Letters

Assessment of Fallout Hazards

J. Gordin Kaplan's very readable letter [*Science* **130**, 728 (1959)] raises some interesting points concerning possible effects of fallout Sr^{90}. Kaplan mentions our studies on high-radium water and bone tumors in the Midwest. There is a preliminary report on this by one of us [L. D. Marinelli, *Am. J. Roentgenol.* **80**, 729 (1958)]. It is now established that some hundreds of thousands of persons have subsisted on water exceeding the maximum permissible Ra^{226} level for large populations, and several thousand, on water with a Ra^{226} content above five times that level.

No areas of significantly increased prevalence of bone tumors related to this exposure have as yet been observed, or can be detected in vital statistics as reported over the past 15 years. Final results of our bone-tumor survey cannot, however, be reported for some time, since it requires careful checking of all cases to rule out such major artifacts as diagnostic errors and areas of occupational and medical overexposure which may affect determination of the true incidence of this rare disease.

We would like to call attention to what is apparently a serious error of fact. Kaplan has suggested "studying" data from Russian reports cited by Engström *et al.* [*Bone and Radiostrontium* (Wiley, New York, 1957), p. 133] which indicate that dogs injected with 0.0001 μc of Sr^{90} per gram developed osteosarcoma 3 years later. We have indeed made a study of this remarkable citation, and it appears that it embodies an error: the dosage given is too low by a factor of about 1000. Our own experience would indicate that this degree of tumor response is characteristic of dosages of about 0.1 μc of Sr^{90} per gram. This is corroborated by published Russian reports [*Summaries of Papers presented at the Conference on Remote Consequences of Injuries Caused by the Action of Ionizing Radiation* (State Medical Literature Press, Moscow, 1956)] and by more recent Russian reviews of the subject [N. A. Kraevsky, *Blastomogenic Effects of* Sr^{90} (Ministry of Health of the U.S.S.R., Moscow, 1958)]. In our own laboratory, injection of 0.01 μc of Sr^{90} per gram in three dogs has induced no tumors, nor in fact has it induced any visible changes in bone structure after 12 years.

The most likely source of this error is the paper of E. V. Erieskova (in the *Summaries* cited above), which describes bone sarcoma in dogs receiving radiothorium (Th^{228}) in dosages of 0.0001 μc/g. This agrees with data obtained by the University of Utah Atomic Energy Project. Radiothorium may be expected, on a basis of microcuries injected per gram, to be between 60 and 600 times as toxic as Sr^{90}. The former figure takes into account only total energy release within the bone; the latter includes consideration of the relative biological effectiveness of alpha particles and the fact that deposition is concentrated in areas of active cellular growth. Another possible source of error might lie in the fact that a translator not versed in the subject could confuse millicurie (мкюри) with microcurie (мккюри). In any case the citation seems to be apocryphal and erroneous.

While a number of other points might be made, one other seems particularly worthy of being brought to Kaplan's attention. The data cited, which indicate that concentrations in "hot spots" in bone and concentrations in other or average areas differ by a factor of 40, are derived from observations where there has been a single injection of isotope. This factor naturally disappears (or nearly disappears) where exposure occurs throughout a major proportion of the individual's life-time, including infancy, since Sr^{90} is laid down along with normally deposited calcium, in more or less proportional amounts. In this case, hot spots are not observed.

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J. Gordin Kaplan deplores the assessment of fallout danger issued by the General Advisory Committee of the Atomic Energy Commission. He urges the committee to consult a biologist, but his letter contains evidence of such serious deficiencies in his understanding of radioactivity that it does not encourage one to place confidence in the advice of the biologist. For example, he argues that radiation reaching the body from outside is largely irrelevant to the subject of potential dangers from fallout, and he uses the childish analogy of comparing throwing rubber balls at a person to a person's swallowing a ball.

He seems unaware of the fact that cosmic rays penetrate the body and leave a trail of ions all along the path they traverse, and that they penetrate many times more tissue than the weak beta particles from Sr^{90}. Consequently the cosmic-ray background is exceedingly pertinent to an assessment of the dangers from radioactivity, and the fact that the inhabitants of Denver do not show a higher incidence of leukemia and other cancers than the inhabitants of New York, although the Denverites are exposed to 60 percent more cosmic

radiation, is strong evidence supporting the argument that a linear proportionality does not exist between biologic injury and radioactivity in the range of natural intensities.

Kaplan also seems to be ignorant of the importance of K^{40}, which is naturally radioactive, emits a more energetic beta particle than Sr^{90}, and is present in the body in such quantities that the average adult body experiences about 200,000 K^{40} disintegrations per minute. It is responsible for about one-third of the total background radiation taken into consideration by the General Advisory Committee.

Kaplan further makes the mistake of equating 1000 r at microscopic hot spots with 1000 r of total-body radiation. This kind of large error in quantitative reasoning is not uncommon among biologists. Many students of aquatic ecology, for example, count the numbers of organisms per liter of water without estimating their body mass. Thus, they attach the same importance to a *Chlorella* cell, with a volume of 50 μ^3, that they attach to a *Ceratium* cell, with a volume of 100,000 μ^3.

Finally, Kaplan selects a single experiment, which purports to demonstrate that 1 μc of Sr^{90} caused bone cancers in dogs, and places so much confidence in it that he says this concentration is "known to cause cancer in dogs." He ignores the principle that experiments should be reproducible—that until several laboratories have verified these results we cannot say that 1 μc of Sr^{90} is "known to cause cancer in dogs."

Until we biologists display more wisdom in our interpretation of the hazards of radioactivity than is displayed in Kaplan's letter, we can hardly urge the General Advisory Committee to seek our advice.

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The first of the three points made by Brues, Auerbach, and Marinelli in their interesting comment concerns their studies on the relation between Ra^{226} in the water supply and in bone of human beings on the one hand and induction of bone cancer on the other. The statement of the General Advisory Committee of the Atomic Energy Commission about which I recommended caution was that "the amount of strontium-90 which has been found in food and water is less of a hazard than the amount of radium normally present in public drinking water supply in certain places in the United States."

Now, there is evidence that Sr^{90} in the food of human beings quickly approaches equilibrium with that in human bone (*1*). The increase in Sr^{90} concentration in human bone seems roughly to parallel that in the diet: from midyear 1955 to midyear 1957, the mean concentration of Sr^{90} in the diet (North America and Western Europe) has risen 1.8 times, and that in human bone has risen about 1.6 times during the same period (*2*). Kulp *et al.* have shown that it is possible to predict with considerable accuracy the skeletal levels of Sr^{90} from dietary concentrations (*2*, Fig 2). With respect to radium, the sparse data presently available do not seem to permit one to conclude that the situation is the same. Data cited by Marinelli (*3*, Fig. 1) show that the range of variation for the Ra^{226} skeletal burden in man is roughly 20 times less than the corresponding variation in the Ra^{226} concentration in drinking water. Indeed, within the range of 0.01 to 0.1 $\mu\mu$c of Ra^{226} per liter of drinking water there seemed to be no corresponding significant variation whatever in skeletal burden for this isotope. Further, Marinelli cites data showing that the mean skeletal burdens of Ra^{226} in the Illinois communities of Joliet, Aurora, and Elmhurst are about 15 times the mean skeletal burden in Chicago, but that the water supplies of the first three communities have about 150 times the concentration of Ra^{226} that the water supply of Chicago has.

Hence, I repeat that "we had better treat . . . with suspicion" all statements citing high radium and thorium levels in the human diet as reassuring evidence of the harmlessness of radiostrontium.

My letter concluded with an argument concerning the possible hazards from the Sr^{90} levels in human bone likely to result from nuclear-test explosions already completed. This argument included reference to Russian experiments on induction of osteosarcomata in dogs by low levels of Sr^{90}, which had been cited by Engström *et al.* (*4*). I am very grateful to Brues, Auerbach, and Marinelli for pointing out that the citation was erroneous; let me add, by the way, that I am considerably relieved, as well. I stated that I was putting forward the argument "in order to solicit a refutation"; this Brues and his colleagues have provided, and I should like therefore to withdraw the argument.

With respect to their final point, one should point out that the absence of "hot spots" in what Björnerstedt and Engström (*5*) call chronic conditions of Sr^{90} poisoning does not necessarily imply that this substance has a completely homogenous distribution in bone. Under these conditions, the Swed-

ish authors point out that "the Sr^{90} concentration may be expected to vary from the average by a factor of about 2 for the 15 year period and by successively larger factors for longer periods." As for the present situation, they state that "today the Sr^{90} contamination of the geosphere and the biosphere is steadily increasing. This corresponds to a situation with aspects that lie somewhere between those of acute and chronic Sr^{90} poisoning conditions. Children in the 0- to 5-year age group are examples of individuals with chronic poisoning conditions. Adults above 20 years of age are more likely to be examples of acute poisoning."

Detailed consideration of Verduin's letter is supererogatory, as it seems largely irrevelant to my own. However, his errors must not be allowed to pass unchallenged. In the recent summary analysis of hearings, the Joint Committee on Atomic Energy stated: "As in 1957, testimony at the 1959 hearings indicated that strontium-90 and cesium-137 are still considered to present the greatest hazard in worldwide fallout . . . But short-lived isotopes, such as Sr^{89}, I^{131}, Ba^{140}, Zr^{95}, and others, were described by several witnesses as worthy of more consideration or as being even potentially as hazardous as Sr^{90} and Cs^{137}" (*6*). My letter stated that "the principal dangers . . . to the human race from fallout stem from the decay of the radioactive fallout material after it has been taken into the body and incorporated within certain cells and tissues." This statement is correct; it is nonsense to bring up cosmic rays and naturally occurring K^{40} in this connection, as they obviously have nothing to do with the matter.

We might have been spared the little lecture on "aquatic ecology" had I stated that the 1000 rads was the approximate dose received by the lungs of the Joachimsthal miners (*7*), not by their whole bodies. I regret this careless error.

I think Verduin is quite wrong in saying that "the fact that the inhabitants of Denver do not show a higher incidence of leukemia . . . is strong evidence supporting the argument that a linear proportionality does not exist between biological injury and radioactivity in the range of natural intensities." This fatigued red herring, which Linus Pauling calls "the Denver argument" (*8*), ought finally to have been exorcized (if that is what one does to red herrings) by the report of Buck, "Population size required for investigating threshold dose in radiation-induced leukemia" (*9*). Buck states: "At an altitude of 6000 feet, the annual excess of cosmic radiation over that received at sea level is approximately 23 mr, or 1.5 r by age 65. I attempted to examine leukemia death rates by altitude in the United States, only to realize that it was

extremely unlikely that the effect of such a small dose, even if it existed, could be demonstrated as statistically significant with the sizes of populations available."

Finally, I must disagree with Verduin's quaint notion that any of my own supposed or, alas, real deficiencies of knowledge make it unwise or unnecessary for the General Advisory Committee to rely on the advice of biologists in preparing a statement about biology. An individual, such as Verduin or I or anyone else, should and must speak out freely at the command of his conscience, even at the risk of making a fool of himself. I stated, and I repeat, that it is presumptuous of an official committee, whose membership comprises not one biologist, to issue what purports to be a definitive statement on a crucial biological matter. This would be so even if the statement did not include the misleading material to which I have drawn attention.

Following my own advice, I conclude by repeating my urgent conviction that nuclear bomb tests must not be resumed.

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User's Need of Scientific Information

What is the use of devising highly expensive machinery for searching information, and spending a great amount of money on its development, if we have no clear picture of what the users require? Who ought to benefit from greater availability of information if not the user? For whom is literature produced if not for those who read and use it?

The Royal Society Conference in London in 1948, and still more strongly the International Conference on Scientific Information in Washington in 1958, clearly showed that the user must be made the central focus of all research in improvement of scientific information methods.

Apparently the user wants only those items that are of interest to him at that moment. That means selection—elimination of superfluous texts (for in-