. Levy, the project head for Dr. J. B. Rhine's ESP lab in South Carolina, was discovered to have been "cooking" data to please his superiors, and he resigned in disgrace. Soal, a UK researcher following Rhine's lead, had produced startling significant results that were dumped when investigators found he had changed a few figures to bias the results. Since these "authorities" have either been caught in blatant trickery or shown to have been self-deceived, they have recently been left out of the references offered to me.

It would be wrong for anyone to believe that a respectable education insulates scientists against being deceived. The fact is that people who have been educated in a formal manner often miss instruction in the "street smarts" that others use in the real world, rather than the theoretical world of the academics. As Arthur Clarke once put it, perhaps unkindly, "Many people are educated far beyond their intelligence." That is to say, many scientists have learned to follow strict methodologies but fail to allow for errors of common sense.

Some scientists are being fooled by "psychics" and other professional fakes because they fail to bring into their decision-making process the fact that human beings can and sometimes will deceive, for whatever reason. Unlike an electron, a bacillus, a falling cannon-ball or whatever other entity is placed under observation, a human being has the ability to purposely skew data by any number of means and to conceal that influence from casual observers. Reasons for such interference may not be obvious, and the intent might well be only mischievous. Frequently the intent is to use the incautious validation of an accredited academic to further a career that is of dubious value to mankind.

From my observations, I would say that certain scientists tend to fool themselves (a) because they have certain deep-seated needs to believe what is being offered them, (b) because they think that their training and intellect will protect them against being deceived (i.e. they are too smart to be fooled) and, (c) because they believe their colleagues are incapable of error.

In 1975, the academic world took scant notice of a group of scientists, philosophers, and specialists who gathered to form the Committee for Scientific Investigation of Claims of the Paranormal (CSICOP). This group is dedicated to examining and evaluating various claims, and making their findings available to interested investigators, students, and the media. Their journal, the Skeptical Inquirer, now reaches 40,000 worldwide, and there are now an increasing number of other groups internationally who share CSICOP's general views and aims. The committee is now well-recognized and has served many departments of the U. S. government who have asked for its advice concerning finding of questionable projects.

What lies ahead for parapsychology? From my experience, I would say that the parapsychologists will continue to search for that elusive experiment that they believe will vindicate their faith in the unlikely phenomena they pursue. Charlatans will continue to pop up to snare unwary scientists, and those scientists will persist in defending untenable positions because they have invested too much time in them. Though I see an increasing role for CSICOP, it is unrealistic to think that the committee will ever be able to retire, having finally brought a universally cautious attitude to science.

There is one specter that looms constantly in view: There just may be an interesting baby in this rather murky bath tub. If so, it appears now to be an elusive, fast-moving and rather ephemeral child. The danger is that a baby can grow up to be a monster. Nonetheless, if it is there, we must rescue it and raise it to maturity. Then we may see what the parapsychologists have been railing about.

I, for one, am not holding my breath.

Symposium: Technology for Nuclear Arms Control. Monitoring Compliance with the INF Treaty

The following two papers form another of our sets of papers based on Forum-organized invited sessions. These papers are based on a session held on 22 March 1989 at the St. Louis APS meeting. The session was organized and presided over by Richard Scribner of the Georgetown School of Foreign Service, Georgetown University, Washington, DC 20057. Since a complete manuscript of the second paper, by Donald Bauder, was not available, that paper is reprinted here in outline form only. Manuscripts were not available for the other two papers in the session: "The physics of portal and perimeter monitoring for arms control," Stanley Fraley of Sandia National Labs, and "Multispectral and moderate resolution satellite sensing for verification," Peter Zimmerman, Carnegie Endowment for International Peace.
Editor

On-Site Inspection for the INF Treaty

Edward Lacey

On 8 December 1987, President Reagan and Soviet leader Gorbachev signed the "Treaty Between the United States of America and the Union of Soviet Socialist Republics on the Elimination of their Intermediate-Range and Shorter-Range Nuclear Missiles," commonly known as the INF Treaty. The treaty was ratified early last year and entered into force on 1 June 1988. It consists of four integrated documents: the treaty itself, a data-base "memorandum of understanding" listing treaty-limited items, a "protocol on eliminations," and a "protocol on inspections." All in all, the treaty calls for the elimination of some 8,000 individual items, including 1,846 Soviet and 846 U.S. intermediate and shorter-range nuclear missiles. By early 1989, the USSR had eliminated almost 40 percent of its INF missiles and the U.S. about 35 percent.

The On-Site Inspection Agency (OSIA) was established on 15 January 1988 with the twofold mission of carrying out the required inspections of Soviet INF facilities and elimination activities, and of ensuring that the Soviet Union is able to exercise its treaty-mandated inspection rights. The INF Treaty provides for five different types of on-site inspection: baseline, close-out, quota, elimination, and portal monitoring.

Baseline inspections assisted in verifying the exchange of data contained in the treaty's memorandum of understanding. These inspections had to be carried out 30 to 90 days after the treaty entered into force. After all treaty-limited items are removed from an INF base or facility, and all INF-related activity has ceased, a close-out inspection of the site may be carried out to confirm that it no longer is engaged in INF operations. Follow-on inspections of INF facilities are permitted after the baseline period of 13 years. These inspections, known as quota inspections, are intended to enhance confidence in adherence to the terms of the treaty.

Elimination inspections are designed to confirm the elimination of treaty-limited missiles, launchers, and associated equipment in accordance with the procedures set forth in the treaty's protocol on eliminations. Both sides are required to monitor the elimination of these items. Finally, portal monitoring inspections consist of reciprocal, continuous, on-site inspection activities at two former INF missile production facilities, one each in the U.S. and U.S.S.R.

Baseline inspections. On 1 July 1988, U. S. and Soviet inspectors began the baseline inspection process that would account for all treaty-limited equipment specified in the memorandum of understanding. Some sites in both the Soviet Union and the United States contained multiple facilities. Thus only 115 separate inspections were required to cover the 133 Soviet INF facilities while just 21 inspections covered the 31 U.S. facilities.

During inspections, every structure and vehicle within a facility that was capable of holding a treaty-limited item was subject to inspection. The inspection team had 24 hours to complete the inspection. However, the inspection could be extended by an additional eight hours if the inspected party agreed. Following the inspection, a joint report was prepared in both English and Russian detailing the missiles and other INF equipment identified at the facility. In this effort, OSIA accounted for the approximately 6000 Soviet treaty-limited items listed in the memorandum of understanding, while Soviet inspection teams accounted for the almost 2000 items listed for the United States.

The baseline inspections, completed by 31 August, involved 146 inspections in nine countries. In addition to inventorying the treaty-limited items detailed in the memorandum of understanding, the process institutionalized on-site inspections between the superpowers.

Close-out inspect*ions:* Close-out inspections are conducted after one of the parties to the treaty declares that a facility is free of INF equipment and no longer supports intermediate or shorter-range missile operations. Within 60 days of the declared close-out data, the other party has the right to visit the facility to confirm its new status. Some INF facilities achieved this status prior to the 1 June 1989 entry-into-force of the treaty. The remaining INF facilities will be closed out over the three year elimination period of the treaty.

Quota *Inspections:* The INF Treaty provides an annual quota of short-notice inspections for a period of 13 years. These inspections are conducted along the same lines as the baseline inspections, with only minor differences. All of the facilities elaborated in the memorandum of understanding are subject to these inspections. For the first three years of the treaty regime, each side can conduct 20 short-notice inspections annually. During the next five years, 15 such inspections are permitted each year. The annual quota for the final 5 years is 10 inspections.

Elim*ination inspections:* The treaty requires that the elimination

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of intermediate and shorter-range missiles, launchers, and associated support equipment be monitored on-site by the other side. The Soviets began eliminating these missiles and equipment in August 1988. The U.S. began the following month.

The U.S. is conducting eliminations at three facilities in the United States and one in the Federal Republic of Germany. Intermediate-range Pershing II and shorter-range Pershing IA missiles are being eliminated at U. S. facilities in Longhorn, Texas and Pueblo, Colorado. The Pershing rocket motors are static fired. The spent motor cases—as well as the missile front sections—are crushed or flattened. The ground-launched cruise missiles (GLCMs) and their support equipment are being eliminated at Davis-Monthan Air Force Base in Arizona. After allowable components such as the fuel and the guidance package are removed, the GLCMs are cut longitudinally in half with titanium-steel bladed powersaws. Pershing II missile launchers based in Europe are being eliminated at the U.S. Army Equipment Maintenance Center Hausen, near Frankfurt. The treaty provides that the launch vehicles be disassembled and cut into pieces.

The USSR has more than twice as many missiles and three times as many other treaty-limited items that must be eliminated as the United States. The missiles and launchers are eliminated at designated facilities in the USSR. The SS-20 missiles are being destroyed at the Kapustin Yar Missile Test Center by explosive demolition. In addition to explosive demolition, 72 SS-20s were launched to destruction from missile facilities at Kansk and Chita in the eastern USSR. The treaty allowed each side to destroy up to 100 intermediate-range missiles in this manner during the first six months of the treaty regime. The U. S. decided against exercising this option. The SS-20 launcher and transport vehicles are being eliminated at a seperate facility in Sarny. The older SS-4 and SS-5 intermediate-range missiles are being cut into pieces at a facility in Lesnaya.

The Soviets' shorter-range missiles, SS-12s and SS-23s, are being eliminated at a facility in Saryozek, while their launcher and transport vehicles are being eliminated at Stan'kovo. The elimination process is identical to that for the intermediate-range systems. The Soviets' intermediate-range GLCM, the SSC-X-4, was eliminated at Jelgava, near the city of Riga. It was never operationally deployed, and all 80 that were accounted for in the baseline inspections were destroyed by 5 October 1988.

Portal monitoring: The treaty allows both sides to establish a detachment of inspectors outside of the gates or "portal" of a designated missile production plant. The U.S. detachment is at the Votkinsk Missile Assembly Plant in the Ural Mountains east of Moscow. This is the plant that formerly assembled the intermediate-range SS-20 and shorter-range SS-12 and SS-23 missiles. At present, the plant produces the Soviets' land-mobile SS-25 ICBM.

The Soviet detachment is at the Hercules Corporation's Bacchus Works near Salt Lake City, Utah. This plant once produced rocket motors for the treaty-limited Pershing II, and now manufactures boosters for the Peacekeeper ICBM and Trident submarine-launched ballistic missile.

The portal monitors have the right to inspect every shipment leaving the plant that is capable of containing an SS-20 (at Votkinsk) or Pershing II (at Hercules) missile stage. They will retain this right for 13 years, or until these provisions of the INF Treaty are superseded by the Strategic Arms Reduction Treaty (START).

Although the inspections are being carried out in a thorough and professional manner, we cannot afford to be complacent. We are less than one year into a 3-year elimination and a 13-year inspection regime. We are off to a good start, but we have a long way to go. Moreover, on-site inspection is not a panacea; it has its limits.

The experience of the On-Site Inspection Agency nevertheless gives testimony to the very positive role that on-site inspection can play in arms control. The Soviets have proven to be serious and businesslike throughout the INF inspection process. In the final analysis, that is what the INF Treaty is all about.

Tagging: "Fingerprints" and Electronic Labeling for Arms Control

Donald Bauder

Outline of talk:

Tagging is a means of positively identifying specific items. Tagging allows one to distinguish legal from illegal treaty-limited items (TLI). A tag is applied to each TLI when declared by the Soviets to be one of the items allowed by the treaty.

•This would include all TLIs that exist when a treaty goes into effect. Tagging would be done at storage and deployment sites.

•Tags would be applied to new production of TLIs as they pass through a monitored portal at the production facility.

In subsequent inspections, tags would be inspected to determine authenticity. These inspections would occur at deployment sites and storage sites.

If a treaty allows reducing the TLI count when an item is returned to a facility for repair, tags would be authenticated to make sure that the Soviets were returning a real TLI and not a surrogate. Similarly, tags would be authenticated on TLIs scheduled for destruction or testing.

Verification of limits

The most obvious method for verifying treaty limits is to count all TLIs. We would know that the Soviets are cheating only when the count exceeds the treaty limit. This can be difficult to do, especially if TLI's can be moved easily. It requires essentially simultaneous inspection of all TLIs.

Tagging can be implemented as a sampling procedure:

•If the Soviets have placed illegal TLIs at inspectable sites, possibly along with legal ones, each inspection gives us a probability that we will detect one of the illegal items.

•It will not be necessary to inspect all TLIs. A small random sample will give a high probability of detecting cheating.

Tagging methods

Sandia and other labs have proposed two tagging methods:
•fingerprinting,
•electronic identification devices.
I will discuss each briefly.

Fingerprinting concept

The basis of fingerprinting is that every object has unique features that distinguish it from every other object. These unique features can

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serve as positive identifiers if:

•they are readable, i.e. if it is possible to create a recordable description of the unique features,

•they are durable, i.e. if the unique features do not change significantly between readings,

•if the features cannot be transferred from one item to another,

•if the features cannot be duplicated.

We have considered the use of the natural unique features of TLIs as the fingerprint and also the use of unique features that we can add to the item.

Natural vs added unique features

Natural and added fingerprints have inherent advantages and disadvantages.

Natural features will be different on different types of TLIs depending on the materials they are made of and how they are manufactured. Different features may require different reading and authenticating methods. Many natural unique features that can be observed are topographic. The roughness of a machined surface, for example. These are easy to counterfeit by taking a mold from the surface and then casting a duplicate. Many features are microscopic and subject to change due to wear and tear, aging and corrosion. They are difficult to read because precise positioning of a reading device is necessary.

Natural features have advantages:

•They may be difficult to remove from one item and applied to another without damaging the item.

•No alteration of the TLI is required.

Added features can be tailored for easy reading and authentication. We can make the characteristics uniform in the sense that tags on all items can be read and verified with the same equipment. The features can be submerged below the surface of the tag:

•This prevents counterfeiting by molding and casting.

•The features are less subject to changes.

Added features have the disadvantage that they must be designed carefully and tested thoroughly to make sure they cannot be removed from one item and placed on another.

Reproducibility

As a result of our work on tagging and on other positive identification concepts, we have developed some rules that apply to any fingerprint tagging concept:

•All two-dimensional patterns of materials are reproducible with common reprographic processes such as photography, xerography, lithography.

•Multi-dimensional patterns of materials made to a specific configuration are reproducible. If you make a pattern to a pre-determined configuration and I duplicate your process, I can make one too.

•Some randomly generated multi-dimensional patterns are impossible to reproduce by known reprographic processes. Not all multi-dimensional patterns are non-reproducable. As I mentioned before, surface or topographic features can be reproduced. Holograms can be reproduced, and there are others.

Show tag

This is a tag we have proposed. It consists of a clear plastic material—either epoxy or acrylic with particles of crystalline micaceous hematite embedded in it. With each angle of illumination there is a different pattern of reflectors. These patterns are a function of the random locations of the reflective particles and the random angles of the reflective surfaces. This is the information that we read and record to describe the tag.

The mixture of clear plastic and reflective particles would be painted onto a small area on a non-replaceable part of the TLI such as the rocket motor case. After the material is cured the tag would be read with a special reader consisting of a still video camera and a number of lights. After the reading is placed on the tag, the lights will be sequenced and a picture will be taken with each light. Data will be recorded on a 2" floppy disk which will be stored. When field inspectors verify treaty limits, the tag will be read again. This recording and the original recording will be read into a computer where the patterns will be compared to see whether or not they are the same.

These tags contain a tremendous amount of unique information: not only the locations and angles of reflective surfaces but also shapes of individual particles, bubbles in the plastic, colors, and many other features. A counterfeiter would not have to include all of these features in his counterfeit to fool the reader. He only has to reproduce or simulate the features that the reader records.

Some methods have been proposed to make counterfeits that might fool the readers. To deter such counterfeits, we believe that we must have the right to do other interrogations of the tag such as visual inspection, high resolution photography, sampling of the material, ultrasonic interrogation. It would not be necessary to do all or even any of these interrogations each time we read a tag. Just having the right to do them would be a deterrent to counterfeiting attempts. People who are red-teaming this concept believe that no counterfeits they could make would pass visual inspection. They certainly would not pass high-resolution photographic inspection.

The key to the concept of electronic identification devices (EID) is that the device will respond with a unique message when interrogated.

Basic requirements for an EID are:

•secure storage of a code or key,

•means of communication,

•means of assuring that the device remains attached to the TLI, i.e. that it cannot be removed from one TLI and then attached to another.

Active and passive electronic tag systems

EDIs may be passive or active. A passive EID would consist of:

•a microprocessor to perform logic functions,

•a memory to store secret codes or keys,

•a means of communicating into and out of the EID,

•a means of determining if the EID has been moved from one item to another,

•a means of assuring that the EID has not been penetrated to read out the secret code.

A potential weakness of a passive EID is that there is no on-board power and so cannot perform electronic functions except when it is being interrogated.

Protection of the stored secret codes must depend upon methods for housing or covering the memory that will result in physical damage to the memory if it is penetrated. This physical damage must prevent subsequent read-out of the stored code.

Sensors can be placed in the EID to sense its attachment to the TLI. The sensed values can be monitored when the device is attached and then checked in subsequent interrogations to make sure the values have not changed. However, if the device has been penetrated to read out the secret code, counterfeits could be made that would simulate the correct sensor output.

Some of these vulnerabilities can be circumvented with an active EID. An active EID would include all of the basic elements of a passive EID but would also include:

•real-time clock/calendar,

•active sensing of penetration and attachment,

•penalty response (erase memory),
•battery.

The microprocessor and memory functions required for EIDs can be handled with existing commercially available hardware and present no challenging problems. Communications and power coupling can be managed with straightforward existing hardware concepts.

Sensing removal and replacement are challenging problems. There are no proven solutions at this time. There are some promising concepts that are being pursued but they are far from being ready to implement in a treaty.

Protocol considerations

The development of tagging technology and procedures is very sensitive to specifics of a treaty. Items that must be considered include:

•what to tag,
•where and when to tag,
•who applies tag,
•verification decision: on-site, central location, inspector's decision, decision by technical equipment, require U.S.-host concurrence,
•storage of tag descriptions: U.S. only, U.S. and host, third party, public information, identifying specific TLIs, identifying legal versus illegal,
•data transmission method,
•number of inspections allowed,
•rights of inspectors,
•locations of inspectors,
•rules of arbitration.

We can make some educated guesses as to what the treaty requirements will be. We can accommodate in our design the constraints and configurations of U.S. items that might be tagged. It would be naive to believe that a system that we can design now will be applicable to every treaty without some modification.

Ethical Issues in the Scientific World

Marshall Thomsen

The scientific training of students in the typical university program focuses on developing knowledge and skills to solve scientific problems. Additionally, some students study the structure of science itself, addressing issues such as how a "scientific law" comes to be accepted as such. What is too often missing from this educational process is an examination of ethical issues related to scientific research. When such issues arise, they typically come up in the biological (particularly medical) sciences. There are, however, equally significant ethical issues for the physical scientist.

This paper discusses the Ethics in Physics course recently introduced at Eastern Michigan University. The course is intended to fill the void in the exposure of our majors to ethical issues, both reinforcing the students' existing ethical standards and broadening their perspective on the role of ethics in a scientific career. The course was not designed to present a list of ethical "do's" and "don'ts." Rather, by encouraging classroom discussion and open debate, it was hoped that the students would develop their ability and interest in making ethical decisions.

The ethics course was open to advanced undergraduate physics majors. Nine enrolled in this initial offering, representing approximately half the number of students receiving physics bachelor's degrees in a typical year in our department.

We met one hour a week in a roundtable discussion. Students were expected to do assigned reading for each class and were graded on both attendance (exposure to the ethical issues) and participation in discussions (indicating deeper consideration of the issues). Two papers were assigned to allow students to interrelate some of the specific issues. To encourage further reading, and at the same time increase the range of sources available to me, I set aside ten percent of their grade for "research points." These were earned by their making available to me articles, books or other sources (one person loaned me a video) on ethical issues in science. At the end of the course, the students were given the choice between taking a standard essay-type final exam or participating in a panel discussion open to the university community.

Ethical issues were broken down into three major areas: one's own research, presentation of results to the scientific community, and interactions between the scientific community and the community at large. In discussing ethical implications of one's own research, we used several case studies involving weapons-related research. We focussed on the fact that there are often consequences associated with research and those consequences need to be examined for consistency with one's own moral principles. The issue may not always be clear-cut, as often the consequences may be both positive and negative, and unpredictable in any case.

Several ethical issues are related to presentation of research results to the scientific community. The most obvious and well publicized of these is faking or distorting data (1). Beyond that, however, is the issue of how results become published: who referees scientific papers, the responsibilities of the referees, and the responsibilities of the authors. For instance, one might suggest that all authors of a paper read and approve it prior to submission, but is this possible for a particle physics paper with 50 or more authors? Or one might suggest that referees carefully check all equations in the paper, but is this realistic given the complexity of some papers?

Finally, we discussed interactions between scientists and the community at large, focussing on the responsibility of scientists to accurately and fairly convey scientific information to the non-scientific public. For example, we discussed the role of Richard Feynman on the commission investigating the Challenger accident (2). It appears that when he has appointed to the commission, the expectation was that he would provide limited advice to the commission's staff but mostly rubber-stamp their conclusions. However, he felt that if he were going to sign the report as a scientist, he needed to be involved in the investigation as a scientist, rather than just lend a well-known signature to the report.

As the instructor, I not only helped broaden students' perspective on ethics, but also observed their reactions to the situations discussed. Obviously, caution needs to be exercised in drawing conclusions based on a sample of nine students who have elected to take a course in ethics. Arguably these students are the most likely to have already given serious thought to ethical issues. On the other hand, this non-technical course was very likely the easiest physics credit a physics major could earn and thus an attractive option for students otherwise not inclined to have an interest in this area.

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Hence the sample is probably less biased than might be supposed.

It was clear to me from class discussions that most students had given serious thought to at least some of the ethical issues raised. However, it was equally clear that all students had their ethical perspectives broadened. Nowhere was this more apparent than in the discussion of how the results of research reach print. While lack of ethics knowledge is not surprising given the way physics is taught, this must be changed if students are to see how the publication system has been abused, cautioning them against blind faith in the written word. A simple example of a published paper, one of whose major results was dimensionally incorrect, served to drive this point home, while also highlighting the responsibilities of both authors and referees.

A second observation was that none of the students appeared to focus on monetary aspects of their future careers. While no doubt the financial aspect of a job will be important to them, in most cases it appeared that this aspect would not compensate for a dull job or a job with no redeeming social value. Their feelings along these lines were sufficiently well developed that several of them felt insulted by one of our readings which implied that the goal of today's typical undergraduates is prestige and money (3).

Course evaluations at the end of the term revealed two areas that need to be worked on in future offerings of this course. One student pointed out, correctly, that little time was spent discussing how to deal with ethical breaches by colleagues. This is an important issue, since not to deal with an observed ethical breach may itself be considered unethical. The second problem is more difficult. It is difficult to draw the line between a discussion of the ethics of, say, weapons-related research, and a political discussion of the desirability of such research. Obviously, they are closely related. I tried to encourage the students to formulate a political opinion and see its potential connection to their own career, but at the same time to avoid expressing my own political opinion, since my object was not to change their political perspective. The result was that during some discussions the students were not clear how germane their political debates were to our topic for the day. Having had this problem identified for me, I think it will be easier to clarify the intended direction of those discussions.

This course was successful in its objective of broadening student perspectives on ethical issues. By stimulating their interest, it is hoped they will be more likely to examine the broad range of ethical issues they may confront in their careers, to the benefit of the scientific community.

References

1. See, for instance William Broad and Nicholas Wade, *Betrayers of the Truth,* Simon and Schuster (1982).
2. R. P. Feynman, "What *Do You Care What Other People Think?"* W.W. Norton (1988).
3 C. Graney, letter to the editor, *Physics Today*, November1987, p.126.

REVIEW

Making Space Defense Work: Must the Superpowers Cooperate? by A. Fenner Milton, M. Scott Davis, and John Parmentola

Roosevelt Center for American Policy Studies, Pergamon-Brassey's International Defense Publishers, Washington, DC, and London, 1989, 207 pgs.

This is the latest in a long list of books analyzing the Strategic Defense Initiative (SDI). It differs from most in that it is more dispassionate and more accessible to the putatively intelligent layman than the great majority. Accessibility is one item that the Roosevelt Center strives for, since it is devoted to increasing public participation in the decision-making process. While the work may not quite live up to its claim of being the "most comprehensive unclassified assessment available today" of SDI technology (the reader is hereby informed that my objectivity may be suspect since I am a co-author of an earlier SDI assessment), it is definitely a useful contribution to the literature, and presents perhaps the most comprehensible although somewhat condensed summary of the main policy and technical issues.

The book describes in some detail the political genesis of strategic defense, beginning with the concept of deterrence and continuing through the expectations of the ABM Treaty of 1972. The buildup of warheads by the U.S. and the Soviet Union in the 1970's is outlined. The problems of extended deterrence, namely the nuclear protection of Western Europe, maintained through the threat of nuclear warfare against the Soviet Union, are then emphasized. The possible advantages of strategic defenses in maintaining this form of deterrence have provided one of the pillars of support for SDI proponents. However, there are also difficulties. The authors give an excellent and useful discussion of the issues.

However, in addition to the political benefits of strategic defenses, which are of course highly questionable, there is the matter of the technical feasibility.

Using unclassified sources and informants with classified access, the authors have produced an accurate and unclassified discussion of the most important technologies, including kinetic kill vehicles, lasers, neutral particle beams, and rail guns. Attendant problems are well enumerated. There is less on sensors and on battle management, computing, and communications requirements.

In a field as far-reaching as SDI, there are always differences and minor arguments that will surface among afficionados. Inevitably, I have some difficulties with a few of their points. I have three main complaints about the technical aspects of the book.

First, in the nth attempt to ascertain how many lasers are needed to cover the earth, directed energy weapons are characterized by a "maximum effective range," a meaningless concept, unless the necessary dwell time for a kill is longer than the amount of time the space platform is above the horizon. The resulting numbers and graph are therefore difficult to relate to the real world. More logical is to calculate the number of weapons needed given the brightness (a term that the authors find distasteful) in watts per steradian, the altitude, and the hardness, location, and size of the opposing ballistic missile fleet.

Second, in assessing the capability of space-based kinetic energy weapons, far too much credit is given for quickness of response. General Abrahamson himself is on record as stating that some 70 seconds would be needed for the response time of an early "Phase I" system. From the book, one gets the impression that responses would be much quicker. The difference is more than academic. A 70 second response time, when appropriate calculations are done, makes all but impractically low orbiting space-based interceptors useless against plausible future threats. They just can't get down in time.

Finally, although the authors do occasionally allude to the problem that anti-satellite weapons (ASATs) can make use of the same technologies as ballistic missile defenses, I do not get the impression that they fully appreciate the enormity of the problems this poses for stability of a bilateral missile defense regime. Not only are the technologies identical, but ASATs will be available first (as they already are) since the requirements are less stringent. This implies a great deal of difficulty in constructing a scenario wherein both sides would have effective space-based missile defense systems that are not vulnerable to each other. And if they are vulnerable to each other, extreme crisis instability is the result, as the side that strikes its opponent's defenses first could destroy them while retaining defenses of its own. Furthermore, and not noted by the authors, X-ray lasers would, if they existed, provide an extreme threat to the survivability of any space-based system.

Overall, the authors are skeptical that a space-based defense can be more than partially effective unless both sides cooperate in an agreed-upon transition to defense dominance from today's offense dominant world. I would agree, but it is not clear to me from their arguments that a stable transition is possible even with cooperation, unless there is a good bit of luck both in politics and in technology development. In fact the authors may not be so sure themeselves, but they are careful to refrain from being judgmental.

This purpose of the book is to lay issues before the public, and they finish by describing four policy options in a fair, calm way. The first two are similar, advocating little or no missile defense. The third would attempt to protect military targets but not populations. Anti-tactical ballistic missiles in the European theater would also be used in this approach. This would deter a first strike by creating uncertainty in the minds of Soviet planners. The authors talk of defending more than hardened targets in this approach. If this were possible, it should be similar to defending populations, which is the last, Reaganesque option. Virtually no one takes this one seriously any more, but the position is given its due.

My main cavil is with the title, which makes it appear that the writers are searching for a way to make the whole thing work. That's not what one finds in the book, which demonstrates frequent and, I feel, justified skepticism about the likelihood of solving the technical problems in the foreseeable future. My impression is that the title was written by a committee and does not well represent the contents.

Anthony Fainberg
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NEWS

Science & Global Security: A New Journal

The first issue of *Science & Global Security* is now available. The goal of this joint US-Soviet journal is to develop a common understanding among the scientific communities of East and West of the technical basis for new policy initiatives relating to arms control and global environmental issues. In pursuing this goal, the journal will encourage dialogue among scientists, encourage cooperative research, and draw upon existing research initiatives.

The journal is directed by a joint board of US and Soviet editors, co-chaired by Roald Sagdeev, chairman of the Committee of Soviet Scientists for Peace (CSS), and Frank von Hippel, chairman of the Federation of American Scientists research fund (FAS fund). Harold Feiveson, senior research policy scientists at Princeton University, is editor. The journal will be published as a quarterly in English by Gordon and Breach Science Publishers, and will also be published in a Russian edition.

The articles for the first issue are mostly drawn from work done within the "cooperative research project on arms reductions" organized in 1987 by the FAS fund and CSS. The remainder is devoted to space reactor arms control. Contents:

- Forewords, F. von Hippel and R. Sagdeev
- Verified elimination of warheads, T. Taylor
- Verification of limits on SLCMs, V. Thomas
- Report on a visit to Sary Shagan and Kyshtym
- Special section on space reactor arms control:
  - Overview, J. Primack, N. Abrams, et al
  - Military connection and environmental hazards, O. Prilutsky et al
  - Background on space nuclear power, S. Aftergood
  - Infrared monitoring of nuclear power in space, D. Hafemeister
  - Detection by gamma and positron emissions, J. Primack et al
  - Civilian uses of reactors in space, R. Rosen & A. Schnyer

Future issues will include:

- Detecting nuclear warheads, S. Fetter et al
- Verifying reductions in MIRV multiplicity, R. Mozley
- Performance limitations on AMS systems, J. Pike
- Verification of laser brightness limitations, R. Ringo et al
- Cooperative seismic verification project, C. Archambeau et al
- Nuclear test bans and 3rd-generation weapons, D. Fenstermacher
- Establishment of a moon base, A. Gurshtein et al
- Hydrogen from photovoltaics for coping with global warming, J. Ogen et al

For further information, contact Harold A. Feiveson, Center for Energy & Environmental Studies, Princeton University, Princeton, NJ 08544, (609)452-4676.

Forum's Missile Study Published

The Forum's second published study, *The Future of Land-Based Strategic Missiles*, edited by Barbara G. Levi, Mark Sakitt, and Art Hobson (287 pages, American Institute of Physics, 1989, clothbound, $28 for AIP or APS members, $35 nonmembers), was released at the recent Baltimore APS meeting. Order a copy by writing to: American Institute of Physics, c/o American International Distribution Corp., 64 Depot Road, Colchester, VT 05446, or by phoning toll-free to 1-800-445-6638.

The future of strategic land-based missiles has been under debate for many years, but there is still no clear choice for what is to succeed the current force of Minuteman missiles. The options include large multiple warhead missiles, small single warhead missiles, superhard silos, mobile trucks, railroad cars, as well as missile defense of silos, launch on warning, no changes in current deployments, and moving away from land-based missiles toward a bomber/submarine "diad."

To promote informed debate, the study group has collected background material on these and other options, and has evaluated each. The book gives a brief summary of study findings, historical background for the missile debate, a discussion of current strategic doctrine, brief evaluations of 10 options, and 12 research articles. It is designed for educators, interested non-specialists, and policy analysts.

For a more detailed description, see *Physics and Society*, April 1989, p. 12.

Promote Science and Society Awareness! Inform Others About the Forum

Distribute *Physics and Society* to others, either by loaning or photocopying your copy, or by writing to the editor (address on page 2) and requesting any number of copies, from a few for acquaintances to as many as 100 (if available) for distribution to physics departments or at meetings. If you distribute very many copies it would be helpful to make an announcement, or to enclose in each copy a note, encouraging Forum membership.

Urge others to join the Forum. *Physics and Society* is sent free to all Forum members, and Forum membership is free to APS members. To join the Forum, APS members need only indicate their desire to join on the annual APS membership renewal notice, by listing "Forum" on the front side of the notice as described under "renewal instructions." Alternatively, APS members can join the Forum by filling out the following statement of intent and mailing it either to the editor or directly to the APS:

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

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COMMENT

An Active Forum

The APS Forum on Physics and Society is your opportunity to become educated and involved in physics-related social topics. Our activities include:

APS sessions. In the past 4 years, the Forum has sponsored 35 sessions at APS meetings, plus 10 sessions at short courses. These sessions provide a timely forum for technical issues of national interest. For example, at the 1984 Plasma Physics meeting, we sponsored an SDI debate between Fletcher panelists and the Union of Concerned Scientists, several years before such formal studies as the APS Directed Energy Weapons study. Recent topics include acid rain, fusion power, ozone hole, DOD weapons reactors, mobile ICBMs, verification, nuclear winter, conventional weapons, Challenger disaster, CTB, Chernobyl, big/little physics, Nevada test site, born secret, renewable energy, solar cells, EMP, and on-site inspection technologies. It is an important principle of Forum sessions about these controversial issues that they should always include both points of view.

Physics and Society. Over the past 18 years, the Forum's newsletter/journal has been upgraded in content and style to include original research, letters, and regular book reviews as a 16-page quarterly. It has become a forum for expression of a wide range of views.

Short courses. The Forum has sponsored several courses at APS meetings:

•Nuclear arms and national security, San Francisco, 1982
•Nuclear arms and national security, Baltimore, 1983
•Energy, Baltimore, 1985
•Nuclear arms and national security, Baltimore, 1988

Books. The Forum has sponsored several books:

•*Physics Careers, Employment and Education*, Perl, AIP 1977, 340 pgs
•*Physics, Technology, and the Nuclear Arms Race*, Hafemeister/Schroeer, AIP 1983, 383 pgs

•*Energy Sources: Conservation and Renewables*, Hafemeister/ Levi/Kelly, AIP 1985, 682 pgs
•*Acid Rain:How Serious and What to Do*, Hafemeister, AAPT 1985, 56 pgs.
•*Civil Defense: A Choice of Disasters*, Dowling/Harrell, AIP 1987, 256 pgs
•*Nuclear Arms Technologies in the 1990s*, Schroeer/Hafemeister, AIP 1988, 484 pgs
•*The Future of Land-Based Strategic Missiles*, Levi/Sakitt/Hobson, AIP 1989, 287 pgs.

Forum studies. The Forum has sponsored 3 studies:
•Civil defenses, chaired by J. Dowling and E. Harrell 1984-86 (published)
•The future of land-based strategic missiles, chaired by B. Levi, 1985-88 (published)
•Energy, chaired by A. Fainberg and R. Howes, 1988-

The APS consists of research and/or teaching physicists. The Forum invites people to contribute their knowledge and their views to a balanced presentation of the technical and policy aspects of physics-related social issues. By applying physics principles and mathematical calculations to such issues, Forum sessions and publications fill an important niche in the discussion of public policy. Although APS studies organized by the Panel on Public Affairs (POPA) have set a very high standard for quality, such large and well-funded studies are necessarily limited in quantity. The Forum fills this gap by conducting smaller studies and sessions on a wide range of fairly specific topics.

Being a general interest group, the Forum bridges the gaps between the various specialized divisions of the APS. The Forum, along with the Division on the History of Physics and the International Physics Group (*Physics and Society*, July 1989, p. 15), has a supportive and unifying effect on our too-fractured profession, by cutting across the specialized divisional lines. Since the Forum does not directly represent the main professional interests of APS members, it needs the support of the society as a whole.

Here are a few things you can do: Write an article, letter, book review, or comment for this newsletter. Also send us newsworthy items. The editor's address is on page 2. Articles should be under 2000 words, reviews and comments under 1000, and letters under 500. Volunteer to organize a Forum-sponsored invited session, by contacting the Forum Chair or Vice-chair as listed in the April 1989 issue, page 14. Attend and participate in Forum-sponsored sessions and workshops at APS meetings. Make use of Forum or other information sources to include physics-related interdisciplinary topics in your teaching. Encourage others, especially your physics students, to join the Forum and to get involved in Forum activities.