Beyea Response Document 1

Summary response to critiques made by the Boeing Company and its consultants of studies of the 1959 accident at the Santa Susana Field Laboratory

Supplementary material provided for the report,

"Feasibility of developing exposure estimates for use in epidemiological studies of radioactive emissions from the Santa Susana Field Laboratory,"

which was prepared in 2006 for the Santa Susana Field Laboratory Advisory Panel, a Project of The Tides Center (http://www.ssflpanel.org)*

(The report has been updated as of June 11, 2007)

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On October 6, 2006, my report on consequences from the 1959 event at the Sodium Reactor was released to the public, along with other reports commissioned by the Santa Susana Field Laboratory Advisory Panel. Unexpectedly, press coverage extended far beyond the Los Angeles region, causing, no doubt, a public-relations headache for the current owner of the site, the Boeing Company (See cartoon attachment 1). On Nov 3rd, 2006, Boeing responded with a critique that included various statements prepared by its own expert consultants. Overall, Boeing felt that the Panel Reports were unfair to its employees and to the community surrounding the facility. Surprisingly, Boeing offered no explanation for why it refuses to release meteorological wind data for the site, either for 1959 or for more modern periods that could be used in dose reconstruction. Boeing's withholding of data makes its claims of unfairness to the community seem rather hollow. Still, Boeing's technical criticisms must be carefully addressed. [Note added 8/20/2009. Boeing has subsequently released 1959 meteorological (met) data.]

In consideration of the Boeing comments relevant to my report, along with comments I have received from other parties, I have prepared a revised report, which is labeled, Revision 1. A major goal in preparing the revision was to clarify some of the confusion that crept into some press stories. (Revision 1 also incorporates additional soil measurements brought to my attention by non-Boeing experts.)

In Revision 1, I have tried to clarify issues, and to answer questions raised by Boeing and its consultants. I have also made quantitative changes to the report that resulted from adding additional Boeing consultants to the set of experts used to develop a likelihood distribution for the release magnitude. When these changes were combined with the newly identified soil measurements, the quantitative scoping calculations I made of projected health effects had to be adjusted. The upper 95%-confidence value dropped by about a factor of 4. [Note added 8/20/2009. Preliminary review of the met data suggests the upper 95%-confidence value will drop still further, when met data is incorporated. Revisions to expert assessments is likely to reduce the upper 95%-confidence value still more, although the possibility of a second (earlier?) accident has been raised to account for excess strontium found in the coolant— a possibility that will complicate the analysis.] The overall conclusions and thrust of the report did not change, namely that

1) It would be quite difficult to successfully find any signal from SSFL in epidemiological data.

2) The uncertainty in the projected number of cancers is quite large, with zero cancers at one end of the likelihood distribution (25%-confidence limit) and a large number of cancers at the 95%-confidence limit (440 of them). Equal numbers of cancers are projected to occur in the future as a result of doses that have been delivered since 1989 and doses that will be delivered out to 2019, all from groundshine produced by deposits of long-lived radiocesium. [Note added 8/20/2009. As said above,

preliminary review of the met data suggests that the upper 95%-confidence value will drop still further, when met data is incorporated. Revisions to expert assessments is likely to reduce the upper 95%-confidence value still more, although the possibility of a second (earlier?) accident has been raised to account for excess strontium found in the coolant-- a possibility that will complicate the analysis.]

3) The uncertainty in projected cancers is large for three major reasons: meteorology at the site is extremely complex, minimal data was collected at the time of the event, and wind data for the site, both in 1959 and more modern periods, has been withheld by Boeing.

In responding to Boeing's criticisms of my report, I found it necessary to reconsider Boeing's specific criticisms of the report of David Lochbaum, because his opinions were used as one input to my projections of health effects. Lochbaum's source term for radiocesium, in effect, determines the 95%-confidence values in my report.

In addition to the general response to the Boeing criticisms contained in this document, I have also prepared two additional documents that contain section-by-section responses to Boeing's critique and the report of Boeing consultant, Dr. John Frazier. These documents are:

Beyea Response Document 2: Annotated comments by Jan Beyea on Boeing's response to reports released in 2006 by Beyea, Lochbaum, and the Advisory Panel.

Beyea Response Document 3: Annotated comments by Jan Beyea, Ph.D., on the critique by John Frazier, Ph.D., consultant to the Boeing Company, of Beyea's 2006-report.

In the current document (Document 1), I discuss the following issues, listed in the order I rank their importance: 1) areas in which I agree with Boeing, 2) problems with press coverage, 3) omissions from the response of Boeing and their consultants that are conspicuous by their absence, 4) the addition of additional Boeing consultants to the list of experts whose views I take into account in developing distributions for released radioactivity, 5) the possibility that the fuel never melted as argued by Boeing consultants, 6) the possibility that there was no direct path to the atmosphere, 7) the appropriateness of the method I have used to combine expert opinion on releases, and 8) my response to additional, miscellaneous criticisms of my report and that of David Lochbaum.

1. Areas in which I agree with Boeing.

Before responding to critiques made by Boeing and its consultants, I want to address matters on which we agree. In particular, we agree that the range of exposures and resulting risks that I calculated were spread out over the entire LA Basin. The only possible exception are children who drank milk in 1959 from local dairies in the San Fernando Valley, particularly from the 60 or so cows grazing on the Peterson Dairy. These children, who will be very difficult to identify, may have borne a disproportionate fraction of the risk – a risk large enough that it is conceivable it could be detected in an epidemiologic study. With the exception of this subpopulation, risks were not focused on the nearby populations of Chatsworth and Simi Valley. This counterintuitive result occurred for three reasons: First, the release occurred on top of a mountain. Second, for material, like radioactive cesium, to escape as a vapor or airborne particulate, the release would likely be hot, which would cause the material to rise. Both of these conditions are conducive to material staying aloft over nearby communities. Finally, the measurements of long-lived radiocesium made on site, although limited and not entirely trustworthy in the early years, essentially rule out a large release that would have descended into Simi Valley or Chatsworth. Only an elevated release is consistent with the data – a release that would have kept most of the material overhead in Simi Valley and Chatsworth. A good analogy is a waterfall. The water touches ground away from the starting point. The measurements do not rule out an elevated release, which would have focused the risks further out. This distinction between ground and elevated releases, unfortunately, is fudged over in the response of Boeing and its expert consultant, Frazier.

Boeing and I also agree, as stated in both my report and the Advisory Panel's report, that the individual risks are small, with the projected health effects, if they occurred, spread out over a large population in the greater Los Angeles region. Although Boeing and I disagree on the significance of such diffusion, there is agreement on the underlying circumstances.

2. Problems with press coverage.

ISome press stories gave a misleading impression that all of the excess cancers calculated were focused on nearby communities, when in fact only about 10% of the excess cancers projected would have been associated with persons living within 4 miles of the SSFL site. Compounding the confusion, one press article indicated that the excess cancers were confined to a 60 sq-mile area. Here, the problem probably arose over a confusion of distance with area. The results were confined to a 60-mile *radius*, which actually means a 10,000-sq mile area.

As a result, I am concerned that residents of Simi Valley, Chatsworth and other local communities were unduly alarmed. I regret this very much. I take responsibility for this misperception. I should have done a better job at explaining, e.g., by introducing the waterfall analogy and by stating the fraction of the excess projected to arise within a few miles. I apologize for any anxiety caused in nearby communities by my lack of clarity.

I also wish that the press had picked up on the fact that the lower 95%-confidence limit was zero excess cancers. Furthermore, as I said in my summary, there was a 25% chance in my simulations that the total excess cancers were eight in number. In other words, a good chance that Boeing was essentially correct in its analysis. Where I differ with Boeing, both at the time I wrote my report, and after reading the comments of their experts, is in the mean value and the upper limit. I do think there is a significant probability that the damaged fuel actually melted and that a large release of radiocesium occurred.

One press story reported persons consulting attorneys in light of the Panel's report. As one who regularly advises attorneys on the wisdom of taking cases related to offsite exposure from toxic substances, I would caution anyone to think twice before relying on my report as a motivation for bringing a legal case. Toxic tort litigation is a grueling process for anyone involved, especially plaintiffs. My report does not provide, nor was it intended to provide, a basis for thinking that an individual could show with publicly available information that radiation releases from SSFL were a significant contributing factor in a particular individual's disease. For one reason, I did not have access to full site-specific meteorological information for either July of 1959, the time of the accident; nor did I have full meteorological information for later years.

I do think that Boeing, assuming it has legal responsibility for the accident, should donate funds to cancer research in an amount that might be won in a court case for the estimated number of people affected, could they be individually identified. Not all projected cancers have materialized yet, so a subgroup of the affected could still be helped by improvements in cancer detection and treatment. The amount of money that would be involved is large, assuming my mean projection for number of cancers is correct. Given the potential dollar amount and the fact that Boeing disagrees with my numbers, Boeing is unlikely to voluntarily agree with any such proposal. And, our legal system is not currently set up to provide such relief when there are so many free riders and low individual risk, even though such relief might bring this matter to a reconciliation. Nevertheless, I shall continue to promote the concept, in the hopes that the laws will someday be changed.

3. Conspicuous omissions from the response of Boeing and its consultants.

The Boeing response avoids the following issues.

1) There is no mention of the withheld meteorological data. Either for 1959 or for later years in the 1990s. Absolute silence. Why?

2) There is no substantive discussion of the possibility of elevated releases that would have carried radiocesium above the region where soil measurements have been made.[1]

3) Boeing's critique does not mention the fact that I combine all the experts from all sides in developing a likelihood distribution for releases. It suggests that my calculated consequences depend only on the Lochbaum report and/or the Windscale accident, whose relevance to SRE, Boeing disputes. (Note that one of their experts, Dr.John Frazier, does mention my combining of expert opinions, but only to assert without explanation that my method is "obviously" biased. I deal with this assertion by Frazier, later, showing how what I have done is standard practice as reflected in the risk assessment and economic literature.)

4) Boeing is silent about the uncharacteristically low fuel burn-up reported for the July 1959 accident, which has been used by a number of experts to estimate release estimates for radioiodine. In my report, I challenged the magnitude of the reported number

5) Boeing is essentially silent about the possibility that there was sufficient heat released to cause bubbling of the sodium coolant, which would have provided a pathway for radioactivity to pass through the sodium coolant that enclosed the reactor core.[2] As I state in my revised report, the difference between the experts in this debate can largely be explained by whether or not they consider or neglect a bubbling scenario.

6) Boeing is silent about the possibility that the hold-up tanks were bypassed during part of the accident, which coupled with a bubbling scenario, would have provided a direct path to the atmosphere from the damaged fuel.

7) Boeing is silent about what happened to the filters in place during the July run. Measurement of the cesium activity on them, if indeed, filters were in place at the

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[1] In both Boeing's summary and in the report of Dr. John Frazier, it is said that soil measurements rule out large releases of radiocesium, without mentioning my claim about elevated releases. In Boeing's section-by-section comments, they finally mention elevated releases, but only in a throwaway line to the effect that I have provided no evidence that elevated releases are compatible with existing soil measurements. They do not explicitly deny it; furthermore, I do provide such evidence in Tables 2-8 and 2-10 of my original report and accompanying discussion. The revised report is much more explicit in this regard.)

[2] In the only location I could find where Boeing mentions bubbling, it is done as a throwaway line that bubbling would have been less likely to have occurred at SRE. The very fact of mentioning bubbling at all means they are aware of the implications for release.

time and not themselves bypassed, would give an excellent idea of the amount released.

8) Boeing is silent about making further soil measurements at distances of 5-15 miles to resolve the issue of radiocesium release.

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4. Addtion of new experts to be considered in developing source term distributions

Previously, in developing source terms for *radioiodine*, I have included the views of Boeing's consultants in the litigation, on the assumption that their estimated release magnitude was essentially zero. I presumed that Boeing would have found knowledgeable experts to take an independent look at the release probabilities, so they would not have to depend only on the AI analysis. I further presumed that the release estimates would be low, because Boeing's attorneys would not have presented experts who did not come up with negligible releases. I had not included estimates for any Boeing litigation experts on radiocesium releases, because I did not know if they had even considered radiocesium releases.

Boeing has now asked John R Krsul, an experienced chemist who has worked with sodium-cooled reactors, to take still another look. His comments also give us a good idea of what Boeing's litigation experts did with respect to their treatment of radiocesium, so I have now added Boeing's litigation experts in my release distribution for radiocesium. Ksrul's report, which relies heavily on the reports of the litigation experts, makes it clear that it is possible to develop a picture of what happened that is consistent with a negligible release. Krsul is knowledgeable, has published in a field that is relevant, and is well-qualified. He comes to a similar conclusion as the Boeing litigation experts. Moreover, unlike the original AI staff, he has no obvious stake in the outcome of his assessment of likelihoods, other than a consulting fee. Consequently, his analysis should be considered in developing source terms.

However, Krsul's analysis is hardly definitive. First of all, it is puzzling that he fails to provide a list of his own publications; nor is a CV available on the web, as far as I can tell. A search of the literature indicates that most of his available publications deal with damage to materials from irradiation, not fission fragment release, although he must have picked up a great deal of knowledge about fission fragments simply by being at Argonne. As for his substantive comments, he provides no citations for his statements about experiments at Argonne National Laboratory, particularly experiments that reportedly show one needs 4.2% atom percent burnup to get iodine out of the fuel. I could find no report of such experiments on the web or in academic search services, so would have appreciated a citation. I did, however, come across a paper that appears to contradict Krsul's statement:

"Large percentage releases from metal fuel occurred below 1% burn-up and irrespective of the burn-up value down to the lowest value studied (~0.24%), provided the fuel approached the melting point." (Buddery and Scott 1962).

This is a factor of 20 discrepancy with Krsul's burn-up claim. Krsul mentions nothing about temperature or heating rate, nor the range of such variables studied at Argonne. Perhaps, the unknown experiments to which he cites were taken at low temperatures, which would make them not particularly relevant to severe accident conditions. Without his having providing more information, it is impossible to know. Buddery et al. go on to say that these large releases occur in metal fuel as soon as the temperature approaches within 10 degrees of the melting point, for the heat rates they consider. Melting was not necessary, which again contradicts statements by Krsul. Furthermore, Krsul does not take into account the impact on release probabilities of the thermal cycling of the fuel that appears to have taken place, most likely because of the operators continually trying to get higher power out of the reactor over a several week period. Such cycling would have made releases more likely at lower temperatures, although a quantitative estimate cannot be made in the absence of relevant experiments. With sodium bubbling taking place, the operators may have had little idea of the true power being generated in portions of the core.

In addition, It should be noted that the spongy nature of some of the fuel rods, found in post-accident analyses, suggests another reason to believe that escape from the fuel was greater than one would expect based on measurements in intact fuel. Krsul is silent on this point.

The maximum temperature in the SRE coolant was supposed to be 649^{o} C (Starr 1955), but would have risen whenever there was inadequate cooling. The boiling point of sodium is 883^{o}C (http://en.wikipedia.org/wiki/Sodium), so the fuel would have to be hotter than that to produce boiling, which even the AI documents indicate was likely to have occurred in the constricted locations. Analysis of the temperature at the surface beneath sodium bubble formation could well have shown temperatures approaching the melting point of the metal, which is 1132^{o} C. (http://en.wikipedia.org/wiki/Uranium). I found a number of papers on the physics of boiling, e.g., (Bang et al. 2005), but I do not have the time to make the complex calculations that would be necessary to estimate the surface temperature of the metal fuel. It would also be necessary to account for the stainless steel cladding. Suffice it to say that the fact that some of the fuel was found in a eutectic mixture does not mean all of it was in that form. All in all, Krsul analysis leaves a lot of questions unanswered.

At another point, Krsul argues that metal fuel is somehow less susceptible to releases of radioactive fission products than uranium oxide. In fact, the melting temperature of metal fuel is much lower than the 3,000 degree melting temperature of oxide fuel (http://en.wikipedia.org/wiki/Uranium_dioxide). An overheated metal core can get close to melting much easier than an oxide core. Krsul deals with none of these issues.

Krsul does argue that the fuel never melted, but all he can say on that score is that there is a reasonable case that no melting occurred based on the timeline he reconstructs, with damage to the fuel caused at lower temperatures due to eutectic melting. I agree a reasonable or plausible case can be made for no melting, but a reasonable and plausible case can also be made that the temperature of the metal did get close to the melting point. Once vaporization of the sodium coolant took place, the cooling rate of the core beneath the bubbles would have dropped, increasing the surface temperature. Furthermore, Krsul never addresses the possibility that a positive sodium void coefficient might have increased the neutron flux and hence the temperature rise of the fuel, once vaporization caused voids. True, the SRE may have been too small in size to give rise to a positive void coefficient of any significance for releases, but Krusl never addresses this issue.

Without citation to a thermal analysis of the metal fuel, which would be very difficult to perform with any confidence given the complex thermodynamics, fluid dynamics, and neutron physics involved, or without some other argument, it is impossible for Krsul to defend the claim that temperatures stayed at, or around, the eutectic melting temperature throughout the core. Perhaps, there is some argument in the sealed testimony of a Boeing litigation expert to which he cites that is relevant, but Krsul does not present the logic to us. There probably does exist a plausible argument, but the fact that it is kept sealed indicates to me that the argument is likely not to be definitive.[3]

Also, Krsul never mentions the fact that the discrepancy between the amount of radioiodine and radiocesium measured in the sodium coolant, if taken at face value, falsifies his theory that the fission fragments were trapped by the sodium coolant. If his theory were correct, the amount of iodine in the coolant expressed as a percentage of core iodine would have been equal to, or greater than, the amount of cesium found after the accident in the coolant expressed in similar units, because of the greater volatility of iodine. Is Krusl arguing that radioiodine was somehow retained more than cesium? He does argue that the iodine was bound to the metal, but he does not try to argue that a smaller fraction of iodine would have escaped than cesium from the core.

Now, Boeing, in its comments argues that the discrepancy between the amount of radioiodine and radiocesium found in the post-accident coolant is due to measurement error. Maybe, but they provide no backup for such a conclusion. It is an assertion, not an argument.

There are other weaknesses in Krsul's arguments, but they probably are not significant.[4]

[3] The Department of Energy, apparently siding with Boeing's position in litigation, has now put this report on the web. I will not rely on the unpublished portions of this report, until such time as Boeing promises to forego legal action against anyone quoting from the report or otherwise clarifies the legal status of the document. Also, Boeing would need to release plaintiffs' relevant expert reports and rebuttals.

[4] Krsul does not indicate how the fission fragments entering the coolant would have gotten well mixed, without boiling, given the forced, thermal circulation going on Koch M, Brockmeier U, Schotz W, Unger H. 1991. A code for the prediction of sodium and volatile fission product release from a liquid pool into an inert gas atmosphere. J Aerosol Sci 22, Suppl. I: 709-712.. (Accounting for circulation as some papers in the literature do would affect his numbers, but would probably not change the substance of his argument that little amounts of fission fragments would have escaped, if they went directly into the coolant.) . Krsul does not account for the fact that Xenon-137 decays into Cesium-137 Paulson WA, al. E. 1968. ESTIMATION OF FISSION-PRODUCT GAS PRESSURE IN URANIUM DIOXIDE CERAMIC FUEL ELEMENTS., so some Cesium-137 must have been in the high bay area, even under his theory. His language, therefore, could have been more precise.

Despite the limitations and gaps in Krsul's argument, I find his reasoning as plausible as anyone else's on this difficult subject.

Krsul's analysis also is relevant in trying to fill in the middle of the source-term distributions with middle-of-the road experts. The experts explicitly available to us, for a number of reasons, are likely to fall at the two extremes of the distribution of experts that would occur, were experts to be picked at random. Given the new arguments that Krsul has added to the public discussion with his report, it seems likely to me that middle-of-the-road experts would come to lower estimates than I previously assumed. For the one, hypothetical middle-of-the road radiocesium analyst I previously considered, I have dropped the range in half, consistent with the reduction in half of the mean release estimate for radiocesium. This conclusion must be tempered by the fact that we do not have access to plaintiffs' responses to defendants' arguments. Nevertheless, as a result of adding Krsul to the mix and lowering the assignment of middle-of-the road release estimates, the mean estimate of the radiocesium release drops, but there is a relatively minor impact on the upper 95-% confidence limit.

For radioiodine, the median release drops by half, falling to 700 from 1400 curies. The mean drops to 1800 Ci from 2000 Ci. The upper 95%-confidence limit drops to 9500 from 10,200.

Boeing also presented a report by another expert, Dr. John Frazier. Although Dr. Frazier is well-qualified to judge health physics programs at reactors, Dr, Frazier does not appear to have the qualifications that would make him an independent judge of the release estimates, other than via the constraints placed on releases by soil measurements, which he happens to get wrong, because he neglects to consider elevated releases. I had already indicated in my report that soil measurements rule out a large release of radiocesium at ground level, but not an elevated release, which would largely pass over the SSFL site, where soil measurements have been recorded, and some of the closest communities. Dr. Frazier ignored this part of my report, so adds nothing new. I have no explanation for why he ignored this crucial part of my argument.

Dr Frazier refers to his having done a number of risk assessments in his career, however, not a single one is cited in his list of publications in his resume. Yet, he claims to be an expert on risk-assessment science. Presumably, he is referring to unpublished, litigation reports that he has done on behalf of employers. It is hard to judge unpublished litigation reports and the degree of peer-review they have received. As for publications in the scientific literature, he only lists a total of 5 publications in his resume of any kind, of which only one is a peer-reviewed journal publication. And that appears to be a reprise of his Ph.D. thesis in chemical physics, which also makes up one of the five publications listed. Also included in his list of five publications are two government reports he did on x-ray measurements. Finally, he participated in a National Research Council study on film-badge dosimetry. His resume is far too thin to support the wide range of subjects on which he opines, including reactor physics, reactor containments, expert elicitations, the field of risk assessment, epidemiology; nor does he support his opinions with citations to independent sources in the scientific literature, which would make up for his lack of publications in those fields. His job experience indicates that he is well-qualified to set up and review health physics and radiation measurement programs established at nuclear reactors. Were that the subject of an expert elicitation, I would certainly include his opinions in developing a likelihood distribution. Not so for source-term estimates. His analysis amounts to praise for the AI staff after the accident. He is impressed that AI formed an internal committee to investigate itself. He takes everything AI writes at face value and as truth. He ignores all the inconsistencies in the record and criticisms that have been made of AI. He ignores the false press release put out by AI. He ignores the fact that Boeing, today, refuses to release meteorplogical data. If this is the level of critical analysis he brings to reviews of health physics programs, I cannot see how he can be considered as anything other than a distraction in trying to develop a source-term distribution for SRE. His praise does not raise my confidence in the AI staff, who had everything to lose from finding that a significant release occurred. He should be aware that his one-sided treatment makes him look like a hired gun. I will deal with his assertions about the inadequacies of my report and that of David Lochbaum's in a later section. In this section, I am simply explaining why I do not include Frazier as one of the experts I use to develop radioiodine and radiocesium source-term distributions.

5. The possibility that the fuel never melted.

Of all the comments presented by Boeing, the most important ones for my report revolve around the discussion of fuel damage, which cite expert reports prepared for defendants in litigation recently settled. Using my Bayesian methods, I have to make an assessment (weighting) of the various expert reports that are available. Although I do not want to second-guess an expert, I must make sure each report is reasonable – a form of forensic due diligence. In the case of the report of David Lochbaum, I reviewed his report and the underlying literature and concluded that he had made a reasonable case, as had others who came up with different numbers. I did discount the likelihood of Lochbaum's higher release numbers somewhat to account for the fact that he did not take into account the implication of ground-level radiocesium measurements made onsite and in the immediate vicinity. However, I implicitly assumed that the damaged fuel, or at least the bulk of it, had melted or at least reached a high enough temperature that cesium would migrate. I further assumed that sufficient heat was generated to produce bubbling of the sodium coolant. If either of those assumptions were false, the release fraction of radiocesium could be considerably reduced and the likelihood of Lochbaum's estimate would have to be correspondingly discounted, lowering both the mean number of estimated excess cancers and the upper 95%-limit.

John Krsul offers a number of theoretical arguments why Lochbaum's source term is either inconsistent with data he did not consider or has not been fully justified. He argues that those who find large releases are relying on intuition gained from experience with oxide fuels, not metal fuels. He states essentially that, unless the metal fuel is burned up by 1%, there will not be enough voids and cracks to allow release of fission products without melting. He cites litigation experts for the idea that the fuel did not actually melt. Ergo, no release. Although the reports to which he cites and plaintiffs' responses are restricted, I presume Boeing's experts have made a plausible case that the damage to the fuel can be explained by thermal recycling and formation of eutectics. Nevertheless, plaintiffs' experts, Makhijani and colleagues, did not fold when faced with the arguments of Boeing's experts. They maintained their position that radioiodine escaped, as evident by their public comments after the litigation was long underway (LOE 2006). Thus, even without considering Lochbaum's response to the Boeing critiques, I presume there must exist a plausible counter argument. Furthermore, I have already listed earlier in this document the many gaps that exist in Krsul's logic.

If the fuel did melt or was kept near melting temperature for long periods of time, then Krsul's arguments lose their force. Fission products could have been released into gas bubbles; escaping in gaseous or aerosol form. One way the fuel could have overheated was due to a failure of sodium to reach the surface of the fuel. One contemporaneous analyst was amazed at the cavalier actions taken by the operators (Thompson and Beckerley 1964). The reported burn-up during the period makes no sense, given the much higher values reached in similar periods prior to the SRE. The ratio of radioiodine to radiocesium measured in the coolant after the accident has the opposite value that would be expected, were Krsul's and Boeing's theory of the accident fully correct. We really do not know what was going on during those two weeks. Plausible speculations can be made, but we must bear in mind lessons that have been learned about the performance of experts in hindsight: they tend to be overconfident and underestimate the wings of the probability distribution (Cooke 1991), (Otway and von Winterfeldt 1992). That is one reason that the use of expert elicitation is undertaken and one reason I am very wary of throwing out anyone's independent assessment. When expert elicitations are used to assess a quantity, the resulting range can be enormous, way outside the ranges assessed by individual experts (USNRC 1995). Although a formal expert elicitation cannot be made of SRE releases due to the complexity and expense that would be required, the use of opportunistic assessments is the next best alternative in my view.

As a result of all these considerations, I cannot rely on Krsul's arguments to reject Lochbaum's report for use in building the source-term distribution. That does not mean that I am perfectly happy with the mix of experts with which I have to work. I remain concerned about the fact that I am lacking real, middle of the road experts, which is a limitation to my approach. However, rejecting Lochbaum or Boeing's consultants will not correct the problem, and will serve to destroy the assessment of the width of the range of views. Furthermore, there are only a limited number of likely sources of experts in this field. They will come from industry, government laboratories, nuclear engineering departments at universities, NGOs with staff with nuclear experience, and isolated independent consultants. With the addition of Krsul as a representative of government labs, I already have samples from most of these sources. Consequently, I do not think the lack of middle-of-the roaders is a crucial limitation. True, other analysts using these same methods might end up with mean values that were lower or higher, but I doubt very much that the 95%-confidence ranges would change by much.

Krsul argues that Lochbaum's choice of a best estimate within the range is soft, so I have explored the sensitivity of changing the location of the peak of the triangular distribution I have assigned to his range of estimates. Lochbaum took the arithmetic mean of his range as his best estimate. Other analysts, looking at the wide range, might have considered the distribution log-normal, and might have taken the geometric mean of the range. Doing so has the following consequences as listed in Table 4-2 of my revised report. When the centroid of the distribution is taken as the geometric mean of his range, rather than the arithmetic mean favored by Lochbaum, the average number of predicted excess cancers drops from 50 to 45 and the value at the upper 95%-confidence limit drops from 440 to 375 cancers. These are not significant changes.

6) <u>The possibility that there was no direct path for airborne material to reach the atmosphere</u>.

In its section-by-section critique of Panel reports, Boeing argues 1) that the only path from the top of the sodium coolant was through the hold-up tanks, and 2) contemporaneous records tell us when the tanks were vented. The first claim appears to be false based on the design of the reactor. The hold-up tanks can be bypassed, which would provide a direct path to the atmosphere, albeit through a filter, if one were in place and not bypassed. Boeing does not mention the possibility of hold-up-tank bypass. Under normal operation, high levels of radioactivity would have triggered automatic diversion to the hold-up tanks. However, it is my understanding that the automatic system failed prior to the accident and diversion to the hold-up tanks had to be triggered manually from the control room. It is my further understanding that rapid changes in measured radioactivity suggested even to contemporaneous reviewers of the accident that hold-up-tanl bypass mode was in operation during the July 1959 event, i.e., the operators were not always manually switching the system properly. This would not be surprising, given the stress of those two weeks.

As for the claim that contemporaneous records tell us when the hold-up tanks were vented: It is not definitive, if the hold-up tanks were bypassed during some or all of this period. Nor, can we be sure that records were kept for the entire period of the event.

<u>7. Frazier's challenge to my method of combining expert opinion on releases</u>

Frazier has challenged the way I have combined expert opinion. He says I am biased towards the high release-side but gives no details, no citations, nor any idea of how biased he thinks I am, which makes a response a bit difficult. The combining of expert opinion has a long history in the scientific, engineering, and management literature (Clemen and Winkler 1990), (Genest and Zidek 1986a), (Otway and von Winterfeldt 1992), (Sandri et al. 1995), (Myung et al. 1996). The goal has been "a method of "averaging" the possibly diverging opinions of a group of analysts and a limit theory for the long run." (Genest and Zidek 1986a). Genest and Zidek have presented an annotated biography through 1986 (Genest and Zidek

1986a). As with most fields, there are differences of opinion (Genest and Zidek 1986b); there is no single way to combine expert opinion. The approach I have taken is a form of "linear pooling of probability densities," which is standard. It has its critics, but it is intuitively simple to understand and is known to be relatively insensitive to the choice of expert weights (Genest and Zidek 1986a), which is an important advantage in the (contested) SRE situation.[5, 6] I have also made estimates of bounds of the distribution using "possibility" theory (Sandri et al. 1995).

Frazier is free to use his own method, which apparently involves ruling out studies completely that don't use the method he prefers. Genet and Zidek caution against this approach, however:

> "Whatever scheme is elected for assigning the weight….,zeros will constitute vetoes and unduly great emphasis will tend to be placed on the opinions of single individuals." (Genest and Zidek 1986a)

I did find one paper on the subject of combining expert opinion (Kaplan 1992) that would probably be more satisfactory to Frazier, because it focuses on eliciting expert views on the science underlying releases, rather than the release magnitudes themselves,. I suspect this would lead to lower estimates at SRE.[7] The danger, however, is that the method requires that the basic science and the release pathways be well understood – conditions that I don't think are met at SRE. The methodology constrains the experts to fitting into a scientific framework that may be incomplete. It begs the question of who it to establish that framework. And what happens, if the experts don't agree on the framework?

In any case, third parties can make their own judgment as to which approach, mine or Frazier's, appears most useful to them. Hogart suggests that the decision context should guide the expert-combining

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5 The choice of expert weights is obviously a difficult aspect of combining expert opinions. In the language of Genest and Zidek, I am playing the role of a "supra Bayesian." It is thus the seemingly impossible task of this supra Bayesian decision maker to evaluate the individuals, their prior information sets, the interdependence of these information sets, the experts' "calibration" or honesty, etc." Genest C, Zidek JV. 1986a. Combining Probability Distributions: A Critique and an Annotated Bibliography. Stat Sci 1(1): 114-135.. Given the difficulty of the task and the fact that my objectivity has been challenged by Boeing in its response, it is better that the pooling method be relatively insensitive to weights assigned to experts.

6 Genest and Zidek prefer logarithmic pooling, which I have not implemented in this situation, because it is more sensitive to the choice of expert weights. [logarithmic pooling] "makes much less sense if the responsibility of the pooling is that of an external decision maker, since the arrival of new data might change his/her evaluation of the relative expertise of the subjects who were consulted" Ibid.. Furthermore, logarithmic pooling produces mathematical problems when trying to take logarithms of zero. In effect, the logarithmic pooling would force all probabilities to zero, if any one expert had a zero for a release at a certain magnitude. It provides a veto power to Boeing's experts on any other release distribution.

7 I suspect application of this method to the SRE set of experts would lead to a much lower upper limit for the release of radiocesium and, therefore, a lower value for the upper limit on health effects. Why? Because by breaking up the release magnitude into a product of terms, there would be multiplication of a series of likelihood distributions, each of which would have a low value for values contributing to high releases, given the views of the experts who have looked at the SRE. Hence, there would be a multiplication of small numbers, particularly so for the radiocesium distribution. As a result, the cancers projected for radioiodine would tend to dominate the overall results, which, in turn, could drop by more than a factor of ten, possibly a factor of one hundred. However, this disaggregation approach has its dangers. In accident risk assessment, it ignores what are called, "common mode failures." In the SRE case, it ignores the possibility that the physics and chemistry of the event, and/or the history of operator actions, may be poorly understood by the experts.

process (Hogart 1986). In the opinion of Genest and Zidek, "…Bayesian methods provide the sole normatively acceptable answer to the aggregation problem when the group reports to a third party." That is the situation I face with my report. I am reporting to a series of third parties: primarily epidemiologists who need to decide about the feasibility of any epidemiological studies, but also to the general public, legislators and regulators, who might want to take some action in response to the accident at the SRE or bring some closure to their concerns.

For reasons of resource limitations, I have not been able to undertake a formal expert elicitation (Keeney and Von Winterfeldt 1989), (Keeney and Von Winterfeldt 1991), (Otway and von Winterfeldt 1992), which would have gone beyond simple pooling of published opinions. Expert elicitations can be very expensive (Hoffman and Kaplan 1999). Furthermore, given the entrenched positions already taken by experts in litigation, it is unlikely that many of the experts would be in a position to enter into an expert elicitation, with its formal set of questions, that might lead them to change their views.

There is a potential problem with the high degree of overlap that exists between some of the expert assessors, especially between the Boeing litigation experts, AI analsysts, and Krsul.[8] One implication of the work of Clemen and Winker "is that overlapping information may not be terribly useful; one may wish to assemble a set of experts whose information overlaps less, on the grounds that there will be more information available from the group whose information is less interdependent." (Clemen 1987)

However, adjusting for overlapping experts would add still more complexity to an analysis that already is puzzling some readers, based on feedback received. Furthermore, adjusting for overlap is likely to be included within the bounds set in the next section using "possibility" theory

Sandri et al. have proposed a range of alternate methods of combining expert probability distribution using fuzzy-set methods involving "possibility distributions." Possibility distributions can be rigorously related to probability distributions, in which case they can be taken to be lower or upper probability bounds (Sandri et al. 1995).

One extreme approach considered by Sandri et al. involves taking the minimum probability assigned by all experts at any particular value of the distribution. Since there is at least one expert in our case who assigns zero probability to all releases above zero, this "lower bound" distribution is identical to the Boeing and Frazier position, which means essentially zero curies released. At the other extreme, Snadri et al. take the maximum probability assigned by a member of the group of experts at any one release value. This approach tends to eliminate duplication among experts with similar opinions. As a sensitivity analysis, I made calculations using this "max" approach for the SRE cesium release. The overall release

[8] "First, experts are located in networks of other experts, and these connections probably influence their methodological decisions and the assumptions that underlie them. Second, experts are embedded in structures of authority-within business and government-and thus the organizational site in the production of expert knowledge is an important factor in determining the relative disinterest of expert judgments. Finally, at a more general level, experts are structurally bounded by others in a scientific community with whom they share normative frameworks regarding what is counted as truth, and what as fiction." Clarke L. 1988. Politics and Bias in Risk Assessment. Soc Sci J 25(2): 155-165.

values are higher than calculated by linear pooling, resulting in an average value of 370 curies, an upper 95%-confidence limit of 1800 Curies, with a median value of 50 curies.

In summary, a range of independent methods of combining expert cesium release distributions results in the conclusion that cesium releases could range from essentially zero to 1,000-2,000 curies at a 95%-confidence limit.. The same stability of conclusions holds for radioiodine releases, when the methods of Sandri et al are applied to the radioiodine distributions.

8. Miscellaneous criticisms of my report by Dr. John Frazier.

8a. Qualifications of Dr. Frazier to make unsupported assertions. Dr. Frazier says my report is not good science. Presumably, to make such a charge, one would have to have some substantial credentials in the relevant science. Frazier does not, as I have discussed above. He is certainly qualified to discuss radiation instrumentation and measurements, as well as health physics programs; beyond that he has no publication or employment record to justify unsupported assertions about the science in my report and that of David Lochbaum. When he tries to provide actual critiques, they are without merit, as I discuss below.

Dr. Frazier claims to have expertise in risk assessment; yet there is not a single risk assessment in the list of (5) publications on his resume. Perhaps, he has done confidential risk assessments for corporate clients, but we have no way to judge their merit or subject area, because they are not listed on his resume. I know from my own experience that he has done risk assessments for defendants in litigation, but these reports are kept secret. Frazier doesn't list them on his resume. Although providing expertise in litigation is important for our legal system, reports prepared for litigation are not generally taken as scientific credentials. Usually, in litigation, attorneys try to retain an expert with a mile-long resume filled with scientific publications relevant to the subjects at hand that give the opinions presented some credibility. In Dr. Frazier's case, there must be another reason why he is retained. In fact, only one of his five listed publications is found in a refereed journal. It is a follow-up to his thesis in chemistry, with no relevance to nuclear physics.

Would his status as a certified health physicist qualify him as a judge of science? I don't see how, because a certified health physicist is not necessarily a scientist. No advanced degree is required. A certified health physicist has passed an exam, making the person qualified to run a health physics program at a nuclear facility.

Nor, does the year he spent as president of the Health Physics Society qualify him as a judge of science. He himself is an example that a HPS president does not have to have a strong scientific resume to be president of that organization.[9]

[9] Here is a quote from an editorial by the current president of the Health Physics Society, Brian Dodd, that demonstrates the kind of person who is acceptable to the Health Physics Society as a leader: "I used to argue tongue-in-cheek that we needed more TMIs and Chernobyls, so that they became as common as traffic accidents and people didn't worry about them anymore. But I admit that that's a bit of a drastic, not

In reality, it appears that the bulk of Dr. Frazier's comments are the equivalent of a political attack-ad and a summary of arguments made by other Boeing experts. The attacks come in the form of judgmental language like, "bad science," "disingenuous," "biased," with no supporting arguments. I welcome vigorous attacks on my work and/or biases, for science is contested territory and all experts have personal biases, but I expect some accompanying analysis behind the attacks. That way, I, and other readers can learn from the criticisms and improve for the future. Frazier does not provide backup for his attacks other than to say I don't accept the arguments of Boeing's litigation experts.

When Dr. Frazier writes within his area of expertise (radiation measurements), he ignores what I actually said in my report. For instance, he never considers the possibility of elevated releases from the SSFL.

Attack reports prepared by experts are a typical strategy I have seen used in litigation, where some experts try to use judgmental language to weaken the arguments put forth by experts hired by the other side in the hopes that the presiding judge will rule favorably on motions to dismiss their opponents reports as bad science. This is a strategy favored by defendants, because plaintiffs have the burden of proof in the courtroom. Not so in the court of public opinion. There, I think the analysis of Boeing's other expert, John Krsul, who brings new ideas into the debate, whis more effective.

8b. Reliance on historical documents. Dr. Frazier cites historical documents from Atomics International as his authority on releases from the facility and argues that I too should accept them as authority. He has no question about data quality, despite the unavoidable evidence of a cover up, some of which even Boeing concedes in its remarks on the 1959 press release from Atomics International. Frazier says such questioning of data authority does not belong in a scientific document. Yet, in effect, I have prepared a forensic report for the Advisory Panel, which must include the possibility of a cover up.

8c. Summing the effects of small doses/Scientific committees. In criticizing my summing the projected health effects of a large number of low doses, Frazier cites a position paper by the Health Physics Society (HPS) that recommends against such summations. To put the statement quoted by Dr. Frazier in context, it is useful to look at its history. The position statements of the HPS are viewed by the Society as political tools, namely, "an important mechanism for the HPS to become more assertive and positive in dealing with Congress, regulators and the public media" (Boerner and Kathren).

> "One of these [position statements], 'Risk in Perspective,' adopted in January 1996, noted that as radiogenic health effects have not been observed at doses below 10 rem, estimation of adverse effects below

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to mention expensive, way to dull the public's sensitivities."Dodd B. 2007. Taking the terror out of terrorism. Health Physics News 35(3): 2.. Dodd's focus is on the expense of accidents like Chernobyl, not the thousands of thyroid cancers that were induced. Or, perhaps, Dodd is a denier of Chernobyl-induced thyroid cancer.

> this level is "speculative" (HPS 1996). Although the statement also recognized the possibility of health effects at lower doses, it recommended that risk estimates be limited to persons with lifetime exposures in excess of 10 rem, or an annual exposure in excess of 5 rem. This position statement, noteworthy not only for its succinctness and directness, was also noteworthy for the rather strong negative reaction it engendered in some of the HPS membership, who felt that the position put forth by the Society was inconsistent with the LNT hypothesis and ALARA. This was a minority viewpoint, and the Society position was buttressed by a position paper issued in March 2000, which stated that compensation for radiation injury should not be made to persons with lifetime doses below about 10 rem (HPS 2000)." (Boerner and Kathren).

The idea of limiting risk estimates and compensation to doses above 10-rem is a public policy statement, not a scientific one. Because so many of the HPS members work in the nuclear power industry or government laboratories that support the nuclear industry, the HPS has a conflict of interest when it comes to making policy statements that affect the nuclear industry.

The rationale given by the HPS for its position is that there is uncertainty below 10-rem about the magnitude of the risk. According to the HPS and Frazier, the risk below 10-rem *might* be zero. In effect, the HPS and Frazier are saying that, when there is uncertainty, society should err on the side of the employer, not the worker or the public. No extrapolation to lower doses is to be tolerated when making risk assessments. The HPS statement does not discuss the other side of the uncertainty coin, namely that the dose response below 10-rem could be "supra-linear," i.e. increase faster than expected from a linear extrapolation from higher doses. Statements like the one Frazier quotes approvingly and the resulting testimony of the President of the Health Physics Society (Otto Raabe), in which he spoke against compensating atomic veterans unless doses exceeded 10-rem,[10] have made me embarrassed to be a member of the Health Physics Society. However, it is the only way to subscribe to the peer-reviewed journal, Health Physics. Could I obtain the journal in any other way, I would immediately resign.

Congress did not accept the recommendation of the HPS on risk assessment and compensation. Today, the NIOSH-IREP calculator (https://www.niosh-irep.com/irep%5Fniosh/) is used to determine compensation for veterans who are not automatically covered. The calculator takes into account uncertainties in both dose assessments and risks of such doses. There is no dose cutoff, only a "probability of causation" threshold. Doses can be below 10-rem for certain diseases and age at exposure. The Boeing critique of my report cites a recommendation made by the International Commission on Radiation Protection (ICRP) that is similar to the HPS recommendation.[11] It, too, is a policy recommendation. Furthermore, ICRP is made up of representatives from organizations like the HPS with conflicts of interest over nuclear power. In effect, the ICRP, Boeing, and Dr. Frazier are opposing the program set up by the

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[10] http://www.lib.ncsu.edu/congbibs/senate/105dgst2.html

[11] However, the ICRP hedges its bets by recommending in the technical appendix a matrix approach that includes some entries for population doses at very low doses. http://www.icrp.org/docs/ICRP_Recs_02_276_06_web_cons_5_June.pdf

US Congress. While I respect their rights to express their views, Dr. Frazier failed to provide the reader with the full story.

In quoting the policy recommendation of the HPS to avoid risk estimates below 10-rem, Dr. Frazier also failed to note the strong minority viewpoint surrounding these policy statements. To many of us these statements are ostrich-like. Also, the epidemiological science has passed these statements by, since recent studies show health effects down to 2-rem. See Figure below, which shows the dose response found in the Techa River cohort. Reprinted from (Krestinina et al. 2005).

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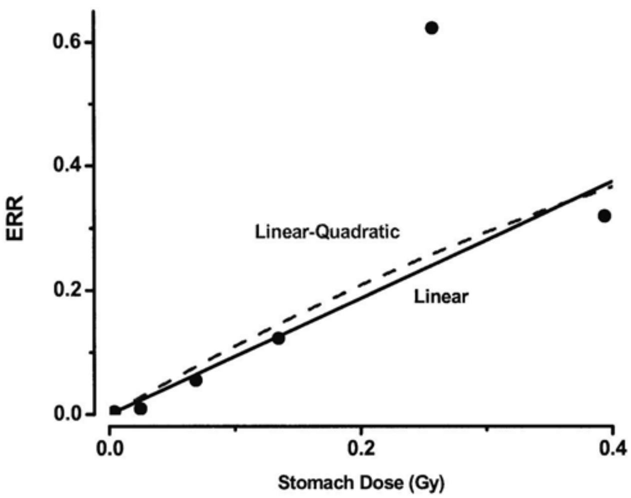

FIG. 1. ETRC solid cancer dose response.

When I first became involved in nuclear risk assessment, the supposed limit where cancer induction had been detected, as quoted to me by people in the nuclear industry, was 50-rem. It is now down to 2-rem.[12] The more data are gathered, the lower the detectable level falls. Has the HPS revised the thrust of its 1996-policy statement? No.[13] Has the ICRP revised its position? No. Yet, according to the latest epidemiological study of nuclear workers (Cardis et al. 2005), a 10-rem exposure corresponds to a 10% increase in cancer mortality. Similar findings turned up in studies of the civilian Techa cohort, which

[12] This is on top of 5-rem in lifetime whole-body radiation from natural background radiation. So, the actual net radiation level in this latest study is 7-rem. Those who expect there to exist a threshold for health effects from human-induced radiation now have to postulate a sharp decline between 7 and 5 rem.

[13] The 1996 HPS statement was reaffirmed in 2001. It was modified in 2004, but there has been no change as far as I can tell to the quantitative recommendation

show linear dose response down to 2-rem (Krestinina et al. 2005). Frazier completely avoids my extensive discussion of these new studies, despite the fact that they falsify the scientific assertions in the HPS study.

The HPS position statement, in recommending against performing any risk assessment on doses below 10-rem, is saying that increases in cancer risks of a population by as much as 10% are not worth legal, political, or public health attention. This raises the question of where the loyalty of the majority of health physicists lies. With the health of the nuclear workers they are supposed to protect or with their employers?

Anyone who thinks about taking the policy recommendations of the HPS and the ICRP seriously should be aware of their history and their conflicts of interest.

Here is how I have ranked information provided by expert committees in developing my report: First, the Committee on the Biological Effects of Radiation (BEIR Committee) of the National Research Council and the Institute of Medicine. All committees involved with radiation health effects are involved with politics to some extent, because so much is at stake. However, the National Research Council and Institute of Medicine's process of choosing members and reviewing the draft reports has led to reviews of radiation that best reflect the literature, in my view. As an advisor to one of the National Research Council's Divisions, I am familiar with, and comfortable with, the way individual biases are identified and balanced within study committees. Of course, there can be exceptions, and few scientists will agree with everything in any report, but usually the BEIR reports draw widespread praise. Next in my hierarchy of radiation committees comes the United Nations' Scientific Committee on the Effects of Atomic Radiation (UNSCEAR). Third, I place the National Radiation Protection Board. In last place, I put the International Commission on Radiation Protection, which Frazier cites. I suspect that Dr. Frazier and I would disagree on these rankings. Nevertheless, I believe that I have reflected all points of view in my distribution of health effects, which has zero cancers as the lower end of the range. It is Dr. Frazier who only reflects one side of the SSFL debate. Nevertheless, in my revised report, I have added, a 10% probability that there are no health effects at the doses of interest at SSFL.

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