Letters

Radiation Dosimetry

Eliot Marshall's articles on the dosimetry of radiation from the atomic bombs dropped on Hiroshima and Nagasaki (News and Comment, 22 May, p. 900; 19 June, p. 1364) are, in general, accurate in their appraisal of the present situation; but there are a few sources of confusion in them. On a personal note, contrary to Marshall's statement, I have long been a member of the Radiation Research Society; I did not "skip" the 31 May meeting of the society at which the dosimetry was discussed but was returning from Iceland, where I was U.S. representative at a World Health Organization meeting on health surveillance related to environmental pollution.

Also, it was not clear from Marshall's presentation why the new data support the view that estimates of cancer risk from low LET (linear energy transfer) radiation should be raised. It is not simply because the new evidence changes the total cancer risk per rad according to the linear hypothesis, although this effect would be expected for the new Nagasaki data. Jablon and Loewe and Mendelsohn (Letters, 3 July, p. 6) correctly point out that for the Hiroshima results the total dose changes little with the new evaluation, and Jablon takes me to task for suggesting in testimony presented before an Environmental Protection Agency hearing in March that the gamma-ray doses in both cities might have been overestimated. At the time I prepared that testimony I had only recently received copies of the original data and tables presented by Loewe and Mendelsohn in August 1980; subsequently they revised Hiroshima gamma-ray exposures upward (*1*). In telephone conversations I had with Loewe at that time, he pointed out the possibility that gamma-ray shielding factors for Japanese buildings might have been underestimated, and if Marcum's new evaluation of these factors proves to be correct, I believe my original statement may still be generally valid, although significant for the higher dose categories in Hiroshima only.

The new dosimetry suggests greater cancer risk for low LET radiation not only because the data for Nagasaki cancer incidence may show about twice the risk found previously, but more important because the linear hypothesis was not the basis for computing risk by the current version of the report of the Committee on the Biological Effects of Ionizing Radiations (BEIR III) (*2*). The discrepancy in results for cancer mortality between the two cities (not observed for total cancer incidence data) was one of the principal justifications of use in the BEIR III report of the linear-quadratic model for calculation of low-dose risks for low LET radiation from the Nagasaki results. Now, however, there is no reason not to combine the results for the two cities, and on that basis use of the linear extrapolation is strongly supported, especially by the cancer incidence data. As I have pointed out elsewhere (*3*), this leads to an increase of the BEIR III coefficients for total cancer mortality by a factor of about 2 for males and about 4 for females; use of cancer incidence for risk evaluation changes these factors to 4 and 7, respectively. Thus it is in correcting a *misinterpretation* of the Japanese results by the BEIR III report that the new dose information has the greatest significance.

Dobson and Straume (Letters, 3 July, p. 8) still are explaining the apparent difference in results for cancer mortality between the two cities as due to the almost negligible contribution of neutrons at Hiroshima; in fact with the new dosimetric data the cancer *incidence* results in the two cities give remarkably similar slopes relating cancer excess to gamma-ray dose, when the old T65 dose categories are roughly corrected. I believe, however, that everyone involved in this controversy agrees that any conclusions about the Japanese results are premature until the individual doses are recalculated for each survivor in the light of the new findings and applied to results from more complete follow-ups of the study population.

Finally, I strongly support Jablon's comments that "this controversy is a compelling argument for bringing the data into the public arena. . . ." It is unfortunate that computational details concerning the T65 doses determined by the Oak Ridge group were not adequately published in the open literature; had they been, some of the errors in the T65 estimates might have come to light sooner. It is, to say the least, regrettable to learn now, 36 years after the events and after more than $100 million has already been spent by U.S. taxpayers for follow-up study of the A-bomb survivors, that the dosimetry related to this study population will now have to be completely redone. Because of potential biases alluded to by Jablon associated with publicly stated positions on radiation risks being taken by many concerned with these dosimetric determinations, it would appear that an independent scientific panel should be appointed, possibly by the AAAS, to review the new dosimetric data as they are developed.

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References

1. W. E. Loewe and E. Mendelsohn, "Revised dose estimates at Hiroshima and Nagasaki" (UCRL 85446 preprint, 1 October 1980), available from Lawrence Livermore National Laboratory, Livermore, Calif.; *Health Phys.*, in press.
2. Committee on the Biological Effects of Ionizing Radiations, *Effects on Populations of Exposure to Low Levels of Ionizing Radiation* (National Academy of Sciences–National Research Council, Washington, D.C., 1980).
3. E. P. Radford, *Radiat. Res.* **84**, 369 (1980).

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In his articles of 22 May and 19 June, Marshall makes a valiant effort to convey a summary of the salient points in the long semidormant, but recently eruptive, issue of the radiation dosages, and the consequences thereof, to the Japanese atomic bomb survivor populations.

This undertaking was a difficult task, not only because of the large number of technical complexities involved but also because there are differing beliefs and differing interpretations among the many scientific investigators involved. . . .

Apart from small inaccuracies (such as the fact that I work as a consultant for R & D Associates, in turn under contract to the Defense Nuclear Agency, not Oak Ridge National Laboratory), I found the article in the 19 June issue to be reasonably accurate with two notable exceptions. I do not believe it likely at all that the Livermore free-in-air dose estimates will need revising, or are incorrect, as is implied by the bold-faced accord attributed to me. I believe that the accuracy of the Livermore calculations over the ranges of interest are ± 15 percent or 20 percent—a vast improvement over the T65D estimates (Oak Ridge has recently completed free-in-air dosage calculations that agree well with the Livermore results). Although small

refinements remain to be made, I do not believe they will have any further significant impact.

The free-in-air dose calculations are, however, only one link, albeit a very important one, in a chain of four links that determine the ultimate radiation dose to the exposed population. The other three links are the amount of radiation that escapes the weapon, the attenuation of the free-in-air radiation through houses or buildings that contained the subject population, and the further attenuation of the shielded radiation through the tissues of the body to the particular organ in question. It is the latter two links which I believe must be revised, not the Livermore free-in-air dosage results (the second link), before conclusions about the absolute toxicity of gamma rays can be reached.

Possibly my insufficiently clear articulation of the facts in the preceding paragraph was the cause of the incorrect statement attributed to me, which in turn had the unfortunate result of casting doubt on the accuracy of the Livermore calculations. Such result was in no way my intention nor did it jibe in any way with my beliefs.

The other important error is that this whole subject revolves around initial radiation effects, and not radioactive fallout as stated.

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The four letters under the general title "Radiation estimates" (3 July, p. 6) probably caused readers, in addition to myself, to wonder what has become of the traditional objectivity of scientists. Authors of these four letters seem to be searching diligently to show why the risk of exposure to low-level ionizing radiation is not greater than has been reported in the BEIR III report using the T65 dose estimates. Using the table of neutron and gamma-ray doses at 2 kilometers from the epicenter of the Hiroshima nuclear detonation given in the letter from Loewe and Mendelsohn, I arrive at the opposite conclusions from those stated or implied in the four letters. If we apply an RBE (relative biological effectiveness) of 20 to the neutron (n) absorbed dose (rads) to obtain the dose equivalent (rems) as suggested by the International Commission on Radiological Protection, and the correction of a gamma building transmission factor of 1/1.6 as given in the letter from Loewe and Mendelsohn, the dose equivalent at Hiroshima is less by a factor of 2 than that reported for T65D or used by BEIR III. Likewise, the dose equivalents at Nagasaki are less by a factor of 2 when one applies this 1/1.6 factor. Thus, it would seem the risk of cancer from low-level exposure may be greater than that given in BEIR III by at least a factor of 2. This conclusion in reference to the Hiroshima data depends critically on the use of $RBE_n = 20$ for total carcinogenesis by neutrons. Of course, no value for RBE_n can now be derived from the Japanese data because of the low neutron doses, but a vast amount of data from other studies of alpha and neutron exposures suggests that RBE_n probably is equal to about 20 and certainly not less than 10. An additional reason why the new data from Lawrence Livermore National Laboratory suggest a larger cancer risk than used in the BEIR-III report is the fact that the slope of the gamma-ray dose curve versus distance at Hiroshima is less than that assumed in T65D, so the control group used in the T65D and Atomic Bomb Casualty Commission studies probably received more dose than has heretofore been assumed.

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Komanoff's Projections

Colin Norman (News and Comment, 8 May, p. 652) apparently gives some credence to Charles Komanoff's latest projections of nuclear and coal power costs (*1*). In recent energy economics debates my adversaries admitted that they were basing their case on Norman's article alone and had never seen Komanoff's book itself. Studies by perennial antinuclear activists call for more than simple reporting, even if it takes a couple of weeks to obtain a critical review.

As in previous writings, Komanoff uses questionable assumptions and selects a proxy variable that projects the answer he wants. A few years ago it was unit size: he said then that the larger a generating unit was, the worse it would run (*2*). He still claims this is valid, but a look at the data shows it is not. This time he chooses "sector size," the total installed capacity, nuclear or coal. By fitting the publicly available data with these proxy variables and extrapolating into the future the nuclear numbers project upward faster than those for coal.

The point is that none of these Komanoff-type analyses deal with the real problem. We will need more nuclear plants and more coal plants to meet the legitimate demand for electricity in the United States. We would require many new plants even if we did not have to build to replace obsolete units and substitute for oil and natural gas. Whatever we build will be very expensive and will be paid for with badly inflated dollars. Long lead times mean even more uncertainty. Who can predict the regulatory climate for coal or nuclear? The costs will rise further, and the biggest factors will continue to be delays, escalation, and inflation.

Utilities need diversity. They need the option to choose between fuel types. Nobody builds the "average" plant; decisions of such magnitude have many unique factors.

The political climate has effectively removed nuclear power from the marketplace for the past few years. However, those utilities that chose to build nuclear plants years ago and have those plants in operation today (other than Three Mile Island) are continuing to achieve substantial savings for their customers. For example, Commonwealth Edison's six large nuclear units (*3*) averaged 1.7 cents per kilowatt-hour in 1980; our six large coal units 3.6 cents per kilowatt-hour. If one arbitrarily set the capacity factors for all the units at 60 percent and doubled the carrying charges (in answer to the argument that four of the six nuclear units were turn-key jobs), coal costs of 3.4 cents per kilowatt-hour would still exceed those for nuclear of 2.6 cents per kilowatt-hour. Carrying charges for the LaSalle 1 plant, when it comes on-line in 1982, will be about 4 cents per kilowatt-hour, but the cost of fuel oil alone for the 9 percent of Edison's 1980 energy that came from oil (some of which LaSalle will displace) averaged 6 cents per kilowatt-hour. Estimates for new coal plants with scrubbers point toward even higher costs.

The United States and the rest of the world will need expensive new coal plants and nuclear plants as well. Claims that statistical projections should preclude the use of either technology call for healthy skepticism.

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References

1. C. Komanoff, *Power Plant Cost Escalation* (Komanoff Energy Associates, New York, 1981).
2. _____, *Power Plant Performance* (Council on Economic Priorities, New York, 1977).
3. A. D. Rossin and T. A. Rieck, *Science* **201**, 582 (1978).

Erratum: In the Research News item on evoked potentials entitled "Testing babies for neurological problems" (17 July, p. 322), the affiliation of Ivan Bodis-Wollner was incorrectly given as Albert Einstein College of Medicine. It should have been Mount Sinai School of Medicine.