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December 2, 1980

Bernard J. Snyder, Program Director  
Three Mile Island Program Office  
Office of Nuclear Reactor Regulation  
U.S. Nuclear Regulatory Commission  
Washington, D.C. 20555

Dear Dr. ~~Snyder~~ <sup>Bornie</sup> Snyder:

This is in response to your letter of November 21, 1980 to John E. Minnich regarding my questioning the error limits associated with potential health effects indicated in the draft PEIS on TMI-2.

First, let me address your comments in the third paragraph of your letter where in closing you state:

For this reason, although it may be desirable to quantify the risk estimates in the PEIS with error limits, it is not possible to do so in a manner which implies statistical accuracy.

I trust you recognize that it is your PEIS that quotes fatal cancer to three significant figures - "131 fatal cancers in the exposed workers per one million person-rem" (PEIS, p. 10-8) - with no uncertainty limits or discussion of uncertainties. Your reference to the BEIR I discussion of uncertainties is supportive of my argument that you should present in the PEIS the range of risk estimates found in the BEIR reports and other credible references. With respect to your discussion in paragraph three of the "order of risk," this is just a lot of gobbledegook that adds more confusion than enlightenment.

Turning to your fourth paragraph, you misread BEIR I. Actually you just didn't read far enough. Your reference to 1350-3300 deaths from cancer during the 25 years following irradiation due to exposure of the U.S. population to 0.1 rem during one year is taken from BEIR I, p. 89, and refers to extrapolation of the A-bomb survivor data. Had you read on to the next page, you would have observed that BEIR I, when taking into consideration the short followup of the A-bomb survivors, other data, and uncertainties, estimated the range of cancer deaths from 2000 to 9000. It is this larger range that is also reflected in the 3000 to 15000 cancer deaths annually due to an additional 5 rem per 30 years to the U.S. population that appears on page 2 of BEIR I.

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If you look on page 170 of BEIR I, you will find the excess cancer deaths for individuals exposed from 20 to 65 years of age ranges from  $81+300=381$  (absolute risk model) to  $181+746=927$  (relative risk model) due to  $5 \times 10^6$  man rem. Thus, the range is 76 to 185 per  $10^6$  man rem. The mean value of these is your value of 131. Thus, you are incorrect to state in your letter that:

The risk factor in the PEIS is, therefore, in the high range of the risk estimates given in BEIR-I and should be viewed as a reasonable upper-limit estimate.

You are quoting a mid-value that is 40 percent lower than the higher BEIR I relative risk model prediction.

Turning next to your last paragraph, I would be interested to know where in BEIR III there is reference to your statements "(although zero risk can not be excluded by the data)" and "(or even zero risk)." This is inconsistent with the BEIR III statement on page 4 that "the Committee [majority] believes that the quadratic model may be used to define the lower limits of risk from such radiation," Also on p. 140, BEIR III states:

[T]he Committee believes that a distinct carcinogenic effect could be discernible for the large doses [0.5-5 rem/year] that may be associated with lifetime occupational exposure.

I am surprised if the Staff is still applying the old threshold argument to carcinogenic effects. I thought this was put to rest years ago, and I also thought it was NRC (and EPA) policy not to consider the threshold model as a basis for regulatory decisions.

Please provide a page reference (in BEIR III) to the 10 to 500 per million person rem estimate. I assume it is from Table V-2, p. 145 (or Table V-23, p. 210). You recognize this is for U.S. population and not ages 20-65.

In reporting "conservatisms," or actually "lack of conservatism," I trust you will discuss the fact that the Subcommittee on Somatic Effects of BEIR III disagreed with the full BEIR III Committee recommendation regarding the use of the linear vs. linear quadratic model (see Radford's statement, beginning p. 227).

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I also trust you will indicate that a number of competent scientists, including Mancuso, Stewart, Kneale, Morgan, and Tamplin, believe the BEIR III Committee put too much weight on the A-bomb survivor data relative to the Hanford worker data; the latter being consistent with somatic risks as much as 10 to 20 times greater than the BEIR estimates. (Kneale, Mancuso, Stewart, "A Cohort Study of Risks from Radiation to Workers at Hanford;" Accepted for Publication by the British Journal of Industrial Medicine, 1980.)

Would you please explain the basis for your 260 genetic effects among the offspring of the work force per one million person rem (PEIS, p. 10-8). The BEIR I (p. 51) estimate is between 300 and 7500 genetic effects per 5 rem per generation to a population of one million. (In addition, BEIR I estimates that the same exposure at equilibrium would eventually lead to an increase of between 0.5% and 5% in the ill health of the population.) This would imply 60 to 1500 genetic effects per  $10^6$  man rem, plus a high risk of ill health.

BEIR III (p. 7) gives a comparable estimate - 60-1100 serious genetic disorders per million liveborn offspring due to 1 rem per generation. Your 260 hardly seems conservative assuming that the average age of the work force is 30 (also the assumed average age at conception) and two children per worker.

In conclusion, I believe the upper limits on the health effects to workers from cleanup of TMI-2 as presented in Table 10.2-1 of the PEIS (p. 10-9) should be corrected to indicate:

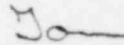
(a) two additional cancers based on BEIR I and III, or perhaps as many as 20 cancers if the Hanford worker studies by Mancuso, et al. (which are controversial) prove to be more accurate for low-dose exposure than extrapolation of the A-bomb survivor data from high to low dose. With regard to the Hanford worker data, a choice here of a factor of 10 increase over the BEIR estimate (e.g., as opposed to 20) is somewhat arbitrary but reflects the fact that (1) the estimates by Mancuso, et al., of the doubling dose (15 and 30 rads) were statistically significant only for certain radiosensitive cancers, and (b) the doubling dose confidence limits are very large.

(b) ~10-20 serious genetic effects and an increase in the ill health among offspring of the work force (based on BEIR I and III and the assumption that the average age of the work force is 30).

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If the Staff disagrees with these estimates, please let the TMI-2 Advisory Panel know as soon as possible, or at least in time to discuss this matter at either our December 18th or our December 30th meeting.

Sincerely,



Thomas B. Cochran

cc: Harold Denton  
TMI-2 Advisory Panel Members