letters

continued from page 15

I hope that this proposal will be fully discussed by the physics community, and that it will be adopted in time for the first evaluation to take place in 1985.

BYRON C. HALL JR
Cincinnati, Ohio
1/83

Energy risk standards

In the PHYSICS TODAY debate on radioactive-waste disposal (December, page 37) R. O. Pohl observed that "the criterion for proper waste disposal must be a protection of future generations equal to that required for themselves by those who produce it." This seemingly reasonable criterion is unfortunately difficult to apply because of the disingenuousness of current attitudes toward protecting the public from ionizing radiation from different sources such as energy generation and energy conservation. It has become national policy to ignore the incremental public exposure to ionizing radiation attributable to energy conservation, while viewing the smaller plausible individual exposures from nuclear power plants, nuclear wastes, and even severe nuclear meltdowns, with great alarm.

R. L. Fleischer detailed the lack of attention to incremental public radiological exposures from energy conservation in his August guest comment (page 9). Since that time, both the magnitude of the problem and the determination of society to take no meaningful mitigative action have been further confirmed.[1] Pohl spoke of the radon decay product lung dose of 30 rem per year in a hypothetical future home embedded in an abandoned uranium tailing pile, but did not mention the 2- to 120-rems-per-year lung dose resulting from actually measured indoor radon levels reported at the 1982 APS Fall Meeting.[2] (It has now been repeatedly demonstrated that, while indoor radon levels depend on the highly variable radon source term, they can be further increased by energy-conserving home characteristics such as tightened construction and improperly designed heat reservoirs.) The bottom line is that incremental public exposures attributable to routine energy conservation measures in normal homes can far exceed standards for nuclear facilities such as waste repositories, and when the radon source term is high or when extensive reductions in air infiltration are made, the incremental doses can reach or exceed the levels associated with the Utah fallout litigation, or even from an uncontained nuclear meltdown.[3]

There is some possibility that utilities, fearing future litigation, will revise their present practice of encouraging and subsidizing potentially hazardous energy-conservation measures without warning the public and determining the existing radon levels. But it seems unlikely, and perhaps not even desirable, that indoor radon standards will every be based on an imputed incremental lifetime risk of less than about 1000 premature deaths per million people exposed. This, is my opinion, is far more meaningful indicator of the level of protection that the present generation is willing to provide for itself than the rhetoric of activist groups that demand levels of protection far beyond the capacity and willingness of society to provide on any uniform basis.

I hope that we will someday acknowledge the necessity of establishing tenable and consistent criteria for the protection of public health. Such criteria would remove the artificial barriers to providing the resources needed by this generation without doing injustice to generations of the future.

References

1. The Final Environmental Impact Statement of the New York State Energy Master Plan II (9 February 1982).
2. C. T. Hess "Radon concentration in Maine houses due to use of radon rich water," Bull. APS, October 1982, Abstract EC 3, page 877.
3. H. L. Beck, P. W. Krey "External radiation exposure of the population of Utah from Nevada Weapons Tests" Report of the APS study group on lightwater reactor safety, Rev. Mod. Phys. **47**, Suppl. 1, 1975, page S108.

HENRY HURWITZ JR
Schenectady, New York
12/82

THE AUTHOR REPLIES: While I, together with many scientists[1], share the concern expressed by Henry Hurwitz about the lack of a public policy on indoor air pollution, I disagree with his belief that once such a policy has been formulated for indoor radon concentration, disposal criteria for uranium mill tailings will be relaxed. If, as has been found by C. T. Hess[2], houses built on normal ground (that is, not on an uranium ore body) and with normal building material can experience high radon concentrations, how much higher must these radon concentrations be if these houses were built on highly porous mill-tailings sand containing the very high radon concentrations characteristic for uranium ore? (Note that the radiation dose I had quoted in my article, 30 rem/year, which Hurwitz repeats in his letter, was a theoretical estimate.) Hess was presented evidence that the high radon concentrations in the houses he studied resulted from ground water transport. As I had pointed out in my article (December, page 43), no estimates appear to exist for the role ground water plays in carrying radioactivity from mill tailings into houses. Hess' work clearly demonstrates the urgent need that this question be studied.

References

1. See, for example, J. D. Spengler, Bull. Am. Phys. Soc. Ser. II, **27**, No. 8 (I), (1982) page 876.
2. Reference 4 in letter by Hurwitz.

ROBERT O. POHL
Cornell University
Ithaca, New York
1/83

Critical review criticized

In his review of *The Cosmic Code* by H. R. Pagels (December, page 60), David Layzer strongly criticizes the book because of its "inaccuracies" and "lack of proper historical content." His criticism of the book consists totally in proving these two points. He fails to say anything positive about the book. In all fairness to the readers of PHYSICS TODAY, I believe an alternate viewpoint is needed.

David Layzer obviously understands the physics, philosophy and mathematics the book sets out to explain, but I wonder how well he understands the reader for whom the book is written. Trained physicists tend to forget the many struggles great scientists had in formulating their theories. Similar struggles are encountered by the intelligent man in grasping their ideas.

Some general notions initially must be developed in learning new theories. At that stage, everything is not clear; everything has not been spelled out. And, if too much time is spent on details (sometimes on mathematical precision), the ideas become obscured. The depicting of general ideas is the goal of my primary scientific explanation and this is what Heinz Pagels has sought to do in his book. He does it successfully.

But let us examine the "inaccuracies" that wrought the strong criticism of the book. They are underlined below, followed by David Layzer's comment and then by some refuting remarks.

▶ *Einstein's general theory of relativity is needed to resolve the twin paradox.* David Layzer states the general theory isn't needed. If the general theory isn't needed to resolve the twin paradox, then what theory is used during the accelerating and decelerating period of the twin? Einstein formulated the general theory to deal with accelerating systems. Is there a new way of dealing with this problem?

▶ *That gravity is the curvature of space and that the round-trip time for a light beam grazing the Sun is increased because "the beam has to bend slight-*

ly." Layzer prefers to state that gravity changes the geometry of space-time rather than space. How much more meaning would the reader achieve from stating that gravitation causes the space-time continuum to change than simply stating that gravitation leads to space warping?

▶ *That the binary pulsar's "loss of energy is revealed by the . . . slowing down [of] its orbital period.*" Layzer would replace that statement by: "the energy loss causes the orbital period to diminish and the orbital motion to speed up." I am sure Pagels would agree with Layzer's statement on the two-body problem as does everyone who knows its solution. For "to diminish the period," Pagels has used "to slow down the orbital period" which is more picturesque than the first expression. Unfortunately, it does lead to misunderstanding if one envisions a more imaginative picture than that called for in the simple "slowing down" statement.

▶ *Averaging over the microworld description . . . introduces the arrow of time.* Layzer states that it doesn't; averaging alone cannot introduce a distinction between the two directions of time. It is not clear what Layzer has in mind. Perhaps he is thinking of a system of molecular particles in thermal equilibrium that is photographed at discrete times. The photographs would show their positions at various times and the particles' average positions would remain the same for all photographs. It would be difficult to ascertain the time order of the photographs. However, if a system in thermal non-equilibrium (in which no external forces are in effect) is considered, as described in Pagels' book, then the time order of the photographs of the system can be deduced from the averaging process. (The entropy of the system would be increasing.)

▶ *That Wolfgang Pauli's exclusion principle states "that two electrons cannot sit on top of each other."* Layzer would amend this statement by stating "that no more than two electrons can sit on top of each other." I would amend both Pagels' and Layzer's statements as follows: "that two electrons with the same spins cannot sit on top of each other" and "that no more than two electrons with opposite spins can sit on top of each other." How important are the refined statements to the reader?

▶ Layzer states that Pagels seems to have confused the problem of defining a random sequence with the problem of recognizing one. Modern mathematicians have difficulty in defining fundamental mathematical entities such as points, lines and so on. Yet they readily work with them from an intuitive notion of these entities. Probability or randomness is one of these mathematical entities and Pagels states that. Pagels promotes an intuitive notion in the reader of what randomness is by descriptive examples. He does not confuse but realizes the problem of defining (or understanding) a random sequence is intimately connected to the problem of recognizing one.

▶ Layzer states that Niels Bohr's and Werner Heisenberg's philosophical interpretation of quantum theory is not completely given (thus lacking accuracy). A great deal of the Bohr–Heisenberg philosophical interpretation is given in terms of the uncertainty principle, the complementarity principle, and the discreteness of atomic changes in energy, momentum and displacement. Layzer considers that this is not enough—that something must be said about the Bohr–Heisenberg interpretation in terms of "operational" language. Where does one stop?

▶ Layzer says certain historical data are missing on gauge fields and on the relationship between symmetry and invariance. In writing a popular book on physics, more historical data can easily be given, but it will be difficult to satisfy the various critics on how much history should be included.

Pagels has written a fine book which should be of interest to all physicists and science enthusiasts who are interested in modern physics and the associated philosophical problems. Besides dealing with the quantum dilemma, he reports on the latest discoveries in particle physics, unified field theory and the origin of the universe. *The Cosmic Code* is beautifully written in a narrative style that should be the envy of all scientific writers of popular works. I believe Pagels' book should be well publicized to illustrate the value and progress of physics, so often known to only a few physicists. It is unfortunate that the reviewer of this book for PHYSICS TODAY has discouraged fellow physicists from reading and discussing the book, thus minimizing its importance. I hope I have convinced the readers of PHYSICS TODAY that the reported "inaccuracies" are somewhat pedantic or, at least, need further investigation. I believe everyone seriously interested in encouraging the development of physics should read Pagels' book and use it as a means to inform the general public of the value and the fascination of physics.

ALPHONSE J. SISTINO
Argonne National Laboratory
Argonne, Illinois
1/83

THE AUTHOR COMMENTS: I agree with Alphonse Sistino that clarity is more important than pedantic precision in a book on physics addressed to nonscientists. But accuracy is important, too, and each of the criticisms in my review was directed against an argument or an aspect of the narrative to which Pagels devoted a good deal of space. Let me reply briefly to Sistino's comments.

▶ A space-time diagram clearly exhibits the lack of symmetry between the experiences of Petra, the space traveler, and Paula, her sedentary twin. Petra's world line consists of two straight segments, AB and BC, corresponding to the two legs of her journey; Paula's world line is the straight segment AC. In Minkowski space-time the "length" of AC, which represents the duration of the journey as measured by Paula, is greater than the sum of the "lengths" of the segments AB and BC, which represents the duration of the journey as measured by Petra. One doesn't need to measure the duration of the accelerated part of the journey (a neighborhood of B) to see that it is irrelevant, provided the unaccelerated legs are long enough.

▶ In a first approximation (the Newtonian approximation), gravity is a warping of time; space remains flat. As to the cause of the radar time-delay, if the explanation given by Pagels were correct, the fractional delay would be of the order of the square of the angular deflection and thus impossible to measure by present or foreseeable techniques.

▶ If "to slow down the orbital period" means anything, it means to lengthen the period or to slow down the motion. I regret that Pagels missed an opportunity to explain (or at least state) the remarkable fact that a self-gravitating system grows hotter as it radiates away energy.

▶ I agree that the arrow of time is a feature of macroscopic descriptions but not of their underlying microscopic descriptions. But it doesn't follow that the averaging process introduces the arrow. Indeed it should be obvious that it can't, since the operation of averaging in no way distinguishes between the two directions of time. Most modern theories of irreversibility trace the arrow to a property of initial states.

▶ Pagels misstates the Pauli principle in the course of a discussion of the periodic table and chemical bonding. The reader who has learned in a high-school or college chemistry course that the covalent bond is produced by a pair of shared electrons could well be puzzled by Pagels' version of the principle.

▶ In twentieth-century mathematics, mathematical objects and predicates are defined implicitly by the axioms that mention them. Axioms do not tell us what points, lines, and randomness "really are." They tell us how we may use these concepts within a given axiomatic framework.

letters

▶ Pagels wrote that, according to the Copenhagen interpretation of quantum mechanics, "reality is partially created by the observer." I suggested in my review that this is not an accurate statement of the expressed views of Bohr and Heisenberg. I tried to characterize these views briefly but did not comment on them.

▶ It seems to me that even a brief history of an idea should not omit mention of its inventor.

In spite of my reservations about Pagels' book, I agree with Sistino that it is important for scientists to "inform the general public of the value and the fascination of physics." I commend Pagels for his efforts, and hope that the success of his book will inspire others to follow his example.

DAVID LAYZER
Harvard University
Cambridge, Massachusetts

1/83

Einstein's light postulate

I was pleased to see the translation of the speech given by Einstein in Japan in 1922 in the August issue (page 45). But it is rather doubtful that this brief speech "throws some light on the current controversy as to whether or not he [Einstein] was aware of the Michelson–Morley experiment, when he proposed the special theory of relativity in 1905," as the translator of the speech, Yoshimasa A. Ono, suggests. The speech is too brief and anecdotal in character to fulfill this function. But there are many careful studies of this topic. One of the new and more interesting is a paper written by Elie Zahar from the London school of Sir Karl Popper and Imre Lakatos, "Why did Einstein's Programme supersede Lorentz's?," which has been published in *Method and Appraisal in the Physical Sciences* (Cambridge U. P., 1976). Apart from methodological considerations, the paper is mainly devoted to very deep historical and logical analysis of the genesis of special relativity theory. The speech by Einstein presented in PHYSICS TODAY does not seem to contradict Zahar's conclusions.

Briefly, Zahar's point of view on the genesis of special relativity is that the formulation of 1905 was derived only from two postulates: (P1) Relativity Postulate and (P2) Light Postulate. The important role Einstein gave to (P2) inspired inductivists to regard the null result of the Michelson–Morley experiment as an axiom of special relativity. However, such a diagnosis disagrees both with logic and history. From the logical point of view, it is not possible to claim that an observational statement of the null result of the