behavior that is selected for, learned, and transmitted from generation to generation. We have to break that cycle.

LAURA NADER
University of California, Berkeley
4/81 *Berkeley, California*

Future of nuclear energy

In what was a most informative article, I would like to take exception to one sentence of Alvin Weinberg's "The future of nuclear energy" (March, page 48). Comparing environmental concerns to the fear of witches, Weinberg writes: "Perhaps most striking was the *hysterical fear* (my emphasis) exhibited by Middletowners when the Nuclear Regulatory Commission proposed to vent 60 000 curies of Kr 85 from Three Mile Island; the maximum beta skin dose per person would have been 11 millirem, the whole body gamma dose 0.2 millirem."

I object to the phrase "hysterical fear exhibited by Middletowners." As a Middletowner, I resent not having the sense that my willingness to participate responsibly is being met seriously by government and industry officials. When the venting of the Kr 85 was proposed there were several alternatives suggested. The one the utility opted for was not the method many of Middletown residents wanted. Once again, many of us felt the same kind of sensation following the March 28 accident. As Thomas Pigford wrote in *Nuclear News* (March 1981, page 41): "Serious fright and trauma resulted from technical errors and public announcements based on these errors a few days after the accident."

In short, one of the real hazards of the TMI accident is to replace decisions made by an informed electorate with *faits accomplis* handed down by technocratic fixers. If Weinberg wishes to label this as "hysteria," so be it.

The way GPU Nuclear deals with this "hysteria" is clear from their "Newsline" (March 1981). At the time of the TMI accident they had only one public information specialist at the plant. Now, they have a Communications Division at TMI staffed by 30 people. From this, I conclude that the way GPU Nuclear deals with residents' concerns is not to fix the damaged plant, but to fix public opinion. Is Weinberg's label designed to fix us in a like manner?

JACOB L. SUSSKIND
The Pennsylvania State University
4/81 *Middletown, Pennsylvania*

•

I read with interest Weinberg's article. He references on two occasions, as a possible solution to presently perceived problems with nuclear energy, the introduction of a new type of "forgiving reactor." By chance is he referring to the HTGR?

HAROLD M. AGNEW
General Atomic Company
4/81 *San Diego, California*

•

One point Weinberg makes is that many of the somatic and delayed effects, and most of the genetic effects of radiation, result from rather small exposures to very large numbers of people. He notes that in the BEIR Committee, Radford argued that the linear dose-response curve for gamma rays is not conservative, while Rossi argued that the linear response overestimates the efforts and that only a quadratic response is consistent. I think you ought to point out to your readers the recently published book by T. D. Luckey, *Hormesis with Ionizing Radiation* (CRC Press, Inc., 1980), in which he compiles data related to effects of low levels of radiation.

These data show that in low doses of radiation, instead of being harmful as the linear curve predicts or nearly benign as the quadratic curve predicts, low levels of exposure to ionizing radiation are really stimulating and beneficial to life. Luckey in his conclusions states, "The argument that low doses give harmful effects in proportion to the dosage is invalid." These conclusions, when accepted, should markedly change the attitude toward the safety of nuclear reactors.

ROBERT M. BRUGGER
Research Reactor Facility
University of Missouri
3/81 *Columbia, Missouri*

•

Numerous authors have attempted to pursuade a reluctant public to accept nuclear power generation by demonstrating that the risks associated with fission reactors are of the same magnitude as many of civilization's commonly accepted accoutrements, such as automobiles, airplanes, cigarettes and hydroelectric dams. This procedure is known as "putting nuclear risks in perspective." Alvin Weinberg's lucid article on the future of nuclear energy is significant in that in it he recognizes that the proponents of nuclear power must address themselves not simply to the quantitative probabilities of harm, but to the more subtle aspects of risk *perception* if they are to find a "perspective" in which nuclear power can be seen as acceptable. His citation of Lundberg's observations on air transport are, in this regard, quite apt.

Unfortunately, Weinberg, like many before him, improperly laments the

continued on page 105

continued from page 15

unsophistication of the public, which "understands consequences [but] does not understand probabilities." Many people do, in fact, understand probabilities, but reject nuclear power nevertheless. They do so for many reasons, two of which Weinberg approaches but inadequately discusses.

First, Weinberg fails to distinguish between public risk and private risk; and he discusses public risk, whereas private risk is the relevant quantity. My probability of coming to grief in a certain kind of accident is not necessarily equal to the *a priori* probability assigned to me by the actuary who does not recognize personal idiosyncrasies. The automobile accident death rate is about 2×10^{-4}/person/year. My personal risk, however, is much lower than that because I drive fewer miles per year than the average person, and I drive more carefully. Someone even more prudent than I has an even lower risk. I can control my automotive risk; I am free to trade risk for benefit according to my own values. Nuclear power does not afford me that freedom: The public and private risks of nuclear disaster will be nearly equal in a country deriving a significant fraction of its power from fission reactors. I cannot reduce my risk exposure by not using electricity; I cannot escape proximity to a reactor when there are the 7500 of them that Weinberg mentions. Without at least a partly free market in risk, many people quite reasonably demand that the public risks be maintained at almost impossibly low levels.

The second point Weinberg fails to treat adequately is that although risk certainly is the product of probability and consequence, consequence is not one-dimensional: It cannot be specified by a single number such as deaths per event. One hundred deaths in one disaster each year does not impose the same costs (economic, emotional or whatever) as 100 deaths distributed over 100 accidents each year. A death from cancer in one particular individual does not impose the same cost as a death in a plane crash of another. All deaths—and even all dollars—are not created equal.

When we trade in risks and benefits—as we surely must—we trade not just in deaths and dollars, which are easily quantified, but in fear and hope, which are not. It does not follow that nuclear power is or *should be* desirable just because it yields more dollars in the GNP and costs fewer lives on the average than our other options.

As a final illustration of these points, let me offer a modest proposal. I will endow a chair in physics at Weinberg's favorite university if, in return, the country will permit my agent to commit with impunity approximately one brutal murder per year. A professor of physics is a valuable thing; the cost is minimal. The probability of dying at my agent's hands is only about 4×10^{-9} per year. Considering that the present murder rate is much higher than that already, the chair of physics is practically free.

This is not a far-fetched proposition. We need only to release from prison one murderer whom we guess will kill people at the appropriate rate, and divert the funds for his maintenance to Weinberg's favorite university. Just don't ask me to explain the arrangement to the victims' families.

Yorke Brown
4/81 *State University of New York at Binghamton*

The author comments: I can sympathize with Jacob Susskind's objection to my referring to Middletown's violent reaction, exposed on national television, as "hysterical fear." The Nuclear Regulatory Commission had approved the venting procedure, and had confirmed the estimate of 11 millirems as an upper limit. Officials of the Commission had moved their families to the environs of Three Mile Island as proof of their confidence in these estimates. Eleven millirems whole body dose is about what one receives from four transcontinental round-trips by air. I can therefore see no rational basis for the reaction of the Middletowners. The incident confirms my observation that the future of nuclear energy will remain in doubt unless the public acquires a better understanding of the hazards of extremely low levels of radiation.

In response to Harold Agnew, I purposely refrained from specifying which reactor is most forgiving. He and Peter Fortescue have made a persuasive case for High Temperature Gas-Cooled Reactors. Various ideas for more forgiving light water reactors have been put forward in recent years, most particularly by Anders Hannerz of ASEA in Sweden.

I was familiar with the evidence (some of it 30 years old) for a beneficial effect of low levels of ionizing radiation mentioned by Robert Brugger. I suspect it will be many years before the general community of radiation biology accepts this evidence at face value.

I am sorry that Professor Brown remains unpersuaded. The 7500 reactors I speak of would be distributed throughout the world; in the United States perhaps 1000 reactors would eventually be deployed on about 100 sites, if the siting policy I espouse were adopted. This allows plenty of room for those who wish to live far from reactors.

I agree with Professor Brown—that

risk is not simply measured by the product of probability times consequences. It is for this reason that I believe the maximum consequences of reactor accident, as well as the risk, must be adequately low. The evidence on iodine release from water reactor accidents suggests that consequences may already be overestimated.

As for Professor Brown's grisly suggestion, I don't understand why he confines his offer to an old nuke like me when proponents of any energy technology (including so-called benign technologies) can, if one applies the logic implied in Professor Brown's suggestion, be held responsible for the many thousands of random deaths each year caused by any technology that transforms energy from one form in another.

ALVIN M. WEINBERG
Institute of Energy Analysis
5/81 *Oak Ridge, Tennessee*

Relativity and field theory

Jacob Bekenstein's response (January, page 69) to a letter by Michael Brill and Winfield Salisbury shows significant misunderstandings of a theory proposed by me. The purpose of this letter is to clarify the misunderstandings and to point out that not only is the theory in agreement with all known experiments, but it has a compelling simplicity and inner consistency of its own which can compare with those of other existing theories of gravitation.

Consider two rooms, one in free space being pulled with acceleration g, the other resting on the surface of the earth where the gravitational acceleration is g. In the first room a piece of iron and an apple (let go from rest) hit the floor at the same time because the floor is going up toward the iron and the apple. In the second room the same piece of iron and the apple (again let go from rest) hit the floor at the same time because they are moving down with the same acceleration g. In this *kinematical* sense the new theory works the same way as any other theory of gravity where the equations of motion are independent of mass.

The difference, and in fact part of the motivation, of the new theory comes from the following *dynamical* question: Will the dents on the floors (caused by the iron) be also exactly the same? To see why the question arises, note that in the first room we would naturally think of the effect as depending only on potentials definable within the room (say, from the floor to a height), whereas, in the second room an overall potential due to the presence of the earth could cause a change in the energy (mass) of the iron. So in the second room there is the possibility that the mass m might become αm. The equations of motion would not be affected (they are independent of mass) but the dents on the floor would be different because now mass is αm instead of m. This possibility cannot be removed by appealing to tidal forces and so on, because the two rooms might be sitting on two identical planets one of which has an outer shell (enclosing the room), causing no change in g but contributing additional energy (mass) to the iron. Motions would be identical but energy–momenta could differ because of this extra potential. (Such a difference would also have a detectable quantum mechanical effect since, say, for a neutron, the phase difference between two heights would be $\alpha\delta$ instead of just δ.)

The new theory eliminates such distinctions by formulating the problem of dynamics so that: 1) The metric depends only on potential differences as $g_{\mu\nu}(\bar{\phi} - \bar{\phi}')$; that is, the metric displays a *group property* with respect to potentials. 2) The tensor potential $\bar{\phi} = \phi_{\mu}{}^{\nu}$ is a generalization of the Newtonian potential; it is symmetric and satisfies the covariant *d'Alembert equation* of the same metric, $\bar{\phi}'$ being its integration constants. 3) The group property is assumed *multiplicative* and the gravitational red-shift $\nu' = \nu e^{-(\phi - \phi')}$, where $\phi = -\frac{1}{2}\log(g_{00})$ is used as a *correspondence condition*. (The red-shift was recently tested,[1] in this form, to a high accuracy of 2.5×10^{-6}.) These conditions plus the relativistic requirement that all space–time variables must be treated *on equal footing* determines the form of the metric to be an exponential. (The usual theory satisfies a similar group property but only on g_{00}. One of the field equations, $R_0{}^0 = 0$, is exactly the general Laplace equation[2] of $\phi = -\frac{1}{2}\log(g_{00})$ in the static Schwarzchild metric. Note also that in a 1907 article[3] Einstein insisted "in all strictness" (his words) on the exponential character of time-dilation, $t' = t\exp(\gamma\xi/c^2)$, where $\gamma\xi/c^2$ is a special case of our $\phi - \phi'$.) The metric thus is

$$g_{\mu\nu} = (\bar{\eta} e^{2(\phi - 2\bar{\phi})})_{\mu\nu}$$

where $\bar{\eta}$ is the Lorentz metric and $\phi = \phi_{\mu}{}^{\mu}$, the trace.[4] (Note that in first order this exponential gives the linearized Einstein metric.) The static field corresponds to $\bar{\phi} \to \phi_0{}^0 = \phi(x, y, z)$, hence the static line element is

$$ds^2 = e^{-2\phi}dt^2 - e^{2\phi}(dx^2 + dy^2 + dz^2)$$

where $\phi = M/r$ is the solution of the Laplace equation of the same line-element. This line-element is known to be in agreement with all experiments having to do with a static field. Likewise when effects having to do with other components are considered (first