PHYSICS AND SOCIETY

Volume 18, Number 4 **October 1989**

The American Physical Society
335 East 45th Street
New York, New York 10017

Physics and Society is the quarterly of the Forum on Physics and Society, a division of the American Physical Society. It is distributed free to members of the Forum and to physics libraries upon request. Nonmembers may receive *Physics and Society* free upon request by writing to the editor; voluntary contributions of $10 per year are welcome. Make checks payable to APS/Forum. *Physics and Society* presents letters and reviewed articles on the relations of physics and the physics community to government and society. It presents news of the Forum and provides a medium for Forum members to exchange ideas. Opinions expressed are those of the author only and do not necessarily reflect the views of the APS or of the Forum. Contributed articles, letters, etc. should be sent to the editor: Art Hobson, Physics Department, University of Arkansas, Fayetteville, AR 72701, (501) 575-5918.

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.

David Hafemeister
Physics Department
California Polytechnic State University
San Luis Obispo, CA 93407

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.

Bennet C. Karp
812 Wellington Place
Aberdeen, NJ 07747

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.

Louis A. P. Balazs
Physics Department
Purdue University
West Lafayette, ID 47907

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.

Igor Alexeff
Electrical and Computer Engineering
University of Tennessee
Knoxville, TN 37996-2100

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
110 Glenbrook Drive
Hendersonville, NC 28739

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 to 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, CO_2, 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