

AFFIDAVIT OF IRWIN D.J. BROSS IN RESPONSE TO
AN AFFIDAVIT OF LEONARD D. HAMILTON (MARCH 9, 1981)

Non-scientists confronted with my scientific report, "A 1981 Reassessment of the Health Hazards of Low-Level Ionizing Radiation", and the affidavit by Leonard D. Hamilton of March 9, 1981, might seem to be in the classic dilemma: What to do when experts disagree?

Fortunately in this instance there are three simple ways to resolve the dilemma. The first resolution is possible because Hamilton and Bross are in complete agreement on one basic point: There are not two experts here, there is one real expert and one fraud. The only question then is, which is which?

Among other compliments, Hamilton says that Bross's work is among the "few studies that have been roundly criticized for reasons ranging from dishonesty to poor statistical methods" (p. 7) and "have been largely discredited by the reputable scientific community" (p. 14).

On the other hand, "A 1981 Reassessment of the Health Hazards of Low-Level Ionizing Radiation" reviews more than 40 scientific reports which have the following characteristics in common:

- (1) They are studies of human populations who were actually exposed to low doses (0.1 rem to 10 rems) of ionizing radiation (in contrast to the data on high-dose exposures used in all of the reports Hamilton cites at the top of page 3).
- (2) The studies are biostatistical-epidemiological studies involving the use of statistical analysis of data on morbidity or mortality.

(3) Most of the cited studies have been reported within the past five years (in contrast to the long-outdated studies used in the reports cited by Hamilton on page 14).

Since the area of expertise required for a scientific evaluation of the current statistical evidence on low-level radiation hazards is training, experience, and demonstrated competence in carrying out studies with the above-mentioned characteristics, it is not hard to distinguish the real expert from the fraud. Consider the factual information on Irwin Bross and Leonard Hamilton.

(1) One has a Ph.D. in biostatistics, has headed a Department of Biostatistics at an internationally famous cancer research institute (RPMI) for over 20 years, and is a long-time fellow of the American Statistical Association. The other has no statistical qualifications.

(2) One has published over 300 scientific papers on epidemiology, biostatistics, and public health, has just had a book (SCIENTIFIC STRATEGIES TO SAVE YOUR LIFE) published by a reputable scientific publisher (Dekker, New York, March 1981) on these specific issues. The other has never published a single original and competent paper in biostatistics, epidemiology, or public health.

(3) On March 17, 1981 in Cincinnati, Ohio, both Hamilton and Bross were at a public meeting of the scientific advisory committee for the Center for Disease Control/National Institute for Occupational Safety and Health (CDC/NIOSH) in the federal building in Cincinnati, Ohio. Dr. Karl Z. Morgan, Dr. Thomas Mancuso, and Dr. Irwin Bross had been originally named to the panel by Congress in recognition of their important scientific contributions to research on low-level

ionizing radiation (See Serial No. 95-179). Subsequently the Atomic Industrial Forum, the official lobby of the nuclear industry, insisted upon representation on the panel and Leonard Hamilton was appointed as their lobbyist.

(4) At the meeting of March 17, 1981 (which might be said to involve the reputable scientific community), Hamilton's repeated interruptions of Bross specifically brought up the question of scientific credentials. On several occasions, Bross referred to Hamilton's complete lack of qualifications (e.g., "amateur statistician"), but no one at this reputable scientific meeting (not even Hamilton) questioned this assessment. Nor did anyone question Bross's statement that he was the only professional statistician on the panel. Bross's standing in the international scientific community is evidenced by the introduction used by the New Scientist for a recent article (Attachment A).

On the basis of this factual evidence, it should not be difficult to decide who is a scientist and who is a lobbyist, who is a real expert and who is a fraud.

A second way to resolve the dilemma directly involves the Motions for Summary Disposition since this puts the burden of proof on NRC. If there is prima facie evidence of serious adverse health effects from normal operations of a nuclear installation under NRC ALARA standards, this evidence should be heard. It is not necessary at this point that the evidence be conclusive.

On page 3 of his affidavit, Hamilton refers to "the fatal flaws in the Najarian and Colton studies on the Portsmouth Naval Shipyard" (Affidavit pp. 8-13) to deny the conclusions in the original Bross

affidavit. Since the shipyard was operating under NRC ALARA standards, confirmation of the Najarian-Colton findings would be direct evidence that the NRC permissible levels represent a serious hazard to the health of nuclear workers. Since the permissible levels for the public are set on a similar basis, there would also be hazard to the public. This would also flatly refute the reports relied on by the NRC Staff in their motion for summary disposition (Affidavit p. 14) and in the Gotchy affidavit.

For the estimates from the reports cited on page 14, it would have been impossible to find excess risks of leukemia and other cancers at the Portsmouth Naval Shipyard. If, as will now be shown, clear scientific evidence of such risks were found in the CDC/NIOSH data on PNS, this is prima facie evidence from a federal study.

The scientific evidence of serious health hazards can be found in CDC/NIOSH documents prepared for the March 17, 1981 meeting in Cincinnati previously cited in connection with credentials. All statements can be verified from the CDC/NIOSH documents and tapes of this meeting. In particular the "fatal flaws" to which Hamilton refers on page 3 were discussed at some length. Although Hamilton repeatedly claimed that the Najarian-Colton 1978 report had been specifically refuted, he was all by himself. None of the other four panel members agreed with this extremist position.

In an attempt to refute a statistical analysis by Dr. Colton, CDC/NIOSH belatedly did some analyses of its own data. The result is described briefly in an article entitled "A False Report on Radiation

Hazards at a Nuclear Shipyard" prepared for publication by Bross (Attachment B). A photocopy of the analysis in which CDC/NIOSH inadvertently confirmed the Najarian-Colton report on leukemia by getting a statistically significant result ($p = 0.0464$) is given as Attachment C and refutes Hamilton's assertions.

From the March 17 documentation it is clear that the original Najarian-Colton study of the PNS workers was essentially correct, that the citation of this study by Bross was warranted, and that the "fatal flaws" claimed by Hamilton exist only in his own mind. It should be noted that this exchange brings out clearly the strong scientific evidence of serious health hazards from radiation exposures at a normally-operating nuclear installation that conformed strictly to all NRC regulations. This is far more than the prima facie evidence needed to throw out the motion for summary disposition by the NRC staff.

This evidence was produced by CDC/NIOSH, a federal agency, but the reluctance of the agency to report the true facts to Congress or the the public is evident from Attachment B. This only emphasizes the need for a full consideration of the more than 30 positive studies of low-level radiation hazards cited in the original Bross affidavit (and attacked in the Hamilton affidavit of March 9, 1981) since none of this important new scientific information has been used in the obsolete risk estimates relied upon by the NRC staff in their cost-benefit calculations. The CDC/NIOSH lung cancer data, for instance, shows risks approximately 200 times greater than the BEIR I and III estimates, more than enough to reverse the cost-benefit ratios, a point made in a recent letter to President Reagan (Attachment D).

Finally, the third way to resolve the dilemma on disagreement between experts is to look at the specific points raised by Hamilton in his affidavit. Because of the foregoing, this can be done as briefly as possible.

The claim on page 2 that "Dr. Bross has no true facts upon which to base his assertion" can be refuted by simply going to the list of references in the original affidavit, noting that there are more than 30 positive studies. Each involves thousands of persons exposed to low-level radiation and each involves "true facts" that have been disparaged but not denied.

Hamilton's claim on page 2 to speak for the "scientific community" is refuted by the March 17 meeting where he was the only member of the panel to hold his extremist views.

The claim that "Bross has completely misrepresented the data he has used" and the "critique" on pages 9-13 is based on nothing more than technical-sounding doubletalk. Hamilton expects to get away with this fraud because non-scientists cannot understand these technicalities. However, the public can judge who is the real expert and who is not and can find a full discussion of these matters in plain English in Bross's new book, SCIENTIFIC STRATEGIES TO SAVE YOUR LIFE.

The same book answers in detail the rhetorical questions about hypotheses and definition of doubling dose (p. 9). Hamilton doesn't understand that adjustments are handled more scientifically in a mathematical model than by his Mickey Mouse arithmetic (and they have been made this way). All that is shown on pp. 9-13 is a total lack of statistical competence on Hamilton's part (a common lack in the radiation

protection community).

Finally, Hamilton says "By ignoring all negative studies, Bross has limited his universe and, therefore, skewed his results greatly". As was pointed out repeatedly (and can be easily verified), the list of studies cited in Bross's affidavit to Reference 34 were all of the relevant scientific reports cited in the Libassi list of references plus a few others omitted from that list. The selection was made by the Interagency Task Force, not Bross. References 41 to 47 updated the listing. Note that references 41 and 43 are negative reports. Clearly Hamilton made his false assertion without bothering to actually look at the references. Once this claim of "systematic error" is refuted by the actual reference list, it might be noted that Hamilton's admission still stands: "The 'bundle of frail reeds' argument is correct when the individual reeds suffer only from random error." Thus, Hamilton's affidavit, when corrected for misstatements, actually supports the arguments in the original affidavit.

LIST OF ATTACHMENTS

- Attachment A: Muddying the Water at Niagara (Published in the New Scientist, Vol. 88, pp. 728-729, 11 December 1980).
- Attachment B: A False Report on Radiation Hazards at a Nuclear Shipyard. (Material submitted for publication).
- Attachment C: Photocopy of CDC/NIOSH printout, March 17, 1981.
- Attachment D: Letter sent to President Ronald Reagan, March 12, 1981.

Forum

Muddying the water at Niagara

Irwin D. J. Bross

LOVE CANAL, a suburb of Niagara Falls in New York State, is the scene of the largest and most notorious of America's hazardous-waste tragedies. It exploded once again into prominence in mid-1980 with the first of a number of controversial studies of the effects on the health of the local community of the toxic wastes dumped there (*New Scientist*, vol 86, p 298). Here a leading American biostatistician airs his views on the latest report.

Was there a serious public health hazard at Love Canal or was it merely much ado about nothing? The answer depends on whether you believe the opinions of a blue-ribbon panel convened by Governor Carey of New York State or look at the factual evidence that this panel was supposed to consider.

The facts come from an epidemiological survey of the Love Canal area begun in June 1978 and focused on excess miscarriages, birth defects, and low birth weights. The panel did not question the facts themselves. Instead, the 10 October, 1980, report of this panel says: "The results of the latter studies were and remain inconclusive, owing in part to the relatively small population available for study and the absence of a comparable, matched population of controls with which to compare the figures. The investigators (Dr Nicholas Vianna and others at the Department of Health) thought there might be some increase in miscarriages and infants with low birth weight, but the data cannot be taken as more than suggestive." The following are the facts:

(1) Using maps and aerial photographs, Vianna and colleagues divided the Love Canal area into five sub-areas where, on the basis of proximity and drainage, different levels of chemical

contamination would be expected. There were about twice as many pregnancies in the three areas of probable lower contamination as in the two areas of higher contamination. The contamination evaluation was made before the health survey was completed.

(2) In the two areas of probably high contamination, there were 158 pregnancies and 37 miscarriages. In the other three areas there were 318 pregnancies and 35 miscarriages. The risk of miscarriage was more than twice as high in the women in areas of high contamination as in the controls at Love Canal from areas of lower contamination.

(3) The 113 per cent increase in the miscarriage rate is statistically significant. (The data have 95 per cent confidence limits from 17 per cent to 319 per cent.) This large increase is also significant from the stand-point of public health and indicates a serious hazard.

(4) In the higher contamination areas there were 122 live births and 14 documented birth defects. In the control areas there were 280 live births and 15 birth defects. Again, the risk of birth defects was more than twice as high in the areas at Love Canal with probable high contamination as in those areas with probable low contamination.

(5) The 114 per cent increase in birth defects considered by itself is not quite statistically significant at the 95 per cent level. However, under the null hypothesis of no effect from chemical contamination, the miscarriages and birth defects would be statistically independent. The chance that both would be markedly elevated is extremely small, much less than 1 per cent. If, however, the contamination had produced genetic damage, then under this non-null hypothesis one would expect both types of reproductive wastage to be elevated.

(6) An informal statistical argument on the above point can be based on the fact that there are 10 ways to divide the five sub-areas into a two-area combination and a three-area combination. There is therefore one chance in 10 that the highest differential in miscarriages would occur in the combinations suggested by the maps. There is the same chance that this would also be true for birth defects. Thus the probability of both events occurring is $0.1 \times 0.1 = 0.01$ —one chance in 100.

(7) Methodological details in the Vianna report establish the validity of the controls. The women in the higher and in the lower contamination areas are similar with respect to age, race, and other factors. The women in the higher

risk areas have a further control obtained by comparing their pregnancy histories before and after moving to Love Canal. There are no documented birth defects in 57 live births prior to the move to Love Canal.

There is a striking difference between what the blue-ribbon panel report says about data and what it actually does with data. It says: "The public deserves no less than the facts as we know them concerning environment-host interactions, even if those facts constitute an incomplete body of knowledge and if they reveal the limitations of the science of this field at the present time." However, the blue-ribbon report does not mention any of the above facts (it offers only opinions of the type already quoted). It might be noted that the Health Department report also does not present the above facts directly, and it is necessary to do some digging to get to the key facts in that report.

Finally, do the above facts demonstrate that the chemical contamination at Love Canal has probably produced a serious public health hazard? This question involves assessment of scientific evidence and there are two different approaches to this task. On one hand, one can make a rigorous and objective evaluation of the quality of the data and their implications for public health. An alternative approach is to make subjective judgements on the basis of "expert opinion". From the internal evidence of the blue-ribbon panel report it is clear that the panel exclusively relied on the latter approach.

What is needed for objective evaluation? One requirement is a careful review of the methodology used in the design, conduct and analysis of the Health Department's survey at Love Canal. By current "state of the art" standards in epidemiological field studies the methodology is acceptable and the quality control on the data exceeds usual ►

One long wait

HOLIDAY TRADE in Red China is booming. Both Pan Am and British Airways have now been granted government approval to provide the first regular passenger air service across the Pacific for 30 years. But once inside China visitors must still fly on aircraft operated by the Chinese Civil Aviation Administration which has a monopoly on internal air travel. A group of Japanese tourists recently told a horror story of CCAA bureaucracy. Their flight, from Chengdu to Tianjin via Peking was much faster than scheduled because fog at Peking prevented the airliner from making its routine halfway stop. But Chinese bureaucracy works in strange ways. The CCAA rule book says that passengers bound for Tianjin have to fly via Peking. So once the fog had cleared the Tianjin-bound passengers had to fly back to Peking and then return to Tianjin before they were allowed off the plane. □



A FALSE REPORT ON RADIATION HAZARDS
AT A NUCLEAR SHIPYARD

On 31 January 1981, The Lancet published a report from an agency of the United States government (Center for Disease Control/ National Institutes for Occupational Safety and Health, CDC/NIOSH) on "Cancer Mortality at a Navy Nuclear Shipyard" (1). In reporting on the results of a follow-up study that cost over \$1 million and took almost three years, CDC/NIOSH reached the following conclusion concerning hazards to nuclear submarine workers at the Portsmouth Naval Shipyard (PNS) in Kittery, Maine:

"Finally, in PNS radiation workers, we found no positive dosage response relationships between ionizing radiation dose and mortality from any cause reported."

At a belated meeting on March 17, 1981 of the scientific advisory committee on the CDC/NIOSH PNS study in Cincinnati, Ohio, the reason for the negative findings reported in The Lancet came to light: At the time when the report was submitted, CDC/NIOSH had not carried out a single statistical analysis of the dosage-response relationship for any cause of death.

In response to a January 26, 1981 memo from me which pointed out a clear and strong relationship between the reported lifetime radiation doses and the occurrence of excess lung cancer in the radiation-exposed workers and a letter from Dr. Theodore Colton with a statistical analysis of the dose-response relationship for leukaemia and related

diseases (Table VI,1), CDC/NIOSH finally did some statistical analysis of the PNS data for the March 17, 1981 meeting. These were reported at this open public meeting and the proceedings were taped by CDC/NIOSH so that all statements in this article can be verified by documents or tape.

What CDC/NIOSH found after doing their own analyses of their own data were positive relationships between ionizing radiation dose and mortality from a number of different causes of death, a flat contradiction of the false report in The Lancet for 31 January 1981. The positive relationships range from weak and iffy to strong and clear. As always, there could be different "interpretations" of the results but this in no way can justify a false report of the actual results.

Scientists or other readers who find it incredible that an agency of the U.S. government would put a false report in the scientific literature must consider the political context of this and other studies of the health hazards of low-level ionizing radiation. The 31 January article notes that "This study was done at the request of the Subcommittee on Health and the Environment of the United States House of Representatives"... "to determine whether the previously reported excess risk of leukaemia and of other cancer mortality among nuclear workers could be corroborated and to evaluate dose-response relationships between radiation and cancer." The "previously reported excess risk" refers to the testimony of Dr. Thomas Najarian at the 1978 hearing and his report (with Dr. Colton) in The Lancet in May 1978 (2).

At the 1978 hearings Congress heard extensive testimony on how federal agencies, particularly the Department of Energy and the National

Cancer Institute, had suppressed the studies of low-level radiation hazards that found positive results (3) and named Dr. Karl Z. Morgan, Dr. Thomas Mancuso, and myself to an oversight committee to keep this from happening at PNS. The Atomic Industrial Forum, the lobby for the nuclear industry, insisted on three panel members to represent the industry position, and eventually there were nine members. At a meeting of this committee in August 1980, it was agreed that the follow-up of the PNS workers was adequate for purposes of analysis. However, as the only professional statistician on the oversight committee, I was concerned that CDC/NIOSH did not present any plan for the statistical analysis.

In September 1980, the committee received a set of data tables for what was now called a "final report". There was no analysis and no text. For dosage-response analyses the critical tables were what CDC/NIOSH called "dosage x latency" tables. For every combination over a set of dosage categories and "latency" categories, these data tables give the observed deaths and the expected deaths (on the basis of U.S. white male vital statistics) for each cause of death. Apart from leukaemia and a few related causes (Table VI,1), these critical tables were missing from the final report.

I repeatedly requested these tables and they were repeatedly promised to me. CDC/NIOSH assured me that the only reason they had not been sent out to the advisory committee was because there were too many and there was nothing of interest in them. My set of tables finally arrived in late December 1980, weeks after the final report was released in early December 1980. The rest of the panel did not see these tables until March 17, 1981.

In October, a draft of the final report was mailed out. It had the original tables, no statistical dosage-response analysis, a brief text, and a completely negative conclusion. Despite my strong objections that this was an incompetent report and not even an adequate preliminary analysis (and despite other objections raised by Dr. Morgan, Dr. Cobb, and other committee members), CDC/NIOSH released its final report to the media in early December and submitted an article to The Lancet without even sending a copy to the committee. Thus, the conclusion of the final report had been predetermined by CDC/NIOSH long before any statistical analysis of the data was made, a practice not uncommon in U.S. science and health agencies when official policies are involved (4).

When I finally got the data on the missing causes of death, I naturally turned first to lung cancer. This is not only known to be radiogenic, but it also had the largest number of cancer deaths. Since it is well known that there is a long latent period for solid cancers, I looked at the workers who had 15 or more years "latency" and then at the simplest dosage categorization. At doses less than 0.5 rem for lifetime exposure it would be difficult to detect any radiation effects, but for dosages over 1 rem there would be some chance of detecting such effects. The expected numbers were similar in the higher dose (6.56) and lower dose (7.40) series, so the observed deaths can be directly compared. Since CDC/NIOSH had said there was nothing in the tables, I was a bit surprised to find 13 lung cancer deaths at the higher dose and only 3 at the lower dose--a difference which would be hard to miss even with the "eyeball" method CDC/NIOSH had used to "analyze" its data (as acknowledged

on tape). There was a clear dosage-response curve for lung cancer. Using either a sign test or Chi-square or non-parametric (rank) tests or parametric regression tests, the dose-response relationship was significant or highly significant by standard methods. I alerted the committee and CDC/NIOSH to the lung cancer relationship in a January 26 memo.

On March 4, 1981, Assistant Surgeon General Anthony Robbins, the head of NIOSH, sent me a letter defending the CDC/NIOSH statements in The Lancet and enclosing a written reply by his staff to my January 26 memo. While conceding that the lung cancer data table in the memo was "a legitimate use of the latency-by-dose chart and is the sort of thing our epidemiologists might choose to do", and that "the probability of the observed excess happening by chance alone is less than 5%", CDC/NIOSH continued to insist that "Dr. Bross's assertions are not supported by this data."

The argument used here (one often used whenever data fails to support a predetermined conclusion) was that I had not fully specified lung cancer as an "a priori hypothesis" to test. Yet the 31 January article specifically mentions lung cancer (citing two papers), and a 15 or more year latent period (and only this period) would be consistent with the March 4 statement by Dr. Robbins that "the report reserves judgment"... "because of the small cohort size and short latency experienced by most of the cohort relative to the cohort size and latency periods necessary to study organ cancers."

To show the danger of doing statistical analyses of dose-response relationships instead of "eyeballing" the causes of death, the CDC/NIOSH staff commented: "To demonstrate further that patterns which

will occur in the data by observation of them in the absence of an a priori hypothesis leave the meaning of their existence questionable, one can examine the category "cirrhosis of liver, ICD581" and apply the same dosage and latency requirements that he used for lung cancer. For this ICD, a statistically significant increase ($p < .05$) can be observed in those with greater than 1 rem and greater than 14 years latency. Similarly, diseases of the digestive system are significantly elevated." Indeed, there are several other suggestive or significant causes of death when an appropriate analysis of the dose-response is made. One of the more important scientific findings of this study may eventually be that there is a broad spectrum of disease from low-level radiation rather than excess cancer only.

It might also be noted that the CDC/NIOSH statistical analyses in the March 4 letter and at the March 17 meeting were not only made long after the paper had been submitted with a flat negative conclusion, but were made for the purpose of "refuting" the communications received from Dr. Colton and myself. The story abounds with ironies. CDC/NIOSH did an analysis of "All malignant neoplasms of the lymphatic and hematopoietic tissue" (mislabeled "Repeat of Colton Analysis") and emerged triumphantly with a "very nonsignificant" p-value of 0.9546. However, to nail down the point, they also ran leukemia alone. This gave $p = .0464$, thus inadvertently confirming the 1978 Najarian-Colton report. CDC/NIOSH disposed of this awkward result by dropping the case of leukemia in the highest dosage category to get the p-value up to 0.64. After all, as reported in the January 5, 1981 Boston Globe, "the NIOSH team said their findings specifically refute the 1978 study."

Readers who might wish to draw some moral from the sad-but-true story of how U.S. government agencies do science have a choice of several. The first is: In normal science "A theory must fit the facts" whereas in U.S. federal science "The facts must fit the policy."

Another moral is: There is no such thing as a "safe" position. Perhaps to mollify me, NIOSH noted in The Lancet that there was still the possibility that at some future time positive relationships might start appearing (because of longer "latency"). Unfortunately this did not please Admiral H.G. Rickover, the father of the nuclear navy who had demanded total "vindication" of PNS from the start. His tirade against NIOSH to a congressional committee hit where it hurt.

Ironically enough, the navy also turned the negative statement against NIOSH. CDC/NIOSH had been hoping to follow the epidemiological study with a large-scale cytogenetic study at the shipyard. On February 23, 1981, Vice Admiral E.B. Fowler refused navy sanction of this proposal with:

"Since this conventional retrospective epidemiology study has demonstrated with reasonable statistical confidence that there are no discernable health effects of radiation exposure in the Portsmouth Naval Shipyard employees, it is concluded that additional studies such as the proposed NIOSH cytogenetic study are not necessary nor justified in the light of available scientific information."

Finally, shortly after writing the cited letter, Assistant Surgeon General Anthony Robbins was summarily fired by the Reagan administration (5).

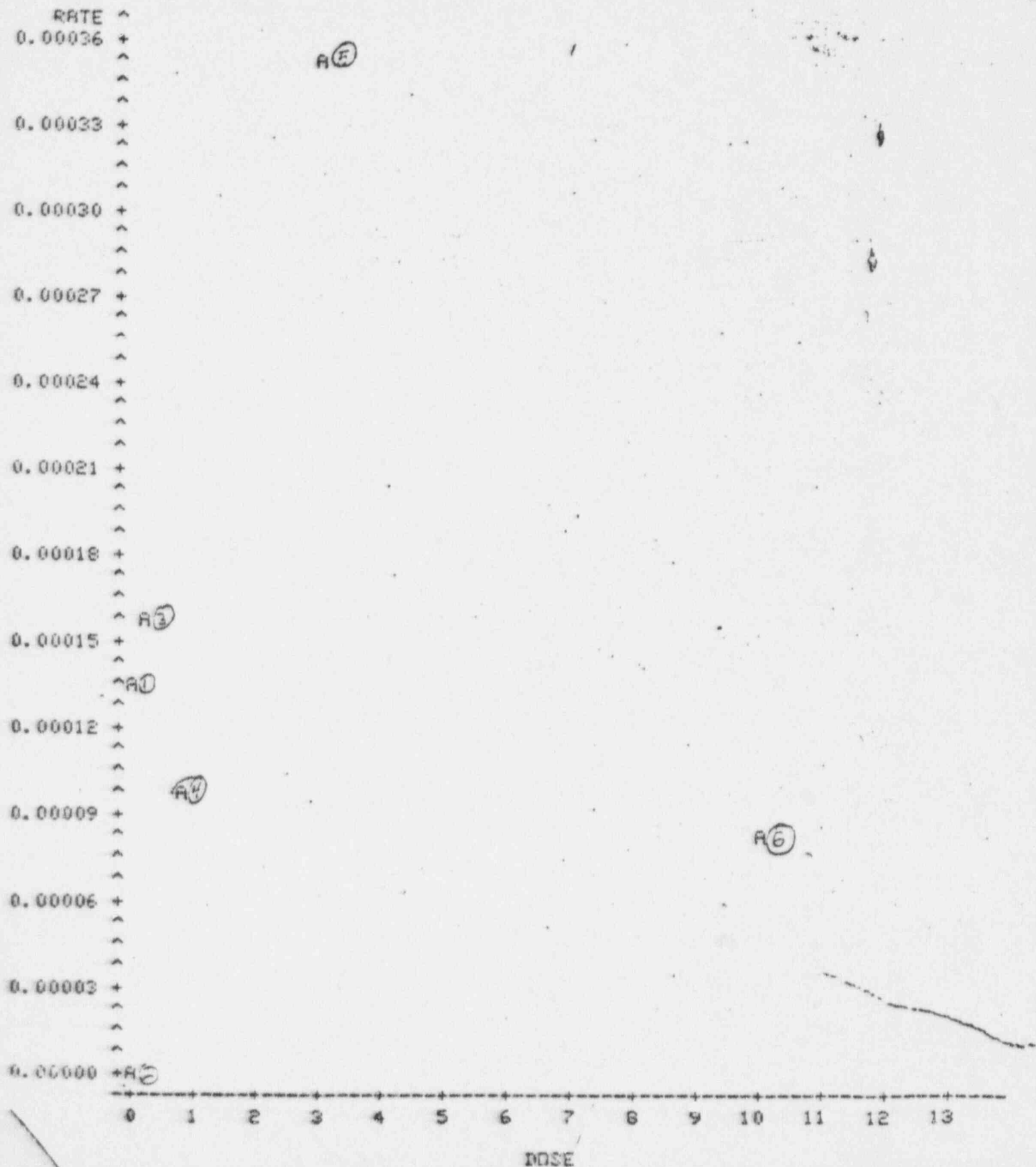
of Lymphatic and
Hematopoietic Tissue
Plot of rate by dose

Repeat of Colton analysis

ATTACHMENT C

midpoint, rate .015,.000137; .065,0; .30,.000157; .75,.000097;
3,.000351; 10,.000084

P=.9546



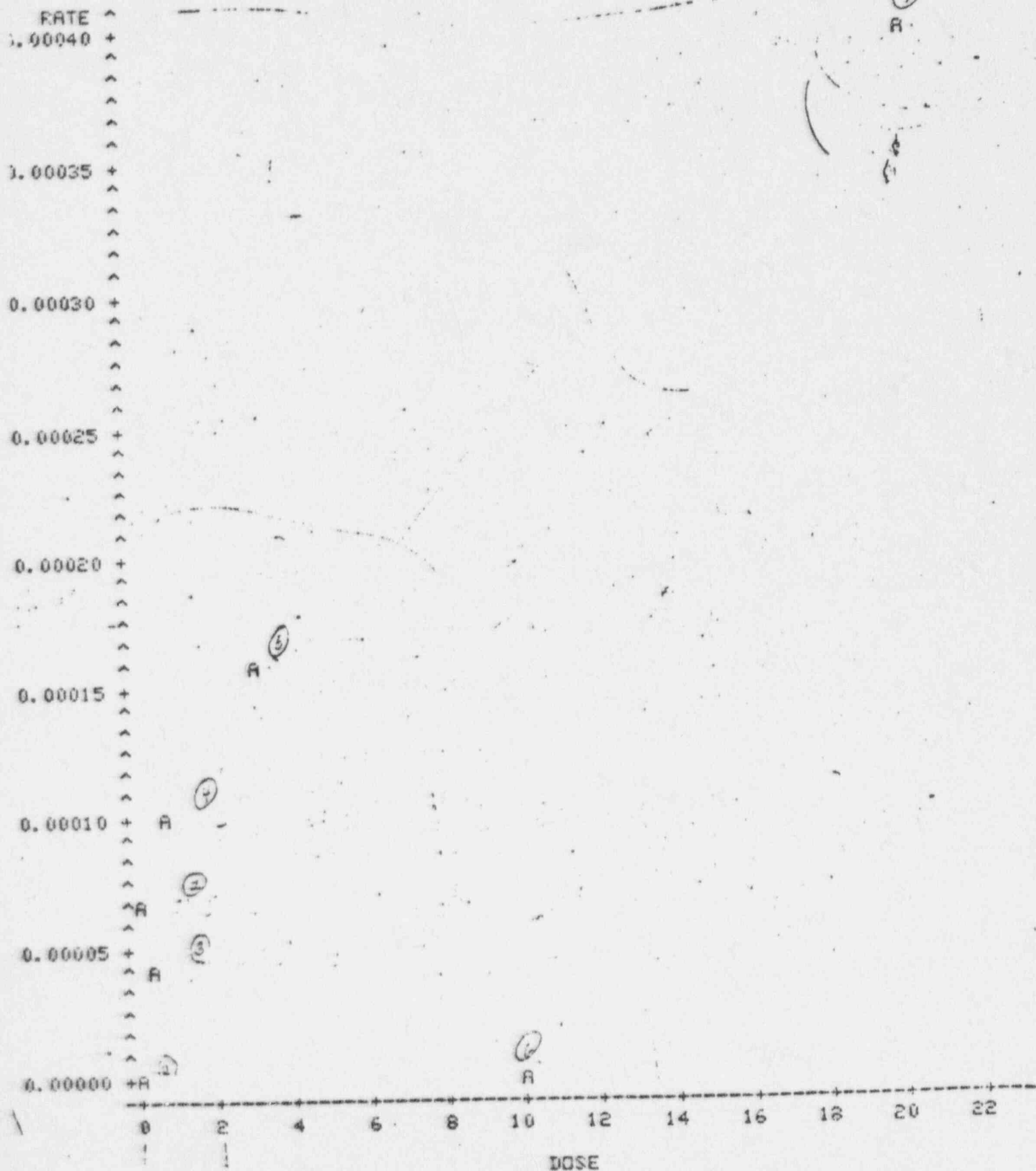
Leukemia

Plot of rate by dose

All 7 cat.

midpoint, rate .015,.000065; .065,0; .30,.000041; .75,.000097;
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P=.0464



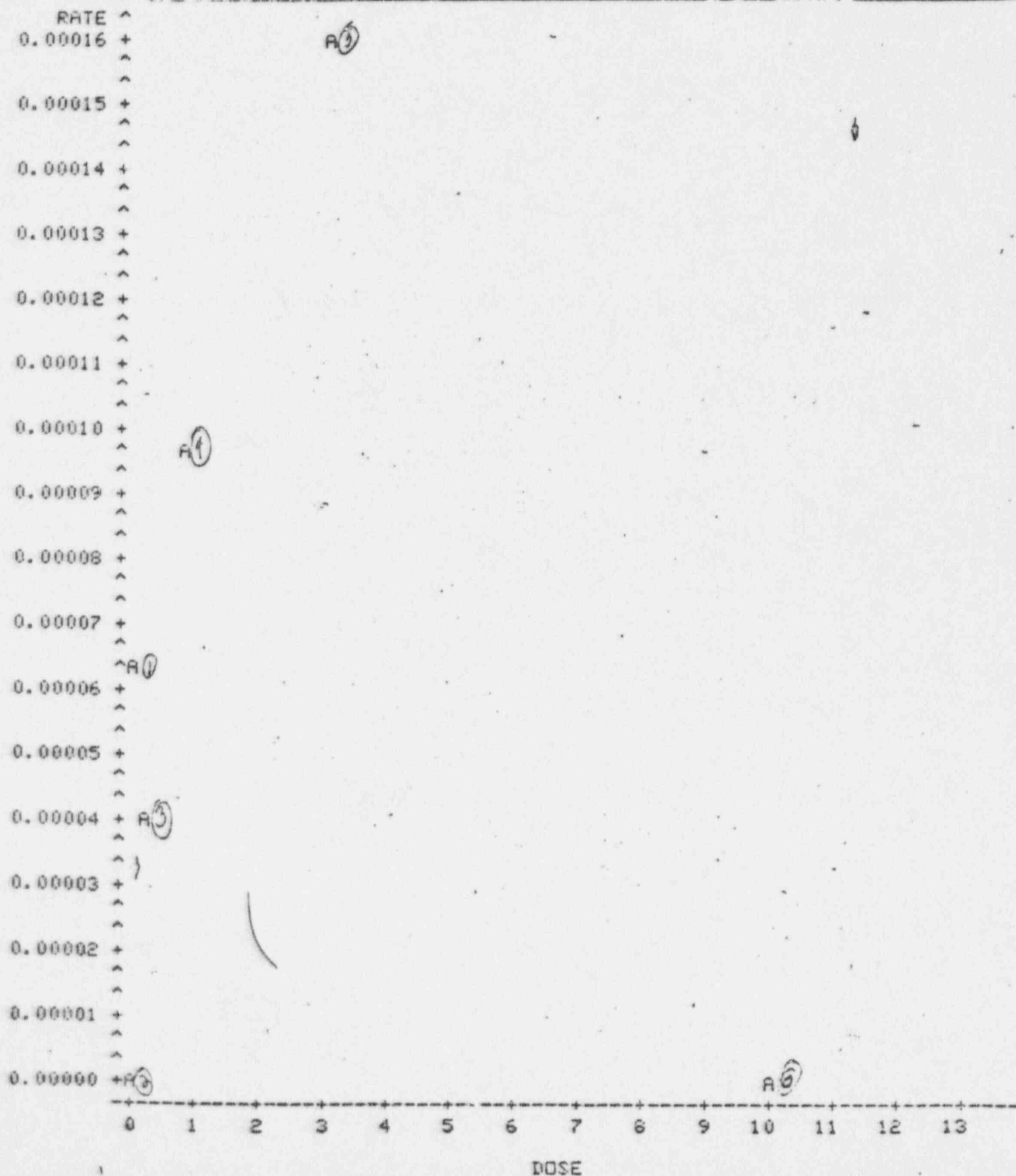
Leukemia

Plot of rate by dose

Drop cat. 7

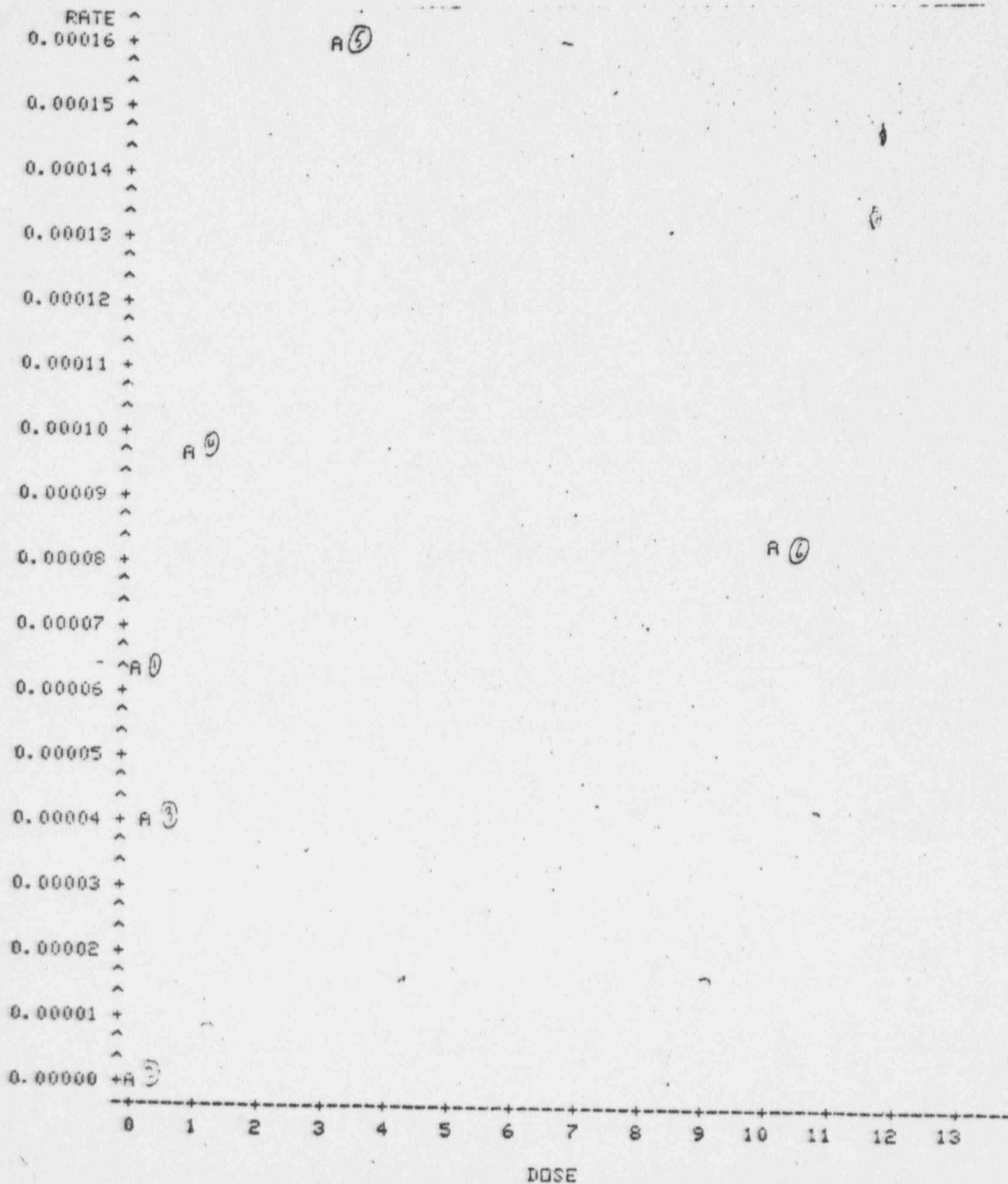
midpoint, rate .015,.000065; .065,0; .30,.000041; .75,.000097;
3,.00016; 10,0

px.64



of rate by dose
midpoint,rate .015,.000065; .065,0; .30,.000041; .75,.000027;
3,.00016; 10,.000084

P=.5179



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No opinions here expressed should be construed as reflecting official positions of the administration of
Roswell Park Memorial Institute or of the N.Y. State Health Department.

March 12, 1981

President Ronald Reagan
White House
1600 Pennsylvania Avenue
Washington, D.C. 20500

Dear Mr. President:

Although you campaigned against regulating corporations to death, you are letting the Nuclear Regulatory Commission (NRC) do just this to the utility at Three Mile Island Unit 2 (TMI-2). What is far worse, NRC overregulation will cause the death of many human beings by insistence on an unnecessarily costly and dangerous "clean-up" plan.

While NRC claims that the "clean-up" will not even cause one death, this estimate is based on out-of-date high-dose data and faulty extrapolation. If instead the latest scientific data on the nuclear submarine workers at the Portsmouth Naval Shipyard (PNS) is used (reported March 17, 1981) the picture is entirely different. There will be around 100 lung cancer deaths in the "clean-up" workers.

Round numbers will be used to keep the calculations simple and the common-sense approach clear. Assuming a 15-year latent period for lung cancer, there are 16 lung cancer deaths among PNS workers with lifetime exposures of 0.5 rem or more. Calculations from U.S. vital statistics by CDC/NIOSH show that only 8.23 deaths would be expected in this series. The approximately 8 excess deaths occurred in roughly 5000 person years. This is about equivalent to 1000 nuclear workers exposed during 1959-1962 getting lung cancer in the 5 years 1973-1977. Since the doubled risk of these workers would continue for at least 10 more years, there would be at least $3 \times 8 = 24$ cancer deaths in this series.

Since probably 4000 "clean-up" workers at TMI-2 would be exposed to radiation doses as high or higher than those of the 1000 PNS workers (whose lifetime exposures averaged only about 5 rem), at least $4 \times 24 = 96$ lung cancers would result from the "clean-up". Note that NRC permits 5 rem annually. In addition to these deaths, there would be radiation-induced deaths from other causes among the "clean-up" workers as well as a substantial number of deaths in the general population from environmental contamination when the radiation is removed from the containment. All of these deaths and disabilities will be caused by NRC regulations because there is a much better way to decommission TMI-2.

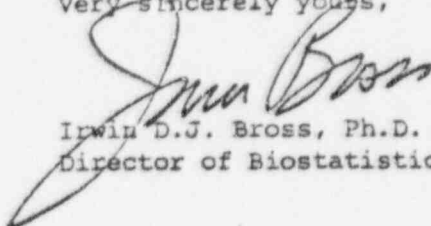
March 12, 1981

Page 2

The method is called "entombment" and consists of immobilizing the radioactivity in TMI-2 in concrete and leaving it in the containment. This would keep the radiation out of the environment. It would also keep the workers out of the containment at TMI-2 since most of the job can be done by remote-controlled equipment. The immobilized radiation isn't likely to hurt anyone until the whole installation crumbles to dust in a few thousand years. The costs of entombment would be about one-tenth of "clean-up" costs.

Unfortunately NRC is determined to decommission TMI-2 by the rule book even though this is going to cause the very public health disaster that was so narrowly averted in the original TMI-2 accident.

Very sincerely yours,



Irwin D.J. Bross, Ph.D.
Director of Biostatistics

IDJB/mak

CC: Open

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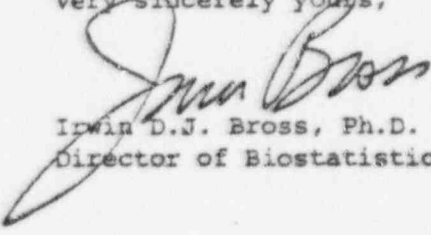
Round numbers will be used to keep the calculations simple and the common-sense approach clear. Assuming a 15-year latent period for lung cancer, there are 16 lung cancer deaths among PNS workers with lifetime exposures of 0.5 rem or more. Calculations from U.S. vital statistics by CDC/NIOSH show that only 8.23 deaths would be expected in this series. The approximately 8 excess deaths occurred in roughly 5000 person years. This is about equivalent to 1000 nuclear workers exposed during 1959-1962 getting lung cancer in the 5 years 1973-1977. Since the doubled risk of these workers would continue for at least 10 more years, there would be at least $3 \times 8 = 24$ cancer deaths in this series.

Since probably 4000 "clean-up" workers at TMI-2 would be exposed to radiation doses as high or higher than those of the 1000 PNS workers (whose lifetime exposures averaged only about 5 rem), at least $4 \times 24 = 96$ lung cancers would result from the "clean-up". Note that NRC permits 5 rem annually. In addition to these deaths, there would be radiation-induced deaths from other causes among the "clean-up" workers as well as a substantial number of deaths in the general population from environmental contamination when the radiation is removed from the containment. All of these deaths and disabilities will be caused by NRC regulations because there is a much better way to decommission TMI-2.

The method is called "entombment" and consists of immobilizing the radioactivity in TMI-2 in concrete and leaving it in the containment. This would keep the radiation out of the environment. It would also keep the workers out of the containment at TMI-2 since most of the job can be done by remote-controlled equipment. The immobilized radiation isn't likely to hurt anyone until the whole installation crumbles to dust in a few thousand years. The costs of entombment would be about one-tenth of "clean-up" costs.

Unfortunately NRC is determined to decommission TMI-2 by the rule book even though this is going to cause the very public health disaster that was so narrowly averted in the original TMI-2 accident.

Very sincerely yours,


Irwin D.J. Bross, Ph.D.
Director of Biostatistics

IDJB/mak

CC: Open

UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

BEFORE THE ATOMIC SAFETY AND LICENSING BOARD

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

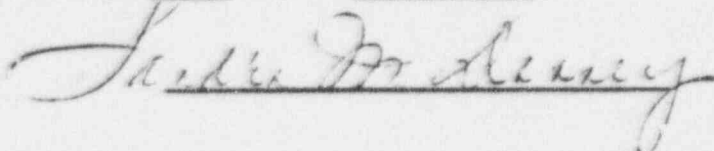
AFFIDAVIT OF IRWIN D. J. BROSS

Buffalo, New York

I, Irwin D.J. Bross, 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 answering Leonard Hamilton's affidavit of 9 March 1981 and that all statements contained therein are true and correct to the best of my knowledge and belief.


Irwin D. J. Bross

Subscribed and sworn to before me this 24th day of March, 1981.


Sandra M. Danney

My Commission expires:

SANDRA M. DANNEY
Notary Public, State of New York
Qualified in Erie County
My Commission Expires March 30, 1983

IRWIN D. J. CROSS, PH.D.
Director of Biostatistics
Roswell Park Memorial Institute
666 Elm Street
Buffalo, N.Y. 14263

No opinions here expressed should be construed as reflecting official positions of the administration of
Roswell Park Memorial Institute or of the N.Y. State Health Department

March 12, 1981

President Ronald Reagan
White House
1600 Pennsylvania Avenue
Washington, D.C. 20500



Dear Mr. President:

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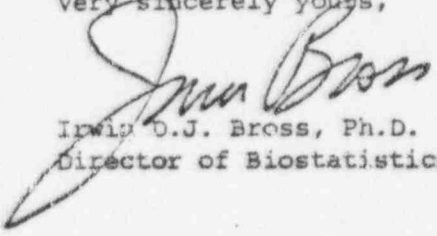
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