3/9/81

RELATED CORRESPONDENCE

UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

BEFORE THE ATOMIC SAFETY AND LICENSING BOARD

In the Matter of	)	
	)	
HOUSTON LIGHTING & POWER COMPANY	)	Docket No. 50-466
	)	
(Allens Creek Nuclear Generating Station, Unit No. 1)	)	
	)	

AFFIDAVIT OF LEONARD D. HAMILTON IN RESPONSE TO

AFFIDAVIT OF IRWIN D. J. BROSS ENTITLED

"A 1981 REASSESSMENT OF THE HEALTH HAZARDS

OF LOW-LEVEL IONIZING RADIATION"

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Leonard D. Hamilton, being first duly sworn, deposes and says:

I am the same Leonard D. Hamilton that testified in this hearing on February 23 and 24, 1981 and submitted an affidavit on December 18, 1980 concerning the health effects of low level radiation. My qualifications are set forth in my testimony.

I.

I have reviewed the 1981 Reassessment of the Health Hazards of Low-Level Ionizing Radiation by Irwin D. J. Bross, October 9, 1980 -- a draft article which Intervenors have submitted "to show that material issues of fact exist which require litigation of this condition [sic] and denial of the motion for summary disposition." I conclude from my review of that article that no facts have been adduced which would lead me to modify the conclusions reached in my affidavit of December 18, 1980.

The Bross affidavit raises three areas that, in my view, must be addressed to refute thoroughly his theory. First, Dr. Bross claims that various recent data, which he fails to discuss in any meaningful detail, demonstrate that the health effects of low-level ionizing radiation have been understated in UNSCEAR, BEIR III and

by the radiation protection community. I will show that Dr. Bross has no true facts upon which to base his assertion, and that the scientific community has soundly refuted the claims which he and a small number of other individuals have made. Second, Bross claims that the linear hypothesis, which the vast majority of the knowledgeable scientific community now accepts, understates the risk from low-level radiation. I will show that Dr. Bross' analysis of the three theories of low-level radiation risks is factually and theoretically flawed and demonstrates that Bross has a very limited comprehension of the theories he discusses. Third, Dr. Bross claims that data derived from the tri-state survey show that the linear hypothesis greatly understates the risks of contracting leukemia from low-level radiation. I will show that in this affidavit Bross has completely misrepresented the data he has used and that, in fact, these data do not support his conclusion.

II.

Irwin D. J. Bross is one of the authors to whom I referred in my December 18 affidavit whose reports have been interpreted by some people to indicate that the commonly employed risk estimates, which are based on UNSCEAR (1977) and BEIR Committee Reports (1972 and 1980) underestimate the risk of radiation at all levels. Bross is among the very few who emphasize that the linear theory (which states that the risk per unit dose as derived from available data at high levels of radiation dose holds all the way down to zero exposure dose) is not sufficiently conservative in estimating risk at low doses but rather underestimates it.

Although he does not discuss any studies in detail,*/ Bross claims that a number of them support his conclusion that the health effects of low-level radiation have been underestimated. However, my affidavit concerning the health effects of low-level radiation reviews the weaknesses of Bross' own papers (Affidavit pp. 4-6), the uncertainties in the Mancuso, Stewart and Kneale data on the Hanford workers (Affidavit pp. 7-8), and the fatal flaws in the Najarian and Colton studies on the Portsmouth Naval Shipyard (Affidavit pp. 8-13).

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*/ Other than his faulty reanalysis of his own tri-state study results, which I discuss in Section IV below.

Dr. Bross apparently faults the Interagency Task Force on the Health Affects of Ionizing Radiation, commonly called the Libassi Report, (his ref. 2) for omitting reports on some studies and especially for neglecting the work of Dr. Ernest Sternglass. As Dr. Bross admits, however, these studies were "disparaged, and then disregarded" in the Libassi Report (Affidavit, p. 9).

Bross's answer is that, although individual studies may be subject to criticism, the combined weight of these studies validates his theory that health effects are higher than conventional estimates such as BEIR-III. More precisely, Bross asserts (pp. 10-11) that:

> Although it is relatively easy to fault the positive findings of each study separately and difficult to argue that any one study is conclusive, with so many positive studies it is now necessary for critics to deal with the cumulative evidence.

His claim that well-known statistical principles can be applied to assess their combined weight is flawed. One can only combine data from like kinds of studies; one cannot, as Dr. Bross does, mix prospective with retrospective data. It is a truism that as one increases the number of studies, if they are effectively replicates without any bias in selection, the power of the analysis is going to increase, but Bross's method of combining data does not make up for the

questionable significance of the individual studies.

Bross claims that he has now overcome the problem of sample size, but sample size still remains a major issue. In this analysis, he looks only at the positive studies; he should also have included negative studies. By ignoring all the negative studies, Bross has limited his universe and, therefore, skewed his results greatly.

Precision is measured by the standard error, and Dr. Bross here develops a method that appears to improve precison. But this appearance is only real so long as there is no systematic bias in the methodology. The "bundle of frail reeds" argument is correct when the individual reeds suffer only from random error. Flipping a coin three times does not give a good estimate of the head/tails probability; combining 20 sets of three flips each does. Combining experiments suffering from systematic error does not make the whole collection any better. To the extent that the "20 studies" are "fragile" on systematic grounds, the criticisms cannot be waived aside by combination of results as if there were only random error problems. To do so is bad science.

The work of Dr. Ernest Sternglass, like that of Dr. Bross, has been widely criticized in the scientific community and the drafters of the Libassi Report were certainly

justified in not giving that work any weight in their conclusions. Except for his original article in Science in 1963 (which has since refused to publish any of his submissions as lacking scientific merit), all Sternglass's references are to his own presentations made at meetings or in unreferenced publications, so that their citation bears no relation to their scientific merit. His arguments have been shown to depend on select evidence which supports his case while ignoring the evidence that does not.

Other scientists whom he has cited in support of his hypotheses have disavowed his interpretation of their findings. For instance, Dr Alice Stewart, on whose work Dr. Sternglass has allegedly relied, (e.g., Infant Mortality and Nuclear Power Generation by E. J. Sternglass dated October 18, 1970, and Infant Mortality Changes following the Three Mile Island Accident by E. J. Sternglass, presented at the 5th World Congress of Engineers & Architects, Tel-Aviv, Israel, January 25 1980) has devastingly criticized Dr. Sternglass. In testimony at the hearings on the Long Island Lighting Company (Shoreham Nuclear Power Station Unit No. 1), Docket No. 50-322, Transcript pages 7483-7484 (March 15, 1971), Dr. Stewart stated:

> I was once asked to review one of his (Dr. Sternglass) rather repetitive articles in the New Scientist and I pointed out that I

> think it does in fact render his approach null and void. He is a physicist and he has done what many people have done before, they have assumed it is quite easy to be an epidemeologist, and they have fallen into one of the more obvious traps. He is not the first person to have done it.
>
> Roughly speaking his evidence is postulating, if I can put it very frivolously, that by 1971 children will be rising from the grave into which they had fallen as a result of infant mortality.

She continued (Tr. 7540-41a); "First of all it [Dr. Sternglass's description of Dr. Stewart's work] is riddled with mistakes . . . It is just about his usual exaggeration."

These quotations demonstrate the low regard which the scientific community has for the work of Dr. Sternglass, and others. It should be noted that the Interagency Task Force on the Health Effects of Ionizing Radiation (Libassi Report) also gave little credence to Dr. Bross's work.

As Dr. Gotchy made clear in his affidavit of November 26 1980, concerning the NEPA impacts of low-level radiation (Affidavit pp. 6-8), Dr. Bross's work is among the "few studies that have been roundly criticized for reasons ranging from dishonesty to poor statistical methods." The BEIR III Report critically reviewed the contributions of Dr. Bross and his colleagues (BEIR III pp. 556-559) and concluded: "The applications by Bross et al. have been clearly incorrect, and they provide no evidence that the risk of cancer from

low-dose radiation is greater than indicated by conventional estimates." (BEIR III p. 559)

III.

On page 4 and Figure 1 of his study, Bross provides his assessment of the three hypothesis relating to the risks of low-level radiation. This assessment is apparently based on his misreading of these three hypotheses on the dose-response function, which Bross designates as curves A, B, and C on Figure 1. Bross' treatment of these "three rival theories" is both ambiguous and self-contradictory.

Bross fails to define what he means by "doubling dose" for leukemia. The only reasonable definition of this term is that excess dose of radiation which doubles the risk of leukemia to twice the control risk (i.e., the risk with no excess radiation).

What Bross does not seem to understand is that any conceivable dose-response function must necessarily give a zero excess risk for zero excess dose, whether the function has a threshold, is linear, or otherwise.

I can see two ways of interpreting Bross' Figures 1, both of which clearly show that Bross lacks as understanding of the theories which he discusses. In either case, I will assume that the X-coordinate measures excess radiation dose, the only reasonable assumption. Suppose (case 1) the Y-coordinate measures excess risk of disease.

Based upon the indisputable fact that there is a zero excess risk for zero excess dose, it then follows that not only the linear curve B, but also curves A and C as well must pass through the origin. If on the other hand (case II), the Y-coordinate measures the total risks (i.e., inclusive of the control risk), then all three curves must pass through the point on the Y-axis corresponding to the control risk. Bross's Fig. 1 does not correspond to either case I or case II.

In my own Figure 1, attached hereto, I have shown four hypothetical dose-response curves which converge in the high-dose range as do those of Bross. Since these all pass through the control risk point on the Y-axis, only curve C looks like the corresponding curve in Bross' Figure 1. However, it is not curve C in my diagram which shows the most damage at low doses. In fact, this curve shows less damage than the linear curve. In order for a curve to show more damage at low doses than the linear, it would have to be shaped like curve D (in my Figure 1) which begins with more steeply inclined slope than the linear. The curve showing least damage at low doses is, of course, curve A which has a threshold. Therefore, it is apparent, based upon Bross's Figure 1, and my revised Figure 1, that Bross has no real understanding of the three hypotheses which he discusses.

IV

Dr. Bross claims (p. 17) that:

> by using the co-occurrence hypothesis, it is possible to confront the three theories [discussed above] directly with the facts. What does the dosage response curve actually look like in the dosage range of about 5 rems? Figure 3 shows the results from one of our studies of men who received diagnostic x-rays with dosages in this range.

The data for Bross's Figure 3, which he states were derived "from one of our studies" were taken in fact from his Reference 25, Table 4. I was able to determine this because the data upon which his Figure 3 is derived are the same data as presented in Table 4 of Reference 25. A review of Reference 25 demonstrates however, that Dr. Bross has misapplied his own data and in so doing, has presented results which the data do not actually support.

In his original Report (Ref. 25) Dr. Bross presented these data in terms of "percent affected," which he described on the preceding page of that report (Ref. 25, p. 133) as: (a) inclusive of a "baseline level for the leukemia even in individuals with no-x-ray exposure;" and (b) related to, but not the same as, the probability of leukemia. When the percent "affected" values of Table 4 (Bross Ref. 25) together with the 95% confidence intervals, are all multiplied by 9, Figure 3 of the Bross affidavit coincides and agrees perfectly

with the transformed data from Table 4 of Reference 25. The implication of this transformation is that the "baseline level" of percent affected is 11.11% (9 times which is 100%) for all those three age groups.

Directly contrary to Bross' statement on p. 18 of the affidavit "that the percentage increase has already adjusted out the background risk of leukemia," the baseline level has not been substracted out. Thus Figure 3 of this paper is mislabelled -- the Y-values do not represent leukemia risk, nor are they excess.

I have revised Figure 3 in a way that is consistent with the data presented in Bross Reference 25.*/ The Y-values are now correctly given as percentage in excess of percent "affected." It is clear from my revised Figure 3 that the line corresponding to the doubling dose of 100 rads fits at least as well as that for the 5 rad doubling dose. In other words, when Bross's own data are presented correctly there is no longer a case for rejecting one in favor of the other.

Bross also does not mention that his Reference 25, including the data cited here, were severely criticized on statistical grounds in a paper by Boice and Land

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*/ However, since I have calculated "excess," the confidence intervals should be increased somewhat.

which was published in the same issue of the American Journal of Public Health and also in the BEIR III Report (1980). Bross has never, to my knowledge, answered these statistical objections. If these criticisms are correct, the data are meaningless even before the various transformations made by Bross for the affidavit presented in this proceeding.

Bross's further analysis for Figure 4 of his affidavit, purporting to give confidence intervals for the doubling dose by a minimum chi-square procedure is, of course, invalid since it is based on the erroneous results given in Bross Figure 3.

CONCLUSION

To recapitulate, my review of I.D.J. Bross' "A 1981 Reassessment of the Health Hazards of Low-Level Ionizing Radiation" dated October 9, 1980, shows that Bross provides no evidence that the risk of cancer for low-dose radiation is greater than those indicated by BEIR I, (1972), the United Nation Scientific Committee on the Effects of Atomic Radiation (1977), and the BEIR III (1980) reports -- essentially the same reports relied on by the NRC Staff in their motion for summary disposition and by Dr. Reginald L. Gotchy in his affidavit. There is no substance in the affidavit submitted by Dr. Bross, whose views as set forth therein have been largely discredited by the reputable scientific community, to show that material issues of fact exist.

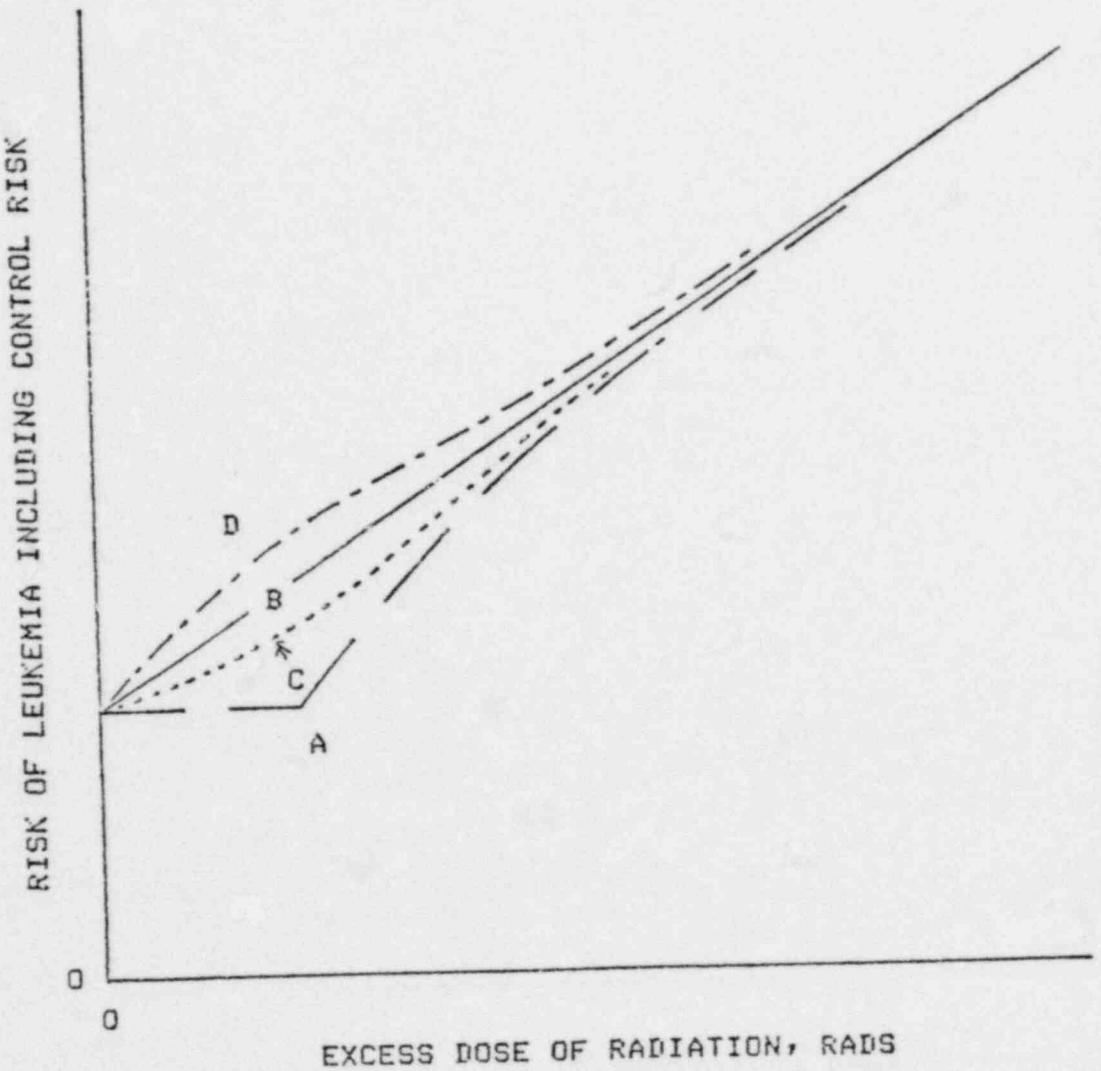
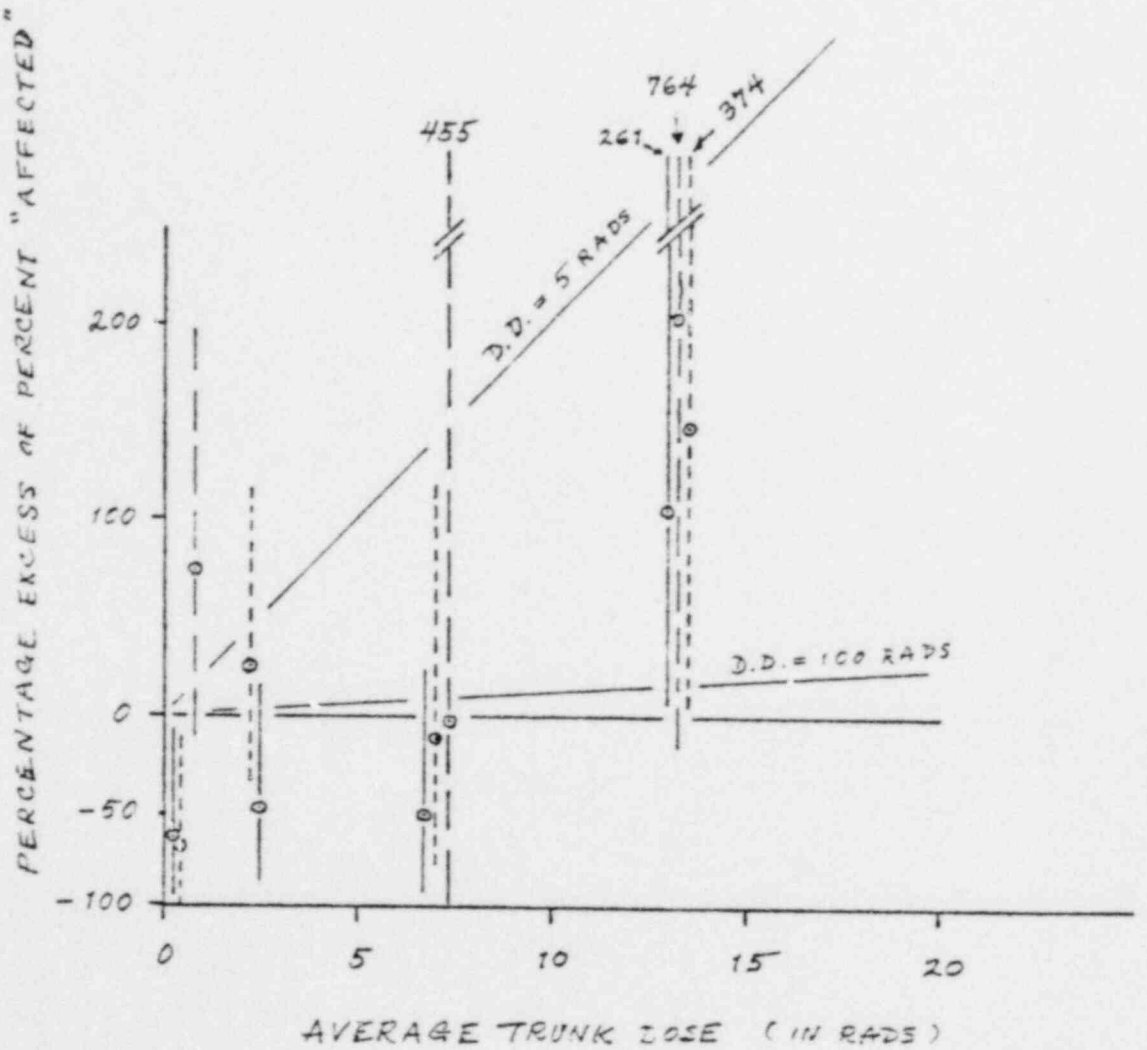


FIG.1. POSSIBLE DOSE-RESPONSE CURVES FOR RADIATION-INDUCED LEUKEMIA (SEE TEXT).



REVISED FIGURE 3

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AFFIDAVIT OF LEONARD D. HAMILTON

District of Columbia

I, Leonard D. Hamilton, of lawful age, being first duly sworn, upon my oath certify that I have reviewed and am thoroughly familiar with the statements contained in the attached affidavit addressing intervenor's contention on the health effects of low level radiation and that all statements contained therein are true and correct to the best of my knowledge and belief.

Leonard D Hamilton
Leonard D. Hamilton

Subscribed and sworn to before me this 9th day of March, 1981.

Jane C Renald

My Commission expires:
June 14, 1985

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CERTIFICATE OF SERVICE

I hereby certify that copies of Affidavit of Leonard D. Hamilton in Response to Affidavit of Irwin D. J. Bross Entitled "A 1981 Reassessment of the Health Hazards of Low-Level Ionizing Radiation" Dated 10-9-81 were served on the following by deposit in the United States mail, postage pre-paid this 9th day of March, 1981:

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