WEST VIRGINIA UNIVERSITY
SCHOOL OF MEDICINE

TO: Duncan Thomas, Ph. D.

FROM: Alan Ducatman, MD

DATE: February 22, 1995

I am sorry I could not respond earlier to the R. Neuman memo of 6 Aug 1994 to E. W. Webster which you faxed earlier. Our funded mission needed urgent attention. Here are my comments.

First, I agree emphatically with Dr. Neumann that there is no convincing evidence of excess cancer in the exposed population. There is also no good evidence for the null hypothesis. To be really clear, there is just no good study at this dose. That is why we do studies, especially at physiologically plausible exposure levels such as this one.

Second, I think it a bit irrelevant to expend effort shooting down Stu Farber's strawmen. It must be clear by now that Stu is not an epidemiologist. The issue I not whether or why Stu thinks the Sandler study is definitive, for example. The issue is whether there are interesting and unanswered questions raised by available scanty data, including the Sandler study.

The discussion of more people versus larger dose (Part B, paragraph 2)is baffling in the context of not wanting to do a study. Does a population study of 2500 at a third of the relevant dose provide an answer? More importantly, a study of >50,000 at the dose of interest would be enough.

There is some obfuscation concerning cancer excess. Meningiomas are among the excess cancers now reported in A bomb survivors. I am not an expert in these data, and do not know what other head/neck outcomes may be important in Japan. This is not really the issue, however. The mode and size of local exposure dose is not exactly parallel in the comparison between A bomb survivors and nasopharyngeal radium patients. I am afraid that this is yet another issue where the focus on Farber statements has got in the way of sound population study design thinking.

The penultimate paragraph in the memo states that basic effects of radiation on human subjects were well understood in the 1940's. I leave the significance of this belief to you, but suggest that wide distribution of this statement would be inappropriate for your needs.

Finally, I agree emphatically with Neumann's conclusion that Farber's projections are naive. (After Farber made them in the Senate hearing, I corrected the impression immediately. This is recorded on C-Span.) None of this detracts from the clear need for a study, which is the essential point about which Farber is right. I suspect and hope that support for a study is the point of Neuman's memo to E. W. Webster.

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