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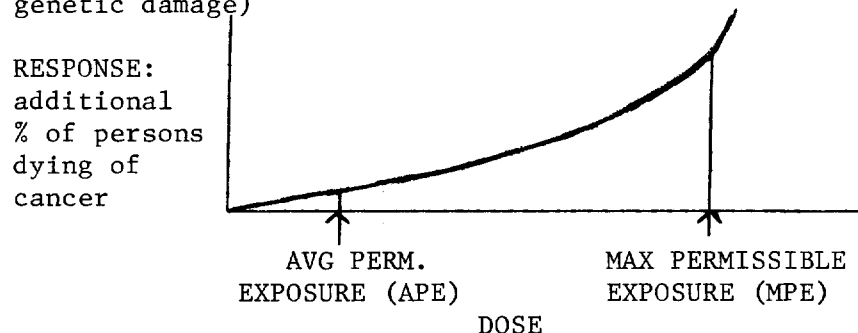
Professor Joshua Lederberg
Professor of Genetics
Stanford University School
of Medicine
Stanford, California

Dear Professor Lederberg:

Your newspaper piece on the hazards of (additional) atomic radiation (Washington Post, May 2, 1971) was very interesting appearing as it did three days after a seminar at the annual meeting of the American Physical Society on the Biologic Effects of Atomic Radiation. John Gofman spoke there, as did Leonard Hamilton of the Brookhaven Laboratory.

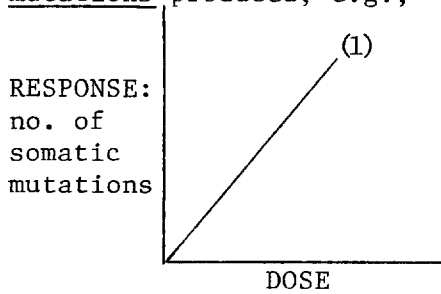
I'd like to talk about some issues that your piece brings to mind (which represent my thoughts, and those of a few colleagues - and not necessarily those of the National Cancer Institute). First, I have found many people, scientific and lay, upset over computations of risk based on "average" exposure. One of my colleagues points out the importance of the distribution of doses among the population through an extreme example: say we want to minimize the risk to the total population for a given average exposure in the population. If one individual received the total radiation necessary to give this defined average, then the risk to the total population would be a minimum. This one individual would (very likely) die, and no one else would be at any risk at all.

Any meaning that working with average exposures would have is also dependent on the linear, no-threshold model of radiation dose-response. For example, if the dose-response curve (for the effects of added radiation looked like this: (I use cancer as the response, as an example, and not intending to slight your concerns about genetic damage)

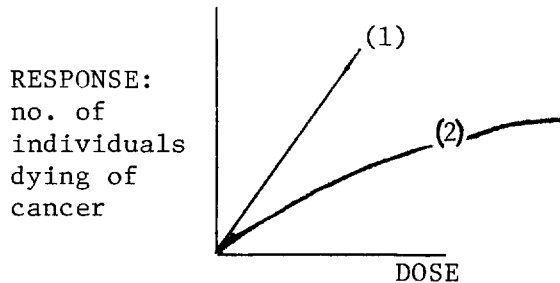


then the people at dose MPE would be at considerably more than proportionally higher risk (in proportion to their dose) than the people at dose APE.

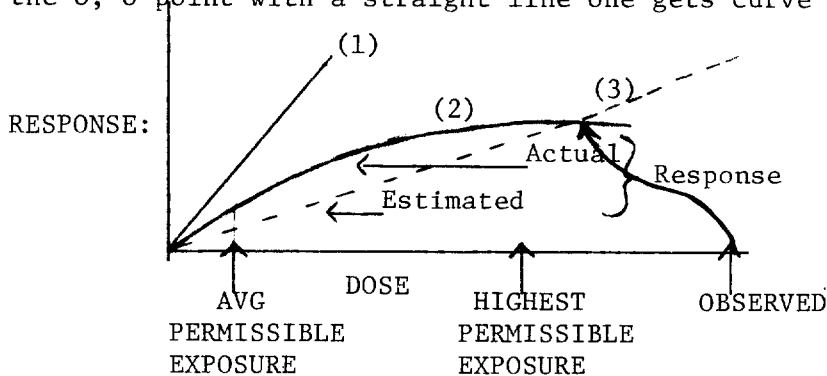
There is another way of under-estimating risk, even if the "true" dose-response curve were linear, no-threshold. Say there really is a linear, no-threshold dose-response in the number of somatic mutations produced; e.g.,



Even if all these somatic mutations were to lead to cancer, since a person can die of cancer only once and since some persons would have more than one somatic mutation, and some would die of other causes before the cancer killed we would observe a non-linear dose-response for cancer deaths; e.g.,

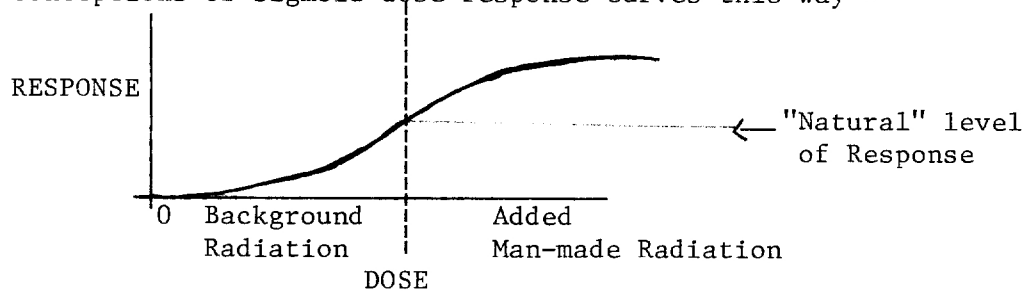


The data in curve (2) are the kind of data that is normally observed and usually only at high doses. If one now takes responses (cancer deaths) at some high doses (curve (2)) and extrapolates back to the o, o point with a straight line one gets curve (3); e.g.,



Now estimating risk from average exposure (using curve 3) would generally underestimate total risk to the population and the greatest underestimates are at the lowest doses. I think the most recent publication from the ABCC on Hiroshima-Nagasaki (25-69, Ishimaru, et al) possibly the uranium miner data (Lundin, et al - not yet published) and some of Miriam Finkel's recent animal data are in line with a dose-response curve like (2).

You are certainly correct in saying that the battle over the legitimacy of the 170 millirad standard* may be the wrong battle. Hamilton suggests that no more than 5 millirem of this 170 need be set aside for atomic power production. If the Seaborg predictions, or the Hamilton allowances are correct then obviously one does not need to have levels anywhere nearly as high as 170 millirems (for any practical purposes). But no matter what the average levels are, or are set at, the greatest risk is to the persons with greatest exposure and these are the people who need to be protected. If the risks of added radiation are not linear - and conceptually I think we could fit this idea in with our usual conceptions of sigmoid dose-response curves this way -



and for the reasons I cited above for being concerned about the distribution of radiation, I think we have to concern ourselves more about the high exposures. I think most people would accept the low costs inherent in Hamilton's maximum of 5 millirems, if they also believed that very great precautions (and always improving ones) were being taken to prevent an atomic Takoma Narrows Bridge accident. Most people go along with Karl Morgan's suggestion that atomic power plants be located outside densely populated areas. I think I may also be saying here that because of fears of accidents

* My preliminary computations have Gofman too high by a factor of 10 even accepting all his assumptions. One of my associates is now going through detailed actuarial computations to check me (and Gofman) out.

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the arguments based on average exposure are emotionally unsatisfying as well as possibly scientifically wrong.

Perhaps this is all academic. We have excluded medical radiation. Why? As the AMA has recently said rather strongly in an editorial, physicians are using diagnostic radiation far too promiscuously. This radiation increases the total exposure very much in a very small number of persons - often not for the patient's direct benefit. Perhaps our discussions of reducing radiation should concentrate more on reducing unnecessary diagnostic radiation. Is this another reason for saying that the 170 millirem battle is the wrong battle?

Very truly yours,

A handwritten signature in cursive script that reads "Marvin A. Schneiderman".

Marvin A. Schneiderman, Ph.D.
Associate Scientific Director
for Demography (Acting)
National Cancer Institute