Beyea Response Document 3

Section-by-section response to critiques of studies of the 1959 accident at the Santa Susana Field Laboratory made by John R. Frazier on behalf of the Boeing Company

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Consulting in the Public Interest
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"Feasibility of developing exposure estimates for use in epidemiological studies of radioactive emissions from the Santa Susana Field Laboratory"
(See www.ssflpanel.org.)

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Note to reader: All sections in italics are by Beyea. Other text is by Frazier.

In this document, I provide a section-by-section commentary on a report by Dr. John R. Frazier (Frazier 2006) in which he criticized the report I prepared on the 1959 accident at the Sodium Reactor Experiment located at the Santa Susana Field Laboratory (SSFL). Furthermore, because he implies that I was mistaken to include David Lochbaum in the mix of experts used to generate likelihood distributions for releases of radioactivity, I also respond to criticisms made by Dr. Frazier of the report of David Lochbaum (Lochbaum 2006). My section-by-section response complements the generic comments I have included about Dr. Frazier's report in Document 1, "Summary response to critiques made by the Boeing Company and its consultants" (Beyea 2007).

Introduction. *Let me say at the outset that I respect Dr. Frazier, even though I will argue over and over again in what follows that he has way overstepped his areas of expertise and has not provided citations to compensate for his lack of expertise in those areas. After reading Dr. Frazier's report, the first question I had to ask was, "Is Dr. Frazier the kind of expert who I should include in my linear pooling of experts for source terms? I concluded he was not, because he relies solely on experts whose opinion I have already included and because he has no documented expertise of his own in the area of source term that would make his professional judgment of value. I discuss my reasoning below. However, had I included him in the source-term pooling, there would have been an insignificant change in the upper 95%-confidence limits. Note that I have added the other expert retained by Boeing, John Krsul, to the group of source term experts whose opinions are pooled.*

The second question I had to ask after reading Dr. Frazier's report was, "Does Dr. Frazier's critique of my report and that of David Lochbaum's, or any other comments he makes, warrant changes in my report?" I concluded, as I discuss below, that no substantive changes were warranted; however, I also concluded that I should have added more explicatory material about linear pooling of expert probability distributions, which I have done in the revision to my report.

Dr. Frazier's main substantive criticism of my report is that I do not properly account for soil measurements in limiting releases. Soil measurements lie within Dr. Frazier's expertise, so I have taken his opinion very seriously. However, as I discuss below, to reach his conclusion, Dr. Frazier steps outside his expertise and into the area of meteorological dispersion modeling, which is one of my major areas of expertise. That's fine, as long as he provides backup by citing to the relevant scientific literature to justify his analysis. He does not. In fact, he makes a false *assumption about elevated releases of a pollutant, namely that soil deposition always declines with distance. Moreover, I have already discussed in my report how soil measurements tend to rule out a ground-level release of several hundred curies of radiocesium, but not an elevated release of this magnitude. Thus, Dr. Frazier and I already agree that soil measurements should be used to restrict releases of radiocesium – an area of agreement that he ignores, writing as if I had ignored soil measurements. In my report, I used meteorological models that predict soil concentrations to restrict radiocesium releases to the elevated kind. Dr. Frazier uses an assertion, based on a false premise, to reject all large releases. There is no way to quantify Dr. Frazier's opinion other than to put the release to zero, a possibility that is already included in the source term pooling of a number of experts. I do not think a falsely premised opinion should be grounds for my adding another zero-release opinion to the mix.*

On the other hand, there have been new measurements that have come to light after both of us have completed our reports. These measurements, although spotty, were made at distances sufficiently far from the release point that they can be used to set constraints on elevated releases, which I have done in my revised report.

In many of Dr. Frazier's other criticisms of my report, he makes repeated assertions of bias or error without offering any backup or explanation. These are very difficult charges to defend against, or to use as a guide to finding errors in my work, because there is no basis to analyze and discuss. I am left repeating over and over again the fact that he provides no basis for his assertions. I also am forced to examine his resume, not just in the area of source term, but also in terms of risk assessment to see if he has the credentials to allow him to speak with authority from "on high." (Turns out, as I discuss below, he really doesn't, except in limited areas. Like most scientists, he needs to provide citations or quotations to back up his statements, especially when working in an interdisciplinary field like health risk assessment. Alternatively, he could have provided a roadmap to his conclusions that other analysts could judge.) The rhetorical technique he uses, "argument by repeated assertions," is standard practice used to puff up attacks against experts as part of, so-called, "Daubert" challenges that are often made in Federal Court.[1] *Dr. Frazier does have extensive experience in defending corporations against radiation-related lawsuits.*[2] *In fact, he received an award from the Health Physics Society, in part, because of his litigation work:*

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[1] *For a discussion of the scientific issues raised by the Daubert case, see (Beyea and Berger 2001). A lack of familiarity with, and misconceptions about, the actual workings of the scientific enterprise (See (Goodstein 2000) for a reality check) makes it relatively easy to undermine lay confidence in an expert who is following normal scientific practice (Beyea and Berger 2001).*

[2] *E.g., (Frazier et al. 1998)*

> *"He continues to influence: the public's perception of radiation safety programs in this country through his presentations before special interest groups and his court appearances in radiation litigation cases."*

In other bios that Dr. Frazier has prepared, he indicates that he "serves as an expert witness and advisor in radiation litigation cases" (CETS 1989). I applaud scientists who serve in the legal system and regularly encourage more to do so. There was no need for Dr. Frazier to leave out these activities in the bio he provided with his response to the reports of David Lochbaum and myself.

However, neither Dr. Frazier nor I are involved in a lawsuit over SSFL; we are involved in a scientific debate about historical events at SSFL, with the public and regulators as onlookers. I expected more from Dr. Frazier in the SSFL context. Even the Boeing critique of my report and that of David Lochbaum do not, with a few exceptions, rely on unsupported assertions. Nevertheless, I want to make it clear that I welcome vigorous attacks on my report and possible biases reflected in it, provided some analysis accompanies those attacks. After all, science is contested territory:

> *"..science is, above all, an adversary process. It is an arena in which ideas do battle, with observations and data the tools of combat. The scientific debate is very different from what happens in a court of law, but just as in the law, it is crucial that every idea receive the most vigorous possible advocacy, just in case it might be right." (Goodstein 2000).*

I caution any readers of Frazier's report to look carefully for the evidence he provides, if any, in support of his assertions. Alternatively, I urge you to read the section-by-section responses that I make below.

In what follows, I have underlined the sections of Dr. Frazier's text to which I want to respond. My responses, which are numbered and indented, are inserted into the text after the underlined sections and italicized. Hopefully, this approach will make it easier for the reader to focus on the issues in dispute. Note that the page numbers at the top of the page are mine; page numbers at the bottom refer to Dr. Frazier's original report.

Note to reader: All sections in italics are by Beyea. Other text is by Frazier.

REPORT OF JOHN R. FRAZIER, Ph.D.
Certified Health Physicist
November 4, 2006

I. INTRODUCTION

At the request of The Boeing Company, I have reviewed extensive information pertaining to the radiological aspects of an incident that occurred in 1959 involving the Sodium Reactor Experiment (SRE) at the Santa Susana Field Laboratory (SSFL). I have also been asked to review the October 5, 2006 reports of Dr. Jan Beyea (Beyea 2006) and David A. Lochbaum (Lochbaum 2006) to the SSFL Advisory Panel and provide my professional opinions regarding the scientific and technical content of those two reports.

->In the following report, I *(Frazier)* present a summary of my qualifications to give opinions in this matter<-, the opinions that I have formed regarding the radiological issues associated with the 1959 SRE incident and the bases of those opinions, and the scientific and technical content of the reports of Dr. Beyea and Mr. Lochbaum.

***Beyea Response 1*.** *What are Dr. Frazier's qualifications that justify his ability to give opinions in this matter about the technical content of my report and that of David Lochbaum's? His resume makes it clear that his expertise is limited to dosimetry and basic health physics. He lists, "Certified Health Physicist," right after his name, an indication that he thinks being a health physicist is a major aspect of his expertise. The Health Physics Society's web site lists a variety of categories for health physicists. Dr. Frazier has had some experience with almost all types early in his career,[3] but for the last 25-years or so, he has been what the Health Physics Society calls an, "industrial or applied health physicist." These persons "draw upon their technical knowledge and varied experience to advise and make recommendations to management regarding methods and equipment for use in radiation work." Health physicists like Dr Frazier, therefore, are heavily aligned with,*

[3] *Dr.Frazier has one paper in 1978 that fits under the category, "research health physicist." He was a "Regulatory health physicist," when he worked for the FDA from 1978 to 1980. He has also been an "educational health physicist" in preparing course material for newer health physicists. "Health physicists working in education develop and instruct training programs for future health physicists. They also provide any necessary training for radiation workers and the general public. These individuals instruct workers and other health physicists on the level of risk associated with particular radiation sources and methods used to reduce risk. One goal is to help individuals understand the relative degree of risk of radiation exposure. In most cases, the risk is no greater than that found in other industries."*

and dependent upon, management.

A striking evidence of the loyalties of the Health Physic Society is the statement contained in one of the Society's position papers:

> *"The Society believes occupational radiation-safety standards and regulations have been sound, and protective of radiation workers, since the mid-1950s (HPS 2000). "*

As evidence for this statement, the position paper relies on the "absence of evidence," claiming that the "most reliable" studies of workers has not found evidence below 10-rem. Absence of evidence is not evidence of absence. Given the latest epidemiological studies of the Techa River Cohort, it is hard to see how the Health Physics Society could pen such language, were they to try to revise this now outdated position paper, which they appear to be in no hurry to do.

The HPS web site goes on to say, "The health physicist also assists engineers and scientists in designing facilities and new radiation-control programs." Thus, a health physicist is separate from an engineer or scientist, according to the HPS web site. It is not necessary to have an advanced degree to be a certified health physicist (http://www.hps1.org/aahp/abhp/prospect.htm#Prof-Resp). Two written examinations must be passed. Required experience can be in any one of a wide variety of categories. Breadth of experience is not a requirement. Approximately, 20% of health physicists have Ph.D.s according to the annual salary survey, a category into which Dr. Fraziser, of course, falls.

The HPS website also states, "As the primary consultant during any radiation emergency, a health physicist commonly has total control of the involved area."

No mention of any scientific disputes about risk of radiation. It is a given to be transferred to the uninitiated. Talk from on high. In that sense, Frazier's report fits right in. He speaks from on high. Expects deference be given to his opinions, without having to provide citations.

Now, Dr. Frazier has assessed historical accidents in his category of advisor to defendants in radiation litigation cases. However, that information is not available to judge. The litigation reports that I have seen in which he is the prime author are similar to his current review of my work and show the same rhetorical style, namely argument by assertion. In one litigation report, he is a co-author with John Auxier, and the presentation is *substantive, involving the running of a computer program. Small sample on which to base an assessment of experience.*

Note that a health physicist need only be a technician, albeit an important one with responsibilities for protecting worker health. Advanced degrees are not required, although some health physicists do have advanced degrees. Thus, a health physicist is not necessarily a scientist or researcher, although some may be both. For instance, the HPS web site states that, "The health physicist also assists engineers and scientists in designing facilities and new radiation-control programs." In this quote, the HPS makes a distinction between scientists and health physicists (although health physicists can obviously also be scientists). Undertaking a historical release and dose reconstruction requires much more knowledge and expertise than is tested in the exam that must be passed to become a certified health physicist. Thus, we must look beyond the designation of certified health physicist to assess Dr. Frazier's background for opining on historical reconstruction of releases, doses, and health effects.

Dr. Frazier has very few scientific or scholarly publications, five in total. In fact, he has but one article in a refereed scientific journal, which was on the subject of his Ph.D thesis, which deals with the spectra of molecules, not nuclei. He lists no publications in the field of risk assessment or nuclear accidents. Yet, he claims to have prepared or contributed to over 100 reports and publications in the fields of health physics and environmental science. Apparently, these reports are not listable on a resume; presumably because they have been prepared for private clients, for litigation, or otherwise do not count as scientific publications. I have located four of Dr. Frazier's litigation reports/affidavits in my files (Auxier and Frazier 1997, 1999; Frazier 1996, 1998),[4] but I have not seen the titles of the rest of his 90 or more unlisted reports and publications. As a result, no one but Dr. Frazier can judge the breadth of his litigation and other reports, their overall quality, and their relevance to the subjects at hand. Furthermore, litigation reports, unless refined and published in a citable journal or separate report, can hardly be considered scientific documents. Thus, Dr Frazier's opinions about the "scientific and technical content of the reports of Dr. Beyea and Mr. Lochbaum" must be taken with a grain of salt.

[4] *I have not been able to locate a report of Dr. Frazier's regarding litigation in McCafferty et al. that I had in my possession at one time.*

That does not mean that Dr. Frazier is unqualified to make unsupported assertions in the field of radiation measurements and radiation protection. His teaching experience at Oak Ridge makes it clear that he knows these subjects well. I would certainly pay close attention to any opinion of his in these areas. There are many parts of my report, where Frazier does have the qualifications to reach judgments based solely on his experience (not that his judgments are necessary correct). The major problem I have is with his unsupported assertions or judgments in fields for which his claimed experience cannot be assessed, particularly radiation risk assessment and reactor chemistry and physics, which involve a wide range of specialties. To be credible, when he steps outside his demonstrable expertise, he must provide citations to back up his assertions. Perhaps, his lack of experience with peer-reviewed publications explains his lack of familiarity with standard citation practice. As a peer-reviewer for many scientific journals, I would find Frazier's work unacceptable for publication as submitted, because of the lack of citation-backup and the one-sided nature of the few citations he does make, which tend to be to internal AI documents or to Boeing litigation experts. When it comes to soil measurements, which is close to his field of expertise, he does cite modern industry contractor reports and EPA memoranda.

By the way, a listing of my (Beyea's) publications can be found at http://www.cipi.com/beyea.shtml. Full citations for the references that I (Beyea) cite in this document appear at the very end.

QUALIFICATIONS (*of Frazier*)

My qualifications are detailed in the attached Curriculum Vitae (Attachment A). My area of expertise is health physics. Health physics is the scientific discipline of measuring radiation and protecting people from the harmful effects caused by high doses of radiation. My academic degrees include a B.A. in physics, M.S. in physics, and Ph.D. in physics (with emphasis in health physics and radiation protection). ->**<u>I *(Frazier)* have over twenty-nine (29) years of professional experience in health physics, primarily in the areas of environmental radiation dose assessment and exposure pathways analysis, external and internal radiation dosimetry, environmental sampling and analysis, and radiation detection and measurement. <-</u>**

<u>Beyea Response 2</u>. *Unfortunately, there is no way to judge the experience that Dr. Frazier claims other than to accept Dr. Frazier's opinion of himself. I respect him, but question his ability to opine in so many fields without citations. Dr. Frazier taught courses on health* *physics at Oak Ridge and is no doubt well qualified in basic health physics. That certainly includes the topics he lists as his professional experience in the routine operation of radiation programs, but not necessarily in the accident context. As far as I can tell, his experience in the field of accident analysis is in the preparation of expert reports for defendants in litigation, but these reports are difficult to get hold of,[5] so they cannot be judged. The experience he lists has nothing to do with releases from reactors, yet he blithely speaks with confidence on this subject in discussing the report of David Lochbaum.*

This is not an isolated example. In his report, Dr. Frazier repeatedly opines with confidence in areas where he can point neither to his own confirmable qualifications nor the necessary backup analysis. On occasion he cites to confidential litigation reports made by experts retained by Boeing, but he does not describe their analysis, only their ultimate conclusions. That is not very helpful to me in revising my report; nor do I think it is helpful to Boeing – certainly not in the long run. Boeing obviously disagrees, since they released Dr. Frazier's report.

I have received Comprehensive Certification by the American Board of Health Physics (ABHP) and am a member of the American Academy of Health Physics. The term "Certified Health Physicist" is a certification mark that may only be used by individuals who have received Comprehensive Certification by the ABHP. Certification in health physics by the ABHP is the same as professional certification by other recognized professional organizations, such as certification in diagnostic radiological physics by the American Board of Radiology. I am an elected member of the National Council on Radiation Protection and Measurements (NCRP) and a Fellow and Past-president of the Health Physics Society. -> **I (*Frazier*) have extensive experience performing environmental exposure pathway analyses and radiation dose assessments for man-made radioactive material and naturally-occurring radioactive material. I have performed numerous assessments of radiation doses and human**<-

Beyea Response 3. *If the experience in pathway analysis and dose assessments claimed by Dr. Frazier is so extensive and numerous, I can't help wondering why there are no reports or articles on the subject listed on his resume?*

health risks from real or hypothetical environmental and occupational exposures to a variety of radioactive materials in several physical and chemical forms. ***Frazier***->**Those dose and risk**

[5] *If there is a settlement between the parties, the documents may be under seal. If not, they are available at the local clerk's office, but one must know the name of the case, the circuit in which it was docketed, and the docket number—information that is generally unavailable. And, it would be a huge job to collect the material.*

assessments have included, but were not limited to, human exposures to fission products, activation products, and naturally-occurring radioactive material.<-

Beyea Response 4. *Once again, I note that the dose and risk assessments mentioned above are not listed on Dr. Frazier's resume. Were all of them confidential? Relevant? Prepared for litigation? Perhaps, Dr. Frazier has a separate resume that is shown to potential clients, but not to the public. If so, it does not seem valid to use a secret document as support for his public work.*

III. DOCUMENTS REVIEWED

Specific documents that I have reviewed pertaining to the radiological aspects of the 1959 SRE incident are listed in Attachment B. ->**I (*Frazier*) have not listed the numerous general reference documents with which I am familiar (and on which I base many of my opinions)<-**

Beyea Response 5. *Dr. Frazier's states above that he bases many of his opinions on documents that he will not bother to cite. We are asked to trust Dr. Frazier's assertion that he has read the right general documents and interpreted them correctly. I shall provide evidence below that this cannot always be the case, since he makes (unsupported) statements that are patently false. One of the reasons scientists cite to other work and one of the reasons journals require citations is that our memory of material we have read in the past is not always correct. Also, our general reading may not have covered the full range of views. It may also be out of date or not directly applicable to the current situation. This is particularly relevant to Dr. Frazier, because Dr. Frazier's last listed publication is dated, 1989. The one before that is dated, 1979.*

***Frazier*->that pertain to the following topics: identities, amounts, and physical/chemical properties of radioactive material at nuclear reactor facilities; ventilation and air filtration systems used at nuclear facilities; environmental fate and transport modeling; sampling and analysis of radioactive material in environmental media; exposure pathways analysis and radiation dose assessments; and human health risk assessment and the bases and use of risk coefficients.<-**

Beyea Response 6. *Dr. Frazier's resume indicates familiarity with some of the subjects he lists above, but not all of them. He certainly doesn't have any publications on many of these* *subjects. Instead, he apparently believes that his general reading on these subjects makes him a qualified expert. Once again, we are asked to trust Dr. Frazier's assertion concerning the breadth and relevance of his expertise, without any references that can be checked or debated. Much of his report is assertions built upon assertions, in particular, assertions about scientific practice that the reader is supposed to trust, because of his assertions about his qualifications to make such assertions. I (Beyea) find that a weak line of argument.*

IV. 1959 SRE INCIDENT AT SSFL

***Frazier*->After carefully reviewing the documents listed in Attachment B<-**, I (*Frazier*) conclude that the following statements pertaining to radiological aspects of the 1959 SRE incident are true and accurate:

<u>Beyea Response 7.</u> *I am glad to know that Dr. Frazier has **carefully** reviewed the documents listed in Attachment B. However, I am disappointed that he apparently did not review the expert reports filed by plaintiffs in the SSFL litigation, which would have provided balance to the reports from experts retained by Boeing. Plaintiffs' experts would have had access to documents available to defendants' experts, including those not available to the experts retained by the SSFL panel.*

A. –*Frazier*-><u>Potential radiological impacts of the 1959 SRE incident were assessed in a timely manner soon after the incident using actual measurement data and first-hand knowledge of those who were present at the time of the incident.<-</u>

<u>Beyea Response 8.</u> *I note that this is an area in which Dr. Frazier is well qualified to express opinions, based on his job experience. However, Dr. Frazier does not address the completeness of the available data, any possible bias in the data, or any incentives those present at the time may have had to come up with a result favorable to the company. In past statements, Dr Frazier has not been above attributing self-serving motives to people, in particular, those who maintain that there are health effects below 10-rem.*[6]

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[6] *Question to Dr. Frazier in Apollo deposition: "Do you think that those people who are in the minority have some motivation, other than a good scientific motivation? Or do you have an opinion on that?*
Answer by Dr. Frazier: I have an opinion that they have motivation other than a scientific motivation.
Q. What's their motivation, in your judgment?
A. I can't put a blanket motivation. But there are situations where, it appears to me, certain individuals in the minority that I don't believe have the data to support their position regarding the low-dose effects, are making the claims in a self-serving fashion.
Q. What are they serving? What would be the point of that?

On July 29, 1959, only three days after the Sodium Reactor Experiment (SRE) was shut down at the end of power run 14, ***Frazier->*****<u>Atomics International formed an eight-member ad hoc Committee to assist in analysis of the problem within the reactor and determine the origin of the problem. <-</u>**

<u>Beyea Response 9</u>. *Dr. Frazier is impressed that Atomics International formed an internal committee to assess Company performance, while keeping the accident secret from the public. This was a Committee appointed by management, a committee that owed its employment to management, not an outside body like the National Research Council that would have made some attempt to insure balance and independence within the Committee makeup. Given that any serious fault finding with the SRE design could destroy the commercial prospects for the company, the independence of the review is suspect. Apparently, Dr. Frazier does not think so. He has a refreshing view of human nature, at least for the staff at Atomics International. Based on his 1998 statements cited earlier in Footnote 4, he apparently does not hold such a benign view of all of those who disagree with his position on low-level radiation. They apparently make false statements to preserve their employment, according to Dr. Frazier. I don't think it is consistent to hold the view that only one group of people can be influenced by a desire to maintain employment. To be consistent, Dr. Frazier should have had some qualms about the reliability of the information provided to, or interpreted by, the committee. Ironically, it is those who Dr. Frazier criticized in 1998 as being self-serving who have been proved correct in the latest epidemiologic studies on workers (Cardis et al. 2005) and civilians (Krestinina et al. 2005).*

Dr. Frazier is impressed that AI formed a committee within three days to analyze the problem. AI should have been making offsite measurements long before the Committee started, let alone before the Committee finished its work. Is this what Dr. Frazier taught his Health Physics students at Oak Ridge? Wait until a Committee tells you what got out, before making offsite measurements? I doubt it.

The Committee was also charged with reviewing and advising on steps to remedy the

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A. The need for their advice and consultation.
Q. In other words, you are saying that --
A. Their future employment.
Q. In other words, you are saying that those people who make these claims that lower levels than what you believe are harmful, that are harmful in their minds, are saying that because they want to be employed? They want to get employment opportunities of one sort or another?
A. That's not quite what I said.
Q. Is that what you mean? I'm trying to understand it.
A. I don't want it to be on the record that I'm making a blanket statement about any group of individuals or any individuals. But there are some specific instances where, in my opinion, it appears as though these individuals support this hypothesis of measurable effects of low-level radiation exposure because of an opportunity to have and maintain <u>visibility and employment. (Frazier et al. 1998)</u>

problem and bring the reactor back into operation, including making recommendations for any necessary changes in operating procedures or reactor system to prevent occurrence of a similar problem. ***Frazier->*****Reactor operations data were re-examined and evaluated. Metallurgical and chemical analyses of reactor components were performed. A radiological characterization of the coolant and gaseous activity was performed. After approximately three months of gathering and evaluating data pertaining to the incident, the Committee issued an interim report that described the origin, the nature and consequences of the damage the SRE fuel from the incident (AI 1959).<-**

Frazier->**Throughout the following year, a more detailed evaluation of individual components of the reactor was performed. The sodium coolant was drained and a detailed analysis was performed to determine the amounts of insoluble contaminants and fission products in the coolant. <-**

Beyea Response 10. *Dr. Frazier in the above discussion does not mention some of the inconsistencies found with the data. For instance, as discussed by Lochbaum, the analysis of the fission products in the coolant revealed a puzzling reversal of the expected ratio of radioiodine to radiocesium. If the theory of Boeing's experts that radioactivity released from the fuel passed directly into the coolant is correct, then the amount of radioiodine in the coolant should have been much greater than the amount of radiocesium. The actual measurements falsify Boeing's theory and Frazier's recitation of it. No analysis I have read explains this discrepancy. It remains a puzzle that should induce caution in those who opine with confidence about what happened in 1959.*

Fuel elements were removed and inspected. The reactor was surveyed and cleaned. ***Frazier->*****Damaged moderator assemblies were removed and replaced. Piping and equipment were also cleaned. <-**

Beyea Response 11. *The cleaning of the reactor, including the duct work and piping, makes reliance on post-cleanup measurements suspect in inferring what occurred during the Sodium Reactor event. Some duct work may also have been replaced prior to the time measurements were taken, years later. Yet, in its response, Boeing argues that these post-clean up measurements indicate that the amount of radiocesium passing through the duct work must have been small. Not a valid inference.*

Additionally, detailed metallurgical, chemical, or radiological examinations were performed of the fuel, moderator cans and other components of the reactor. In 1961, the Committee completed its investigations and issued its final report that revised and supplemented its interim report (AI 1961).

B. ->The assessments of potential radiological impacts that were made following the incident showed that there were no radiological hazards to the environment.<-

->The 1959 interim report by the ad hoc Committee prepared soon after the incident concluded that "no radiological hazard was present to the reactor environs."<- (AI 1959 at p. I-2). The 1961 final report of the ad hoc Committee described their investigation and presented conclusions regarding the causes and consequences of the fuel element damage (AI 1961).

Beyea Response 12. *The ad hoc Committee never had to face vigorous peer review. The accident was kept secret at the time from the pubic and independent scientists. In effect, the Company investigated itself and gave itself a clean bill of health. The fact that Dr. Frazier takes everything done by AI at face value without worrying about conflict of interest indicates that he has a generous spirit in judging people who work in the nuclear industry, but not that he would make a good investigator in a crime lab.*

Radiological conditions in the surrounding environment were monitored during the investigation of the incident and the 1961 report noted that "[c]ontinued routine monitoring of soil, vegetation, water, and air revealed no increase in background radiation levels." (AI 1961 at p. III-21) The final report also concluded that:

> **"->In spite of the cladding failure to 13 fuel elements and the release to the primary coolant of several thousands of curies of fission product activity, no radiological hazard was presented to the reactor environs. <-**"

***Beyea Response 13*.** *How lucky for the future of the Company that its internal investigation committee concluded there was no radiological hazard from its actions. I note that there is no mention of uncertainty in the text. The air of absolute certainty here has the ring of a press release, rather than a scientific investigation.*

The Committee performed a final review of the causes and effects of the fuel damage, the modifications that were made in the reactor system, and the changes in operating procedures and management. Based on that review the Committee recommended approval for operation and the SRE was returned to operation on September 7, 1960.

C. The identities, locations, and amounts of fission products in the sodium coolant, cover gas system, and other components of the primary system were studied extensively soon after the incident.

An assessment of the locations and amounts of fission product contamination in the SRE was performed soon after the incident. Fission product release and distribution data were compiled during the time between the incident in July 1959 and the restart of the reactor in

September 1960. The report of that study was prepared by R.S. Hart of Atomics International and issued on March 1, 1962 (AI 1962). Several of the conclusions of that report are significant.

For example, the report noted that only isotopes of xenon (Xe) and krypton (Kr) were found in the cover gas (helium) (AI 1962). The assessment also found that a carbonaceous particulate material in the sodium effectively scavenged fission products from the sodium. In addition, it was found that "[t]he cold trap in the primary system was effective in removing fission product contamination" from the sodium (AI 1962). The design and operation of the reactor was such that "[a]lthough 5,000 to 10,000 curies of fission product activity was unexpectedly released to the primary sodium coolant system, no radiological emergency of any nature occurred" (AI 1962). The limited amount of fission products released into the sodium coolant during the incident and the effectiveness of the coolant cleanup components combined to allow continued use of the same sodium that was in the reactor during the incident (AI 1962).

D. Studies of the radioactivity released from the SRE reactor during the 1959 incident, including measurements of radioactive material in and around the reactor, show that only inert (noble) gases (isotopes of krypton and xenon) were released during the incident and that no radioiodine or radiocesium was released to the environment.

***Frazier*->**<u>**The only transport pathway for radioactive material (fission products) from the SRE reactor fuel to the offsite environment initiated with release from the fuel into the sodium coolant, <-**</u>

<u>Beyea response 14.</u> *The statement that the only transport pathway involved release of radioactivity into the sodium coolant is too simplistic. As David Lochbaum quoted in his report, formation of sodium gaseous bubbles were likely.*[7] *Once sodium vaporizes it loses most of the ability it has as a liquid to remove heat. Still, for a while cooling can continue, because a bubble initially has a liquid layer between it and the surface that can evaporate into the bubble, keeping the surface relatively cool (Bang et al. 2005). Once the surface layer of liquid sodium under a bubble evaporates, however, overheating of the fuel can occur. If that overheating is great enough, the reforming of a new liquid layer can be prevented, causing melting or near melting of the fuel (Bang et al. 2005).*

Bang et al. give a nice discussion of the physics of bubbling, including the merging of bubbles into a large vapor region that can lead to melting or burnout (Bang et al. 2005).) Although Bang et al. discuss boiling in water, the general principles should apply to any liquid.

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[7] *"It is clear that still higher temperatures were reached in the region of failure, and it is likely that sodium boiling occurred in that region." Lochbaum citation to, AI – 1961a.*

Another difference is that Bang et al. consider heating from a horizontal surface, whereas the fuel in the SRE was vertical.

What may be particularly different in the sodium case is the possibility of a positive sodium void coefficient that could have led to increased power generation and more rapid overheating, once bubbling started (http://en.wikipedia.org/wiki/Void_coefficient). In large reactors, sodium is known to have a high positive void coefficient of approximately 10 (Kessler 1993), (Fontana 1978). A positive sodium void effect with metal fuel is due to neutron "spectral hardening" that dominates capture and leakage changes during sodium voiding (Dobbin et al. 1991). I have seen no discussion of the magnitude of the sodium void coefficient at the SRE. That the void coefficient was significantly positive is suggested by the investigating committee's attribution of the SRE power excursion on July 13th to sodium vapor in fuel channels (Personal communication, Gordon Thompson, 2006). Consequently, bubble formation would have been expected to increase power, thereby increasing the amount of overheating. Neutron activity in the vicinity of the sodium voids might not have been fully detected at the neutron monitor in the core, giving the operators a misleading impression of what was happening.

In contrast to the picture painted by Frazier and Boeing, there are scenarios that could lead to fission products bypassing the liquid coolant. First the fuel must overheat. The most likely scenario I have come across for bringing the fuel to the melting point or just below it, where fission product migration within the fuel speeds up, involves a vapor layer over sections of the fuel. How long bubbles will hover on the surface can be addressed quantitatively (Haramura and Katto 1983). Such an analysis has not been considered in the SRE context, which is unfortunate, because it might give insight as to whether or not there was sufficient time for the liquid layer underlying bubbles to evaporate. In any case, radioactivity from the fuel could easily bypass the liquid coolant, traveling within bubbles of gaseous sodium to the cover gas. I realize that John Krsul has an implicit counter argument for radioactivity-filled bubbles, claiming that little radioactivity escaped the fuel in the first place, because the fuel never actually melted, forming instead a eutectic mixture of fuel components. However, Dr. Frazier does not make such an argument, which makes his contribution to the debate extremely simplistic. As for Krsul's argument, I note that he does not present evidence that all the fuel stopped changing at the eutectic state. In fact, there is evidence that sufficient disruption occurred to make large releases possible:

> *"Severe overheating of some of the fuel elements is known to have existed. Some melting of uranium involved in the Fe-U eutectic formation also occurred. Many of the fuel slugs were badly swollen, cracked and spongy. It may well be that*

> *these effects were sufficient to allow the release of the majority of the measured amounts of fission product contamination by diffusive processes. Moreover, the conditions conducive to diffusion may have persisted over a fairly long time during the last reactor run, perhaps up to two weeks. It is also possible that the fission product release from a few of the badly damaged elements raised the overall average release appreciably." (Hart 1962)*

Krusl's conclusions about the eutectic are taken from unavailable reports of Boeing's litigation experts.[8] Krsul does not describe the analysis. At this point the eutectic argument is only an assertion to those of us working in the public domain. I would welcome further information on the strengths and weaknesses of this theory, which is certainly plausible, albeit not definitive. The fact that the eutectic formation (as well as shifting between phases of uranium) could explain the fuel damage without melting makes it possible that melting did not occur, but it does not prove that melting did not occur. The fact that many of the fuel slugs were badly swollen and spongy suggests something unusual was going on beyond eutectic formation at the cladding/uranium interface. Even without melting, spongy and swollen fuel would allow significant release of fission products. I discuss these issues in more detail in Appendix 5 of my revised report.

Another major question remains in my mind about the bubble pathway. Are the conditions for melting of the fuel under a vapor layer consistent with bubble transport to the surface being fast enough to prevent radioactive cesium and iodine inside the bubbles, either as gases or suspended particles, being absorbed into the liquid sodium boundary of the bubble? Perhaps, cesium was more likely to condense on the inside bubble surface than was iodine during the time it took for bubbles to reach the cover gas, thereby explaining the higher than expected ratio of cesium to iodine measured in the sodium after the accident. The more I think about the complex physics and chemistry going on with the SRE at the time of the accident, the more I am drawn to a dynamic, changing, and oscillating picture of fission product release and transport.

followed by release from the coolant into the helium gas that covered the coolant. Measurements of the identities and amounts of fission products in the sodium coolant and in the helium gas following the 1959 incident showed that although numerous fission products were within the sodium coolant only isotopes of the inert (noble) gases krypton and xenon were present in the helium cover gas (AI 1961; AI 1962). The measurements made in 1959 and thereafter clearly show that the sodium coolant retained the fission products except for the noble gases krypton and xenon that were released into the helium cover gas. Most of the krypton and xenon was contained in storage tanks and held until it underwent radioactive decay, but some of these

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[8] After this text was written, DOE put the sealed reports on one of its website. Until released from possible legal action by Boeing, I will not quote anything from this report that has not already appeared in Boeing-funded documents.

inert gases escaped into the offsite environment. The potential radiation dose from the released krypton and xenon was a small fraction of the radiation dose that is received from natural background radiation in any day.

E. There were multiple components of the SRE that contributed to containment of radioactive material within the SRE facility. At commercial power reactors (light-water reactors), the reactor building is a containment pressure vessel that encloses the reactor and many related components and is necessary for containing high pressure gas releases that might occur during an accident in those reactors. Because the fundamental design of the SRE prevents generation of high volumes of gas (i.e., steam) during routine operations or during incidents (such as the 1959 incident), it was unnecessary for the reactor building at the SRE to be a containment pressure vessel.

Frazier*->*The SRE reactor building was not designed as a containment pressure vessel, because the maximum credible accident that could have occurred at the SRE would not cause a release a high volume of gas (such as steam) that would require pressure containment. <-**

***Beyea Response 15*.** *Who decided the limits of a "maximum credible accident" – the worst accident that the SRE was designed to counter? Dr. Frazier does not say. Presumably, based on the qualifier he uses here, there exist accident scenarios that require pressure containment to limit their release, presumably accidents involving a sodium fire or explosion. This is not a major point in my response to Dr. Frazier, but I do not want to let this statement to go unchallenged.*

"A safety disadvantage of using liquid sodium as coolant arises due to sodium's chemical reactivity. Liquid sodium is extremely flammable and ignites spontaneously on contact with air or water. Thus leaking sodium pipes could give rise to sodium fires, or explosions if the leaked sodium comes into contact with water." http://en.wikipedia.org/wiki/Integral_Fast_Reactor

To reduce the risk of explosions, the SRE had an intermediate coolant loop between the reactor and the turbines. But the risk was not zero, so a strong containment would have had a purpose. It also could have served to keep air coming into the structure after a sodium fire. What Dr. Frazier is doing in the above passage is simply repeating the major marketing claim that was made for the Sodium Reactor at the time, namely that no release could occur, so a containment was unnecessary.

"The absence of releasable potential energy from pressurization or chemical reactions increases the inherent safety of this reactor system and permits the plant configuration to be easily arranged to contain radioactivity under all circumstances." (Starr 1955)

To state that it is easy to "contain radioactivity under all circumstances" is to make a very strong claim. Had the ad hoc Committee that Dr. Frazier praises found otherwise, the major marketing claim would have been lost.

However, even Dr. Starr recognized the disadvantage of sodium:

> *"Sodium, however, is not without certain disadvantages. It reacts chemically with oxygen and water and must therefore be isolated from these. Further, when exposed to neutrons as in a reactor, it becomes a strong radioactive gamma ray emitter with a halflife of about 15 hours, and this creates a serious problem in nuclear power plant applications." (Starr 1955)*

This raises an interesting point, if there were bubbles of sodium and there was a release of radioactive cesium, there should have been radioactive sodium escaping as well. The equilibrium inventory of 15-hour Na^{24} produced by neutron absorption in the Na^{23} coolant would be roughly 5,000 Ci per megawatt of reactor power for an "intermediate" reactor, according to historical estimates (Weber and Epstein 1949). At maximum power of 20 Mw, this would amount to 100,000 Ci. This number is consistent with another estimate of Ci/kg of Na^{24},[9] but 20-times higher per kg than levels found in a very small reactor after an accident.[10] To look at the possible importance of Na^{24}, I use the higher number of 100,000. How much of this 100,000 Ci would have boiled off? If only 1% boiled, 1000 curies would have entered the cover gas. If the holding tanks were on bypass, as some evidence indicates for at least part of the time (personal communication, Gordon Thompson), there would have been an immediate direct pathway to the atmosphere for Na^{24} vapor, albeit through filters, were they in place and fresh enough to be operating efficiently. In addition to lack of knowledge of the filter efficiency, there is lack of knowledge about the loss in the piping due to plateout, or in the holdup tanks, were they not bypassed. In any case, based on relative lifetimes, even if 10,000 Ci of Na^{24} escaped, the release would not compare in health effects to even a small number of curies of radiocesium in terms of external dose. As for internal dose, based on inhalation coefficients given in (USEPA 1988), 10,000 Ci of Na24 would give about the same whole-

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[9] *The figure of 10 Ci per kg of coolant is given for Na^{24} in an operation fast breeder by IAEA (IAEA 2002). Assuming 10,000 kg of sodium in the SRE reactor, this means 100,000 Ci in the coolant during full operation. I estimated 10,000 kg of coolant by taking ¼ of the volume of the reactor, which I computed using the dimensions given in (Parkins 1955), 19 ft deep, 11 ft in diameter.*

[10] *At full-power level in a 1 Mw reactor, EBR-I, the activity of Na^{24} was reported to be 24 mCi/kg. (ORAU) Using this concentration multiplied by 20 to match the SRE power, along with the 10,000 kg of sodium assumed to be in the SRE, would imply a Na^{24} activity of only 5000 Ci in the coolant.*

body dose as I computed for the thyroid dose received from 5 to 10 Ci of released radioiodine. Only if one were convinced that essentially no radiocesium or radioiodine got out, would one likely bother to try to quantify the Na^{24} release and its corresponding health effects.

Also of interest in the coolant, because of its long 2.6-year half-life, would be Na^{22}, whose decay leads to a 1.3-MeV gamma ray from its daughter product, Ne^{22}. Na^{22} is produced from coolant sodium, when a neutron knocks out a neutron in the reaction, $Na^{23}(n,2n)Na^{22}$. With a cross section that was thought in early documents to be only 1/50th of that of the cross section for making Na^{24} (Weber and Epstein 1949), but a 1500 times longer half-life, Na^{22} could conceivably build up over time to become the principal radiosodium hazard in a release to the environment. However, more recent measurements imply a much lower cross section for production of Na^{22}, based on measurements in coolant. As a result, there might only be 1-Ci of Na^{22} in the coolant.[11]

My (Beyea's) report has not considered whether or not levels of gamma radioactivity measured during the SRE would have picked up sodium as vapor, were it present, thereby setting some limits on the radioiodine release via the bubbling scenario. This limit might not turn out to be very useful, however, because gamm- measuring equipment was likely set to exclude the (ever-present) sodium gamma peak.

*Frazier->***The SRE was designed to retain gases at about atmospheric pressure and reduce leakage of potentially contaminated gases from the facility**<- to the environment by maintaining the reactor building at a pressure slightly lower than the outdoor air (i.e., it was maintained at slight negative pressure).

Beyea Response 16*. *Systems do not always behave as designed.

*Frazier->***Containment of radioactive material at the SRE was assured through multiple components that were integral part of the reactor design.** <-

Beyea Response 17. *To say that containment was assured is circular reasoning. I don't understand why Dr. Frazier ignores the fact that there is a debate going on about whether or not radioactive material other than noble gases escaped. You can't just assert in response that containment was achieved. Furthermore, noble gases did escape during the*

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[11] *A factor of 100,000 below Na^{24} (IAEA 2002), consistent with (Stamm and Stade 1987).*

SRE event, so the statement of Dr. Frazier is false on its face. Moreover, radioactivity was designed to escape from the hold-up tanks, when deemed safe. With all due respect, this statement by Dr. Frazier sounds like it came from a Company press release.

The components that contain radioactive material within the SRE reactor and its associated structures include the fuel, fuel elements, cavity liner and cover shields, sodium coolant, helium gas cover of the coolant, nitrogen gas cover of other components, reactor building, shielded gas storage vaults, gas storage tanks, and exhaust air filtration systems. *The absence of any releases of radioactivity other than the inert gases krypton and xenon from the SRE reactor during the 1959 incident confirmed the SRE contained radioactive material as it was designed.*

Beyea Response 18. *This is more circular reasoning, as well as more argument by assertion. At issue in the debate between Boeing and the consultants to the SSFL panel is the likelihood that any significant non-noble material did get out.*

F. Over the 47 years since the SRE incident additional assessments of offsite releases and/or radiation doses to offsite personnel have been made and none of the assessments based on actual SSFL-specific measurement data have changed or modified the conclusion of the 1961 final report that "no radiological hazard to offsite persons was present."

Although assessments of offsite releases and radiation doses to offsite personnel from the 1959 SRE incident have been performed in recent years in association with litigation related to the SSFL, ***Frazier->*****all of the assessments that used site-specific measurement data<-**

Beyea Response 19. *Using what Dr. Frazier calls, "site-specific measurement data," is the method preferred by defendants in litigation to assess offsite releases. Defendants used experts who accepted all of AI's statements at face value. Plaintiffs in the litigation, apparently, used experts who were skeptical of AI statements and used other methods to assess offsite releases. I point out that defendants were selective in their use of SRE measurements. For instance, the discordant ratio of cesium to iodine has been ignored, even though it strongly suggests a release of iodine. The extremely low burn-up reported for the accident period has not been questioned. David Lochbaum in his analysis relies heavily on statements and measurements made by Atomics International staff. He has a different interpretation, no doubt, of these data than that of Boeing's consultants.*

No doubt all experts, ranging from those retained by the SSFL panel to the opposing sides in the litigation, were selective in their use of local data. Given the inconsistencies in the picture of what happened, professional judgment must be used to reach a conclusion about *releases. Professional judgment differs among experts; hence the existence of different views on releases and doses, and the need to combine expert opinions in some kind of a range, such as a likelihood distribution.*

reached the same conclusion as the 1961 final report (AI 1961) that the 1959 SRE incident did not produce any radiological hazard to offsite persons. ***Frazier->*****Other assessments I (*Frazier)* have reviewed of releases of radioactive material from the 1959 SRE incident have in my opinion ignored the science and experimental facts pertaining to the design and operation of the SRE and the measurements following the 1959 SRE incident, and base their assessments on speculative theories that are unsupported by science or measurement data.<-**

Beyea Response 20. *Frazier's opinion on this matter, that only Boeing's experts have taken into account science and experimental facts, is expressed without supporting arguments. He has no demonstrable expertise in the science, design, or operation of nuclear reactors. He is willing to step outside his area of expertise without providing backup analysis. Had he restricted his opinions to those areas where his expertise is demonstrable, e.g., in the area of soil measurements of radiocesium, or had he cited to the literature in the other areas on which he opines, his remarks would have been more credible.*

G. Concentrations of radioactive cesium in surface soil have been measured at numerous locations surrounding the SRE site since the incident in 1959. The results of those soil measurements show that ***Frazier->*****there are no areas in the vicinity of the SRE site having radioactive cesium greater than amounts due to fallout from atmospheric weapons testing<-**. Measurements of radioactive cesium in soil in the environs of the SRE do not indicate or suggest in any way that radioactive cesium was released to the environment during the 1959 incident.

Beyea Response 21. *In discussing radioactive cesium in surface soil, Dr. Frazier finally enters an area where he has expertise. However, he completely ignores the possibility of elevated releases, a point I stressed in my report. In addition, he ignores uncertainty in soil measurements. He provides no evidence of what the levels of fallout from atmospheric testing might be in this are, and, most importantly, he provides no assessment of their standard deviation. He fails to say what level of SSFL radiocesium could have been detected within the uncertainty of fallout measurements. He is making a statistical statement without backup. Furthermore, he does not discuss the completeness of the angular coverage. Are there no gaps in wind direction through which a large round release could have passed unnoticed? Without actually looking at the soil data, it is not possible to be definitive. I have assumed that the coverage is complete in my report, but I am forthright in stating it as an assumption.*

Frazier->**<u>If there had been a release of significant quantities of radioactive cesium to the air as a consequence of the 1959 SRE incident there would have been (and would continue to be) elevated concentrations of radioactive cesium in surface soil in the vicinity of the SRE site, with the highest concentrations near the site boundary and concentrations decreasing with increasing distance from the site.<-</u>**

<u>Beyea Response 22</u>*. This is a fundamental error. As is typical with Dr. Frazier's statements in this report, there is no citation for his claim that cesium concentrations would decrease with increasing distance from the site. Had he tried to find a citation, he might have discovered that the statement is only true for ground level releases. It is not necessarily a true statement for elevated releases, as I indicated in my report. The hottest spots subsequent to an elevated release can be far away from the reactor, depending on meteorological conditions, with concentrations increasing with distance until a maximum is reached, decreasing with further distance. The best analogy is a waterfall, where the water reaches a maximum away from the starting point. I have a hard time understanding how this statement of Dr. Frazier's could be in his review, especially given the fact that I specifically discuss elevated releases in my report in quantitative fashion. The presence of this statement in Dr. Frazier's report seems to imply that he is unaware of the most basic facts about dispersion from releases at elevated temperatures. This is the kind of error that can be made when one steps outside one's area of expertise and doesn't check the literature. It is an example of why journals require citations. Courts, the venues in which Dr. Frazier apparently presents most of his science, would do well to require the same.*

As a result of this fundamental error about elevated releases, Dr. Frazier reaches a false conclusion in stating that that the signal from a significant release of radiocesium would be evident in the vicinity of the SRE site. It might, but it might not, depending on the detailed history of the release. I do agree, as I say in my report, that the signal would be detectable somewhere, which leaves distances beyond 5 miles as the place to look.

Frazier->**Soil sampling at the SRE site and in the surrounding areas was performed after the 1959 incident, with no elevated concentrations of radioactive cesium being present in those samples (AI 1961).<-**

<u>Beyea Response 23</u>*. In stating that no elevated concentrations were found in the vicinity of SRE, Dr. Frazier is making a statistical statement, with no statistical backup. It is a non-trivial matter to determine the background levels of radioactive cesium from weapons tests in complex terrain (Harold Beck, personal communication, 2006), such as exists around the*

SSFL site. In addition, before being able to say there was no elevated concentrations, one has to know the standard deviation of the background soil measurements. Because of fluctuations, there will be a minimum detectable level of radiocesium above background, which Frazier does not give. I do agree, however, as I say in my report, that the minimum detectable level should be enough to rule out a large, ground-level release.

Additional soil sampling and radiation measurement studies have been performed in the environs of the SRE throughout the intervening years since the incident and ***Frazier***->**none of the data from those studies indicate any radioactive cesium greater than amounts due to fallout from atmospheric weapons testing<-** (EGG 1979; McLaren 1993; McLaren 1995; Hamilton 1997; Ogden 1998; USEPA 1998a; USEPA 1998b; and QST 1999).

Beyea Response 24. *Once again, Dr. Frazier does not state the minimum level of detectability of cesium released from the SRE. This is a small point, given the fact that it is large releases that are of most concern, but it is another example of imprecision within his report that weakens his credibility on more important subjects.*

H. Because there were no releases of radioactive cesium or radioactive iodine from the SRE to the offsite environment during the 1959 incident there were no radiation doses or adverse health risks from those materials to anyone offsite.

Assessments of exposure pathways, radiation doses, and cancer risks to offsite individuals from radioactive cesium and radioactive iodine released offsite as a consequence of the 1959 SRE incident should conclude necessarily that there were no offsite radiation doses and no increased health risks from those materials from the incident. ***Frazier***->**Simply stated, with no releases of those radioactive materials offsite there was no offsite exposure, dose, or cancer risk from them.<-**

Beyea Response 25. *It does follow that, if there were no releases, there would be no adverse health effects from SSFL radioactivity. This is a truism. No disagreement there. The disagreement revolves around Dr. Frazier's statements that no releases occurred.*

V. OCTOBER 5, 2006 REPORT OF JAN BEYEA, Ph.D.

Frazier->**The report of Jan Beyea, Ph.D., to the SSFL Advisory Panel, dated October 5, 2006, includes numerous statements unrelated to science, engineering, or the technical aspects of assessing offsite exposures, doses, and human health risks from the 1959 SRE incident (Beyea 2006). <-**

Beyea Response 26. *Dr. Frazier is correct in stating that my report goes beyond physics, chemistry, and earth science, but these sections of my report that do are necessary to assess offsite exposures, particularly because they address the issue of data quality. I also provide considerable historical context, which goes beyond technical calculations. Some of my analysis relates to the confidence that readers should put into statements of Atomics International. I take specific notice of the fraudulent statements made in the AI press release, which states there was no release of radioactivity from the reactor.*

In contrast, Dr. Frazier's accepts as valid all AI statements and data. There are never any cover-ups in Dr. Frazier's world, apparently. However, he is not beyond attributing financial motivation to some of those who maintain different positions than he does on low-level radiation, as revealed in the excerpt from a 1998 deposition quoted in Footnote 4. In contrast, when it comes to 1959 and Atomics International, when the future of the company depended on there not being any releases from their new reactor design, he does not see any possibility that their satisfaction with the picture of the accident they drew might have been increased by a desire to retain employment. To me, this is an inconsistent position on Dr. Frazier's part.

As for "statements unrelated to science, engineering, or the technical aspects of assessing offsite exposures, doses, and human health risks," I quote one of Dr. Frazier's statements about my report,

> *"This attempt to lend technical credibility to his release estimate is disingenuous, without scientific basis, and blatantly misleading."*

What kind of statement is this, I (Beyea) ask?

I have prepared the following comments and conclusions regarding the specific sections of Dr. Beyea's report pertaining to radioactive materials releases, transport and offsite dose calculations, and human health risk assessment. It is important to note at the beginning of this section that ***Frazier->*****Dr. Beyea states in his report "... the estimates in this report are limited to scoping calculations that carry a wide range of uncertainty<-**, complicating their use for estimation of statistical power." (Beyea 2006 at p. 4).

Beyea Response 27. *I do indeed discuss in my report the wide uncertainty in my calculations. In contrast, Dr. Frazier admits no uncertainty in any part of his report.*

A. Release Estimates

Frazier->**Dr. Beyea bases his determination of the amount of radioactive material**

<u>released during the 1959 SRE incident on his own method of comparing release estimates prepared previously by five (5) individuals or organizations, giving a weight to each of those estimates that he somehow determines.</u>

<u>Beyea Response 28</u>*. I would be proud to have invented the method I used in my report to combine release estimates made by different experts. Unfortunately, I cannot take credit for it, because 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 the method 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.[12, 13] In the revision to my report, I have also made estimates of bounds of the distribution using "possibility" theory (Sandri et al. 1995).*

<u>*Frazier*->This method of determining the most accurate value of any parameter is without scientific basis or merit.<-</u>

<u>Beyea Response 29</u>*. Wow. Apparently, Dr. Frazier is unaware of any of the literature I cite above, or the use of it by the Nuclear Regulatory Commission (USNRC 1995), yet he does not hesitate to assume I have presented a novel and non-standard method of analysis. Once again, he relies on his (obviously limited) general reading to condemn the report of another*

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[12] 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 and Zidek 1986a). 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.

[13] 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" (Genest and Zidek 1986a). 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.

expert.

Frazier->**<u>He selects release estimates from individuals or organizations without examining the basis, assumptions, and limitations of each of those estimates<-</u>**

<u>Beyea Response 30.</u> *Dr. Frazier is wrong in saying that I have not examined the basis, assumptions, and limitations of estimates made by experts I include in my likelihood distribution. However, it is true that a reader of the original report might not know how much effort I spent in looking into the basis of each expert assessment, beyond simply reading the reports available to the public and finding them reasonable. In fact, I did consider the basis of all estimates for which I could obtain the analysis, particularly the highest release estimate of the group, namely that by David Lochbaum, which I found to be reasonable, although not definitive. I did discuss the main limitation of his report, namely that his best estimate did not account for the close-in measurements of soil radiocesium, which rule out a large ground-level release of radiocesium. I corrected, in part, for that limitation in my weighting of his release estimate. In the latest revision, a much better adjustment has been made, using, in part, newly identified soil measurements made in LA parks.*

I did not have access to litigation reports for defendants, but presumed Boeing hired competent people who would have filed reasonable reports. I could not discuss either their strengths or weaknesses, because I have no approved access to these documents,[14] *which are sealed with the Court as a result of a settlement agreement. If those reports were definitive, than plaintiffs' experts would have folded their case, which I know did not happen, based on a transcript of an interview with one of them that I located on the Internet (LOE 2006). Furthermore, I know the past work of these source-term experts, who are very competent. I have no doubt that their arguments are reasonable, but are also unlikely to be definitive, because there is so much uncertainty associated with what happened in 1959 at the SRE. I certainly wish that the expert reports in the SSFL litigation report had been available to me. Boeing can make them public at any time. Until Boeing does so, I must proceed with information in the public domain, which now includes a discussion of arguments made by*

[14] DOE has recently placed some of defendants, but not plaintiffs, expert reports on its SSFL web site. Until Boeing promises no legal action for use of these sealed reports, and until plaintiffs' expert reports and rebuttals are released to provide balance, I cannot discuss the merits of these reports. Fortunately, the bulk of the arguments have been made publc by Boeing in its responses to the SSFL Panel reports.

Boeing consultant, John Krsul, who apparently has listed the main arguments of Boeing's litigation consultants, which allows me to discuss them.

I should say that, given the approach I have taken, I doubt having access to the full set of reports would change much the weights I assign. Furthermore, the linear pooling technique I have used to develop a likelihood distribution is relatively insensitive to assigned weights, unless they are very extreme (Genest and Zidek 1986a). Although I have a "due diligence" responsibility to be sure that each expert has a reasonable basis for reaching a conclusion and is not totally biased, I must not second-guess experts when there is uncertainty. I would be understating the uncertainty in our state of knowledge of the SRE accident, if I left out either the high or low (reasonable) estimates. In any case, in my revised report, I have included sufficient material to show that I have looked into the physics and chemistry of the SRE event and have identified weaknesses in every expert's line of reasoning, while finding them all plausible. Perhaps, that was not evident in the original.

. He admits that his selection of the release estimates is non-random (Beyea 2006). ***Frazier-***>**<u>However, a review of the origin of the release estimates shows that his selection is in fact biased toward the estimates of the greatest amount of radioactive material released from the site. <-</u>**

<u>Beyea Response 31</u>. *Dr. Frazier refers above to a "review" he made that supposedly shows that my selection is biased toward the greatest release estimate. Where is that review? It does not appear in the report. Apparently, Dr Frazier is again arguing by assertion. It is very difficult to defend against such charges, when their basis is not provided. All I can do, each time such statements appear, is point out to the reader the absence of any basis for them.*

Frazier->**<u>Those estimates of large releases are based on flawed and unsupportable assumptions regarding the fundamental science of the SRE design and operation.<-</u>**

<u>Beyea Response 32</u>. *As I have stated earlier, Dr. Frazier has no credentials in the field of reactor design and/or operation, so cannot speak, "Ex Cathedra." Dr. Frazier does not list or discuss the "the flawed and unsupportable assumptions regarding the fundamental science of the SRE design and operation" on which the largest estimates were made. Therefore, it is difficult to respond other than to point out, once again, that this is another example of his use of the rhetorical technique, argument by unsupported assertion. Presumably, Dr. Frazier does not believe there are any flaws or unsupportable assumptions in the lowest* *estimates, e.g., those made by Atomics International or consultants to Boeing in litigation. In fact, all of the expert opinions have gaps in their analysis, because the necessary measurements to understand the release were not made at the time. The blame for this lies at the feet of the staff of Atomics International's and the company's management. As a result, considerable expert judgment must be used in all estimates of releases from SRE.*

Frazier->**Moreover, the estimates of large releases ignore the detailed release estimates made soon after the incident using actual site-specific measurement data and first-hand knowledge of those who were present at the time of the incident.<-**

Beyea Response 33*. In saying that site-specific measurements suggest small releases, Dr. Frazier ignores the issues of data quality, completeness, and consistency. A lot of data were missing, because measurements were sporadic; there are many inconsistencies. As I indicate in my report, measurements that could have resolved the issue were never made, or at least never made available to the public. Dr. Frazier ignores the possibility of sodium bubbling, holding-tank bypass, and the inconsistent ratio of cesium to iodine found in the coolant that suggests a significant release of iodine.*[15] *He ignores the suspiciously low value reported for burn-up during the 14-day event. True, by focusing on some data and downplaying others, particularly the cesium/iodine ratio, it is possible to come up with a reasonable picture that suggests a low release. Quite possibly, that picture is correct. However, there are other ways to weight the data, leading to different conclusions, which also may well reflect what actually happened. There were many measurements that could have been made contemporaneously, even after the event, that could have put the release issue out of dispute. Those measurements were not made, suggesting to me that no one dared double-check the Committee's major conclusion. Of, if such measurements were made, they have never seen the light of day. Note that Dr. Frazier only cites reports of defendants' experts, suggesting that he has not seen plaintiffs' expert reports. That means he has not seen all sides of the story.*

From the results of the release estimates that he selects, Dr. Beyea presents his mathematical "analysis" in which he calculates the "statistical parameters" associated with the

[15] *I note that the discrepancy cannot be explained by assuming that more iodine than cesium would have been removed by the cold traps. Cold traps work in order of decreasing solubility (IAEA 2002). Their main purpose to remove sodium oxide (Parkins 1955). Cesium is less soluble than iodine in sodium (Koch et al. 1991).*

combined estimate. ***Frazier***->**<u>This attempt to lend technical credibility to his release estimate is disingenuous, without scientific basis, and blatantly misleading. <-</u>**

<u>Beyea Response 34.</u> *This sentence characterizing my combined likelihood distribution is my favorite one in Dr. Frazier's entire report. "Disingenous." "Without scientific basis." "Blatantly misleading." Oh, my. Once again, no basis is given in his report for his assertions. What is it about what I have done that makes it disingenuous? Surely, I should be told? What is it about my analysis that makes it blatantly misleading? Why keep it a secret? As for saying that my analysis here is without scientific basis, I have already discussed Dr. Frazier's apparent lack of knowledge of this part of the field of risk assessment. My approach, "linear pooling of probability densities" is quite standard (Genest and Zidek 1986a). It is even used by the Nuclear Regulatory Commission in its reports (USNRC 1995).*

Frazier->**<u>Through application of his own method for estimating the amount of radioiodine released to the offsite environment<-,</u>** he arrives at an estimate of from 0 to several thousand curies released to the offsite environment due to the 1959 SRE event.

<u>Beyea Response 35.</u> *As I said earlier, I would be proud to have invented the method I used in my report to combine release estimates made by different experts. It is hardly my own method. In this passage, Dr. Frazier continues to demonstrate an apparent lack of familiarity with modern risk assessment.*

Frazier->**<u>The scientific approach that Dr. Beyea should have used to determine releases from the SRE site is the accepted method….<-</u>**

<u>Beyea Response 36</u>. *Dr. Frazier has introduced a new term, "accepted method." He says I should have used the "accepted" method, without saying accepted by whom, without presenting a single citation as backup, and without indicating the range of opinion on the subject. Presumably, he means a method accepted by Boeing and other defendants in litigation with whom he has worked. These are, I suppose, the methods that produce the lowest release estimates in dose reconstruction. Any approach Dr. Frazier favors certainly deserves consideration, but not to the exclusion of all others.*

Frazier->**<u>…whereby the "source term" for releases to offsite areas is determined from site-specific measurement data (if available)….<-</u>**

<u>Beyea Response 37</u>*. Dr. Frazier says the source term should be determined from site-specific data (if available), without discussing availability. He does not say what to do when the company (Boeing) withholds data. He does not say what to do when the data is inconsistent. He does not say what to do when the data is missing. He does not say what to do when experts disagree on how to interpret the data. In fact, every one of the experts whose release estimate I combined used some site-specific data. None of them used all of the site-specific data, because of the inconsistencies between some of the measurements. In fact, because I combine the analysis of all of the experts, I have implicitly used more site-specific data than anyone else.*

Frazier->**<u>…..or through calculations based on relevant, site-specific parameters.<-</u>**

<u>Beyea Response 38.</u> *Dr. Frazier says the source term should be determined through calculations based on site-specific data. Again, he does not say what should be done if data is withheld. He does not define, "relevant." He does not discuss what to do when experts disagee about what data is relevant. No doubt, Dr. Frazier would argue that the discordant ratio of cesium to iodine measured in the core is not relevant. Other experts, including me, would disagree with him on that point. The method I picked from the scientific literature, one that has been used many times before, handles cases when experts disagree. In Dr. Frazier's methodology, one is supposed, essentially, to look at all the experts and decide like a judge or jury which expert is most likely to be right. Then, accept that one expert or team of experts as offering the truth. That methodology fails to present the full range of uncertainty in our state of knowledge.*

Frazier->**<u>This accepted method…<-</u>**

<u>Beyea Response 39</u>*. This is Dr Frazier's second use of "accepted method," still without saying who accepts it.*

provides deterministic values of the identities, amounts, and concentrations of radioactive material released during an event.

*Frazier->***<u>This accepted method is that which was used in the release calculations soon after the 1959 SRE incident (AI 1961).<-</u>**

<u>Beyea Response 39</u>*. Third repetition of "accepted method," still without saying who accepts it. We now learn that this is the method used by Atomics International in 1961. Below we learn it was used by defense experts in recent litigation. No mention of how uncertainties, missing data, and inconsistent data were handled. No mention of the professional judgment needed to fill in the gaps in data. No mention of alternate interpretations of the data. No mention of the financial pressures on the company and its employees. No mention of the failure to take measurements that would have been helpful in resolving uncertainties, such as offsite ground measurements and post-accident measurement of radioactivity left on the exhaust filter.*

*Frazier->***<u>This scientific method …<-</u>**

<u>Beyea Response 40</u>*. Fourth mention of the "accepted method," now called a "scientific method," still without a single citation. How is a third-party reader to judge such claims?*

*Frazier->***<u>…for determining the source term for the 1959 SRE incident was also used by defense experts (Christian 2005; Daniel 2005) in the recent litigation pertaining to the SSFL [O'Connor v. Boeing]. <-</u>**

<u>Beyea Response 41</u>*. We are told that defendants' experts used the "accepted" and "scientific" method, but we are not told what method plaintiffs' experts used in the litigation. Did they not also use a scientific method? Apparently, Frazier does not know, because he does not list their analysis in the list of reviewed documents at the end of his report. Did he request such reports? Was he, like the rest of the public, also denied them?*

The approach used by Christian and Daniel for determining the "source term" for the 1959 event incorporated site-specific facility design parameters, operations history, and measurement data obtained during and following the event. *Frazier->***<u>The results of Christian's detailed analyses of the event based on extensive theoretical and measurement data show</u>**

<u>that radioiodine was retained in the reactor and was not released to the offsite environment (Christian 2005).<-</u>

<u>Beyea Response 42</u>. *Although not necessary for the method I used to develop source terms, it sure would be nice to be able to discuss this sealed report by Christian,*[16] *along with plaintiffs' expert's rebuttal. Instead, I must read between the lines of Frazier's and Krsul's discussion of Christian's work. In Frazier's view, Christian's analysis shows that radioiodine was not released to the offsite environment. Case closed. No doubt here on Frazier's part. No uncertainty. No room for other views or other interpretation of data. I have discussed in many places earlier in this response the problems with this approach. In contrast, pooling of expert likelihood distributions gives you an idea of the range of expert views on an issue, which reflects the uncertainty in our knowledge of a situation. And, the results are not very sensitive to weighting of experts, unless the weights are extreme (Genest and Zidek 1986a). Third parties will have to decide which approach is most useful to them.*

I note that Frazier only refers to a conclusion by Christian concerning radioiodine releases. That suggests that Christian did not consider releases of radiocesium.

Beyea also applies his method for estimating the amount of radioactive cesium (specifically cesium-137) released to the offsite environment from the 1959 SRE event. He arrives at an estimate of from 24 curies to 2,400 curies released to the offsite environment due to the 1959 SRE event (Beyea 2006). ***<u>Frazier</u>*****<u>->A release of such large amounts of cesium-137, if indeed it had occurred, would have been detected at the time of the event in measurements of radioactivity in the helium cover gas</u>**

<u>Beyea Response 43.</u> *I have discussed in my report this argument and the one that follows about detectability of radiocesium at the time of the accident. Frazier does not address my counterarguments, preferring to ignore them. Cesium-137 would only have been detected in samples of the cover gas, if Cesium-137 were present during the sporadic samples that were collected. Pulsed releases would not have been detected. It is also not clear how much Cesium-137 would have had to be present to be detected, in the first place. What*

[16] DOE has recently placed some of defendants, but not plaintiffs, expert reports on its SSFL web site. Until Boeing promises no legal action for use of these sealed reports, and until plaintiffs' expert reports and rebuttals are released to provide balance, I cannot discuss the merits of these reports. Fortunately, the bulk of the arguments have been made publc by Boeing in its responses to the SSFL Panel reports.

methods were used to look for Cesium-137? Dr. Frazier does not tell us.

Frazier->**<u>…and other components of the SRE, in environmental samples following the event,</u>**

<u>Beyea Response 44</u>*. Good idea, but Dr. Frazier makes no quantitative estimates of what limits could be set on radiocesium releases using contemporaneous measurements. Gordon Thompson did use arguments about possible Cesium-137 deposits on pipes and surfaces to limit Cesium-137 releases to 50 Ci. The analysis was reasonable, so I included it in my linear pooling of expert estimates. However, the analysis was not definitive, because it implicitly assumes that there was no significant cleaning of surfaces and no significant removal of heavily contaminated piping and vents. I am not aware of any measurements made of activity before piping and other surfaces were cleaned or replaced. Measurements made after cleaning and replacement are not particularly helpful. I discussed this in my report. I do agree that, had, measurements been made immediately of surfaces inside piping, one could have set useful limits on releases, using a range for deposition velocities within piping. Also useful would have been measurements of the filter contents, although it would be necessary to assume the filter had not been in bypass mode and to assume a range of filter efficiencies. Measurements of residual radioactivity on the filters could have been made long after the pipes were cleaned or replaced. The fact that such measurements were never made indicates to me the possibility that no one dared make them, in case they contradicted the AI Committee's favorable conclusions based on limited data. Or, it is possible that no filter was in place at the time of the event.*

Frazier->**<u>….and in surface soil in the area surrounding the SSFL even today.<-</u>**

<u>Beyea Response 44.</u> *The interpretation of soil measurements of cesium-137 is intimately tied to assumptions about the rise of the plume. Frazier indicates in his report that he believes that soil concentration always decreases with distance from a release. If he is correct in that (unsubstantiated) view, then I am wrong. If he is correct, then measurements near the facility, the only ones that have been widely available prior to the information I received recently from Harold Beck, rule out a large release. If, on the other hand, if the Gaussian plume model calculations I present in my report that show soil concentrations peaking at a distance set by the plume rise are correct, then Frazier is wrong and the close-in soil* *measurements say more about the likely release height than the amount of curies of cesium released. My arguments are based on model calculations; his are based on assertions, without a single citation.*

Had Boeing allowed consultants to the SSFL panel like myself to have access to site-specific meteorological data for the 1990-1994 period, I would have been able to set constraints on cesium releases for each assumed plume rise. It was frustrating not to have that information, being forced to use the wedge model, which can't really account for plume rise accurately. By the way, calculating plume rise is particularly uncertain, even when one knows the heat rate, which we don't in this case. Thus, I don't want to overstate the significance of having the 1990-1994 met date for resolving the cesium release magnitude.

The new data provided by Harold Beck, which gives cesium soil deposits far from the site is very useful in setting limits on releases for certain wind directions. These are the kinds of measurements, along with milk measurements, that could have been made immediately after the accident, when knowledge of wind directions would have been known.

The presence of quantities of cesium-137 that would indicate a release to the atmosphere was not observed in any of the relevant measurements and analyses at the time of the event or in the years that followed.

The absence of elevated levels of cesium-137 in surface soil in the environs of the SRE site is discussed in Section IV.G.

B. Dose Estimates

In Chapter 3 and Appendix 2 of his report, Dr. Beyea presents the methodology and results of his calculations of radiation doses from iodine-131 and cesium-137 from the 1959 SRE incident. ***Frazier->*** **He uses radionuclide fate, transport, and dispersion models and parameters that overestimate offsite concentrations.** <-

Beyea Response 45. *In saying that I use techniques that overestimate offsite concentrations, I would have expected Dr. Frazier to explain. However, once again, he provides no backup for his assertions. The reader is supposed to accept this assertion on faith from a person with no published record in this field.*

Frazier-> **He then assumes exposure assessment pathways and parameter values that overestimate offsite doses** (Beyea 2006 at Appendix 2).

<u>Beyea Response 46</u>. *Same response as 45: In saying that I assume exposure pathways and parameter values that overestimate offsite doses, I would have expected Dr. Frazier to explain. However, once again, he provides no backup of his assertions. The reader is supposed to accept this assertion on faith from a person with no published record in this field.*

He presents tables of thyroid doses to offsite residents that are based on offsite releases of 10,000 curies and 20,000 curies of iodine 131 –hypothetical amounts that are unrealistically high and unsupported by the data.

<u>Beyea Response 47.</u> *The calculations of thyroid doses I made assuming 20,000 curies of radioiodine released were illustrative. To estimate consequences, I scaled these illustrative results according to the release distributions determined from linear pooling of expert likelihood distributions. Furthermore, a 20,000 Curie iodine release is only unrealistic, if one accepts the very low value of burn-up reported for the period of the July SRE event. That low value is inconsistent with the operators spending two weeks running the reactor. Finally, the 20,000-Ci release matches the measured release from the 1957 Windscale event, which is the one historical data point we have where an intensive effort was made immediately after the event to determine what got out, and to protect public health. The Windscale reactor was a government reactor; the SRE was a private reactor. Boeing, in its response, argues that Windscale was so different from SRE in having a direct path to the atmosphere. However, at SRE there is a good chance there also was a direct path to the atmosphere, certainly during the times the holding tanks were bypassed, assuming bubbling to the surface took place.*

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(As noted in preceding sections of this report, assessments performed soon after the 1959 SRE event showed that there was no release of radioiodine to offsite areas.) Even with the very large activity Dr. Beyea uses as input to his offsite dose calculations for various distances, ***Frazier->*** **<u>the individual thyroid doses to members of the public that he calculates are comparable to the annual dose that one receives in a year from natural background radiation sources in California</u>** – a small radiation dose.

<u>Beyea Response 48</u>. *I agree that the thyroid doses I calculate are comparable to the annual* *background from natural radiation. However, in contrast to Dr. Frazier, I consider in important to also consider issues of scale (Shrader-Frechette 2004), which I do by considering the cumulative dose. A 1/100th of a rem delivered to one person is different than 1/100th of a rem delivered to a million people. Although any expected increase in health effects may be lost in a sea of disease from other causes, each extra case or earlier onset is a social detriment and a problem for the unlucky individuals.*

Dr. Beyea's tables of radiation doses from cesium-137 are based on releases of 300 curies and 600 curies to offsite areas from the 1959 SRE incident. As noted previously in this report, such large releases of cesium-137 did not occur, were not indicated by measurements at the time, and are not indicated by soil sampling data even today. Although Dr. Beyea uses release estimates of a large quantity of cesium-137 to calculate the offsite doses from cesium-137, ***Frazier->*** **the resultant annual radiation dose from cesium-137 that he calculates is less than the dose each resident of the U.S. receives from natural background radiation sources.<-**

Beyea Response 49. *Same response as 48. I agree that the annual radiation dose from cesium-137 is less than the annual background from natural radiation. However, in contrast to Dr. Frazier, I consider in important to also consider issues of scale (Shrader-Frechette 2004), which I do by considering the cumulative dose. A 1/100th of a rem delivered to one person is different than 1/100th of a rem delivered to a million people. Although any expected increase in health effects may be lost in a sea of disease from other causes, each extra case or earlier onset is a social detriment and a problem for the unlucky individuals.*

Based on his own calculations of releases of radioiodine and radioactive cesium from the 1959 SRE incident, Dr. Beyea calculates offsite radiation doses that are within the range of natural background radiation doses in the U.S. As noted previously in this report, there was no radioiodine or radioactive cesium released offsite and, hence, there would have been no offsite radiation doses to anyone from radioiodine or radioactive cesium from the 1959 SRE incident.

C. Projected Health Effects

The key to Beyea's assessment of potential health effects is his calculation of collective (population) doses. He first calculates a radiation dose (in the radiation dose unit rem) for hypothetical persons (with undefined population and exposure characteristics) within each of 96 specific geographic areas (sectors) from the SRE site boundary to beyond 100 kilometers (approximately 62 miles) from the site. He then multiplies each sector-specific dose by the total population within that sector (as of 1960) to calculate the collective dose (person-rem) for each sector. Finally, he adds the collective doses for all 96 sectors to arrive at the population dose that he uses to assess a number of cancers that he claims would be due to the 1959 SRE event.

As noted in the preceding section, the radiation doses that Dr. Beyea calculates are very low, especially for locations more distant from the SRE site. However, the populations in many of those distant sectors are very large. Therefore, even though the doses he calculates are very low (within the variations of natural background doses), the collective (population) dose that he calculates appears to be very large because he includes distant locations having very large populations with very low doses. It is only through such calculations of multiplying very small doses (fractions of rem) by very large populations that he can arrive at large values of collective doses (person-rem).

Frazier->**<u>The preeminent international radiation protection organization, the International Commission on Radiological Protection [ICRP], reports that the method of multiplying very low doses by very large populations to calculate and interpret collective dose (the very method used by Dr. Beyea) is inappropriate and is not a valid predictor of adverse health effects from very small doses (such as the doses calculated by Dr. Beyea). The ICRP notes that collective dose is not intended as a tool for epidemiologic risk assessment and it is therefore inappropriate to use it in risk projections based on epidemiologic studies.</u>**

<u>Beyea Response 50.</u> *Dr. Frazier does not give a citation for this statement attributed to the ICRP rejecting the use of collective dose in risk assessment, but I presume he is referring to the following statement, which I shall argue below reveals a number of internal inconsistencies within the ICRP community:*

> *"Collective effective dose is not intended as a tool for epidemiologic risk assessment and it is therefore inappropriate to use it in risk projections for such studies. Specifically, the computation of cancer deaths based on collective doses involving trivial exposures to large populations is not reasonable and should be avoided. Such computations based on collective effective dose were never intended and are an incorrect use of this radiological protection quantity." http://www.icrp.org/draft_progress.asp 1/2007 draft*

The statement is illogical on its face, suggesting internal debate. The reasoning in the ICRP quote is that, because ICRP did not ***intend*** *to use it in epidemiologic risk assessment, it is* ***inappropriate*** *to use it in risk projections. That is a non-sequitor. The statement also claims computing trivial exposures to large population is not reasonable, but gives no reason. In Annex B of the draft recommendations, a reason is, in fact, given, namely that the risks at low dose are uncertain. However, this contradicts other ICRP scientific documents that say it is reasonable to assume the linear non-threshold model holds (see below). Furthermore, Annex B suggests it is actually OK to compute population doses, if* *they are included in a matrix of population doses computed with different dose cutoffs. If the matrix methods means providing more information about collective population doses, that is fine, but it certainly does not imply avoiding population dose calculations altogehter. I do not have a problem with a population-dose matrix, but I presume Frazier and HPS no doubt will. I suspect that the ICRP population dose-matrix in Annex B won't last long. Roger Clarke, who started the move in ICRP away from population doses was quite clear in his original paper that the reason for moving away from population doses was to retain credibility with industry (Clarke 1999). Industry is unlikely to accept a population dose matrix.*

The ICRP in its report on, "Low dose extrapolation of radioactive cancer risk," highlights concepts that seem to contradict Dr. Frazier's paraphrasing, as indicated in the following quotation:

> *"The LNT hypothesis, combined with an uncertain dose and dose-rate effectiveness factor (DDREF) for extrapolation from high doses, remains a prudent basis for radioactive protection at low doses and low dose rates."*

In any case, the ICRP, unlike the National Research Council and the Institute of Medicine, is not a scientific body, although is has many scientists involved with it. Having representatives from many countries and a role of balancing health and safety aspects with economic and other social factors (Clarke and Valentin 2005), the ICRP is necessarily political. Its latest recommendations about the collective dose have their roots in a desire to maintain credibility with industry (Clarke 1999). The statements Dr. Frazier attributes to the ICRP are policy statements, not scientific ones. Moreover, the policy Dr. Frazier is supporting is questionable policy, not just for dealing with the public regarding accidents, but also for Boeing, when it comes to clean up of the SSFL site. If those involved in cleanups were not allowed to use risk assessments involving collective doses, they would have a difficult time being able to justify what is safe enough. If fact, in a Department of Energy (DOE) environmental assessment of the cleanup of the reactor section of the SSFL site, the authors do exactly what Frazier says I shouldn't do. (Moreover, DOE uses ICRP numbers for that very purpose.[17])

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[17] *"The estimates are based on the dose received and on dose-to-health effect conversion factors recommended by the International Commission on Radiological Protection (ICRP 1991). The Commission estimated that, for the general* population, a collective dose of 1 person-rem will yield 0.0005 excess latent cancer fatality." (DOE 2002)

> *"Under Alternative 1, the expected latent cancer fatalities in a population of 500 people living on the ETEC site following remediation to the 3 x 10^{-4} theoretical lifetime cancer risk standard (not taking ALARA into account) would be 0.15 as a result of residual radiological contamination." (DOE 2002)*

What it appears that Dr. Frazier is inadvertently maintaining is that it is legitimate to use collective dose to compute cancer estimates, if the numbers are small enough to reassure the public about cleaning up Boeing's inherited contamination, but it is wrong to use them when they might reflect badly on Boeing. Or, possibly, Dr. Frazier would not just criticize me, but also criticize DOE for estimating the number of latent cancer fatalities that might result from its alternative clean-up plans at SSFL.

As noted in the preceding section, Dr. Beyea calculates very low doses to offsite individuals as a consequence of the 1959 SRE incident, ***Frazier-***>**<u>even though he incorporates assumptions of large release quantities and environmental exposure assessment parameters that maximize his calculated doses. <-</u>**

<u>Beyea Response 51</u>*. There is no basis given anywhere in Dr. Frazier's report for this statement that I have incorporated parameters that maximize the calculated dose. He cites to a preceding section, but that section only includes another unsupported statement, namely his claim that that I overestimated doses through choice of parameters. Now, in this current section, he escalates the charge to say that I maximized the calculated dose. In both sections, Dr. Frazier simply makes assertions without any backup analysis. Argument by repetitive assertion.*

Several organizations have addressed the inappropriateness of calculating health risks at such low doses. For example, the Health Physics Society (the 6,000-member professional organization of radiation safety professionals) issued a Position Statement in 1996 (revised in 2004) entitled "Radiation Risk in Perspective"(HPS 2004). The following is an excerpt from that statement.

> "In accordance with current knowledge of radiation health risks, the Health Physics Society recommends against quantitative estimation of health risks below an individual dose of 5 rem in one year or a lifetime dose of 10 rem above that received from natural sources. Doses from natural background radiation in the United States average about 0.3 rem per year. A dose of 5 rem will be accumulated in the first 17 years of life and about 25 rem in a lifetime of 80 years. *HPS->* ***<u>Estimation of health risk associated with radiation doses that are of similar</u>***

magnitude as those received from natural sources should be strictly qualitative and encompass a range of hypothetical health outcomes, including the possibility of no adverse health efects at such low levels." (HPS 2004)

Frazier->**The radiation doses calculated by Dr. Beyea are less than the doses received from natural background radiation sources. Hence, Dr. Beyea's quantitative calculation of health risks from those doses cannot be supported by the science and, in fact, adverse health risks from such low doses may actually be zero.<-**

Beyea Response 52. *Based on a* **policy statement** *by the Health Physics Society and the assumption that doses comparable to, or below, background are of no significance, Dr. Frazier says that my quantitative calculations of health risks cannot be supported by the science. There is no science about low-level radiation doses discussed in the quotation from the Health Physics Society;*[18] *nor does Dr. Frazier discuss the science of this issue at all in his report. He doesn't even respond to the discussion in my report about new studies that show health effects at average doses at 2-rem and linearity down to very low doses. Look at the dose-response for the Techa Cohort, which I reproduce below. Note that 0.2 on the horizontal axis is 20-rem. There are 3 positive points below 20-rem, one of which is at 2-rem. The best-fit lines indicate linearity. The linear- quadratic fit fails to suggest a threshold. (The graph is reprinted from (Krestinina et al. 2005).)*

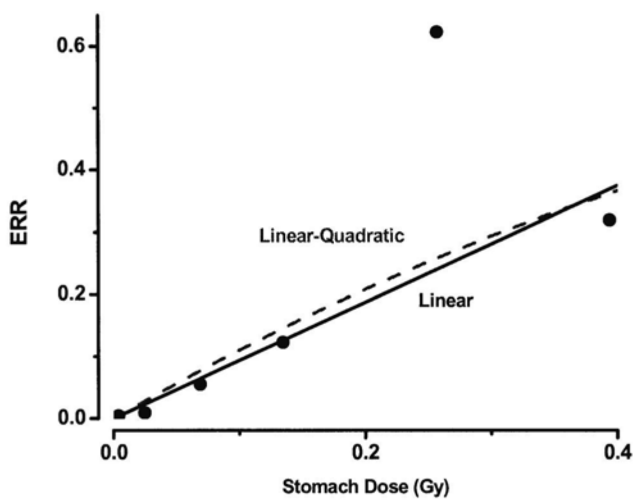

FIG. 1. ETRC solid cancer dose response.

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Dr. Frazier is free to challenge my calculations as having some socially bad effect, but he does *not do so. Dr. Frazier is free to challenge them on scientific grounds, but he does not provide any basis for such a challenge, other than to say the adverse health effects may be zero. Nor does he provide any scientific evidence of a threshold. In any case, what difference does it make if the adverse health effects may be zero, when it comes to making quantitative risk calculations that include a range? If I had not included zero as the lower range of health effects in my report, Dr. Frazier may have had a point. But, I did include zero as the lower limit. Dr. Frazier seems to forget that the health effects of low-level doses may also not be zero, and the latest evidence that I discuss in my report makes that seem very likely. There is a range here, which should be discussed openly. I realize that those who have a vested interest in nuclear power do not think it is fair that they be penalized, when there may be a possibility of zero health effects at low doses, but I believe that the public (and my epidemiology audience) is entitled to know the complete range of expert opinion. Furthermore, advocates for a zero effect do not give a likelihood for it actually occurring. In my revised report, I have added a 10% probability that there is no health effect at the doses received at SSFL. The HPS statement goes on to say,*

***HPS**->* **"There is substantial and convincing scientific evidence for health risks following high-dose exposures. However, below 5–10 rem (which includes occupational and environmental exposures), risks of health effects are either too small to be observed or are nonexistent.<-**

***Beyea Response 53**. I note that the Health Physics Society is not a scientific body, although it has many scientists as members. No advance degree is required to be a member. In any case, this statement about health effects below 5-10 rem being too small has been falsified by the Techa River Cohort study, which shows a linear curve down to 2-rem. Dr. Frazier does not say a word about the Techa River Cohort, the Cardis Worker study, or dose response data from Chernobyl studies on thyroid cancer (Sadetzki et al. 2006), (Jacob et al. 1999). He does not discuss the fact that we are not talking about linearity down to zero dose, but down to background plus accident doses. For there to be zero health effects at zero accident dose, the overall dose response must drop precipitously between 7-rem and 5-rem, where 5-rem is a 50-year dose to the whole body from background radiation.*

The Health Physics Society also states:

***HPS->*Epidemiological studies have not demonstrated adverse health effects in individuals exposed to small doses (less than 10 rem) delivered in a period of many years.<-**

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[18] *Positions papers of medical societies include citations for scientific claims. Not so in the Health Physics Society.*

<u>Beyea Response 54</u>. The above claim that epidemiological studies have not demonstrated adverse health effects below 10-rem has been falsified by data from the Techa River Cohort that shows linearity down to 2-rem. It has long been falsified down to 5-rem by the A-bomb survivor data.[19] I can't help wondering why the Health Physics Society has not updated its statement. No doubt, it is difficult to have one's long-held beliefs refuted so dramatically. I am sure that, had the Techa River Cohort study shown a threshold at 10-rem, the Health Physics Society would have updated its statement immediately to strengthen it. I am equally sure that Dr. Frazier would have cited it.

VI. OCTOBER 5, 2006 REPORT OF DAVID A. LOCHBAUM

The report of David A. Lochbaum, prepared for the SSFL Advisory Panel, dated October 5, 2006, presents his estimates of the amounts of gaseous radioactivity, especially radioiodine and cesium that was released to the offsite environment from the 1959 SRE event (Lochbaum 2006). **<u>I (*Frazier*) have reviewed Mr. Lochbaum's report and have the following comments and conclusions regarding that report.</u>**

<u>Beyea Response 55</u>*: Dr. Frazier is not a nuclear engineer, nor does his resume indicate any familiarity with reactor physics. Thus, his analysis of David Lochbaum's report must stand on its own, based on Frazier's arguments and citations, which are minimal. In what follows, all of Frazier's citations are to Boeing's consultants or to AI employees. He does not discuss reports from plaintiffs' experts in litigation, which means his analysis is not balanced.*

A. Gaseous Fission Products That Escaped from the Fuel

Although Mr. Lochbaum is correct in stating that gaseous fission products escaped from the reactor core into the sodium coolant and that some of that gas passed into the helium cover gas or into the high bay area and was released offsite, ***Frazier***->**<u>he fails to note that those gaseous fission products were limited to radioactive isotopes of krypton and xenon.</u>**

<u>Beyea response 56</u>*: Dr. Frazier assumes the answer that is under debate by saying that only krypton and xenon were released. Not everyone agrees with his statement that only noble*

[19] *"There is no indication of upward curvature in dose, and the smoothed nonparametric estimate even at doses as low as 0.05 Sv (5-rem) coincides with the linear regression on the full dose range." (Preston et al. 2003). 5-rem conversion added.*

gases escaped. Experts like Dr. Arjun Makhijani, who prepared a report for plaintiffs in the recent litigation, disagree, as, of course, does David Lochbaum. Peculiar way to argue, namely, by ignoring the arguments of those who have different opinions.

Frazier->(Dr. Beyea) ignores the measurement data obtained following the 1959 SRE incident that showed that of the fission products present in the reactor core or the sodium coolant, only the inert (noble) gases krypton and xenon escaped into the helium cover gas or the high bay area.<-

Beyea Response 57*: Dr. Frazier assumes the answer, once again. He assumes that the measurement data can be used to infer zero release of radioactivity other than noble gases. Moreover, he does not cite such data. To what is he referring? Secret data? Data unavailable to the public? Data used by defendants in litigation? How can anyone judge its reliability? If he is referring to data in public AI reports, then he is ignoring the discrepancy in the ratio of cesium to iodine found in the coolant that indicates missing iodine. He is ignoring the fact that measurements of samples taken from the cover gas were sporadic.*

Lochbaum goes through an analysis as to why he can't rely on all of the data that is available to him. If there is other data Lochbaum has not seen, it should be made available to him. However, Frazier doesn't cite anything new, other than the reports of Boeing's litigation experts. If Frazier is relying on data discussed by Boeing's litigation experts that was unavailable to Lochbaum, it would be premature to conclude that Lochbaum would necessarily agree with their interpretation. Experts on the other side of the litigation would have seen any data used by Boeing's experts and they didn't accept the interpretation of Boeing's experts. Lochbaum might not either.

Instead, Mr. Lochbaum uses data from another reactor incident, at Fermi I in 1966, to speculate that the amount of gaseous fission products released from the SRE fuel elements into the sodium coolant was significantly higher than the amount released in the Fermi I incident and that the gaseous fission products that were released from the sodium coolant contained large amounts of radioiodine and cesium (Lochbaum 2006 at pp. 14-15). The principal basis for his opinion in this regard is the difference between radiation readings at the two facilities. ***Frazier->*** **There can be numerous causes of such differences between readings** (e.g., differences in quantities measured, differences in the type and location of the detectors at the two facilities, differences in the range and calibration of those detectors, etc.) that are unrelated to Mr. Lochbaum's conclusion that the SRE event released radioiodine and cesium to the environment.

Beyea response 58*: This is a very revealing passage. Dr. Frazier discounts data that he doesn't like in this passage, finding reasons why it might not be relevant. This kind of speculation can always be made of data, no matter where it occurs. Dr. Frazier is certainly correct to point out other explanations that may chance data interpretation, although I point out that the discrepancy that Mr. Lochbaum notes was quite large. The problem comes when he criticizes others for doing the same. Based on this passage, when data contradicts his opinion, he waves it away, unlike his behavior towards data he likes. There is no balanced, critical analysis in Dr. Frazier's report, for instance, of data relied upon by Boeing experts. I invite third-party readers to note whether, as I believe, that Dr. Frazier in his report judges data that supports his views by its strengths and he judges data that contradict his views by its limitations. This is a partisan approach.*

Here are some quotes from David Lochbaum's report that indicate a dearth of measurements. Dr. Frazier presents no evidence that measurements were complete.

> *"The first storage tank sample taken on July 15, 1959, after the start of run 14, indicated an extremely high activity; so high in fact that the counter had not been calibrated in that range" AI – 1959"*

When data goes out of range, as indicated in the above quotation, it is an indication that something unusual is happening. There is no indication in this quotation that isotope-specific measurements were taken. In general, the operators may have been assuming a noble gas release, rather than determining, if extra isotopes were present.

Here is another quotation from a 1959 AI report taken from Lochbaum's report:

> *"The first helium cover gas sample during run 14 which gave an indication of unusually high radioactivity was taken on July 18. Since the radiation level at the surface of the 2-liter sample chamber was ~30 mr/hr, no further attempt was made to assay the concentration quantitatively. "*

So, based on this quotation, there was no attempt on the first sample to assess the makeup of the cover gas. Therefore, it is not valid to say that only noble gases escaped. David Lochbaum also looked for evidence about the number of discharges to the atmosphere, which he found missing:

> *"Unfortunately, no data was found in the documents reviewed regarding the number of (or absence of) discharges from the gaseous storage tanks*

following the July 13th event. Thus, it is impossible to confirm or refute the assertion that "no radiological hazard was present to the reactor environs" via the gaseous storage tank pathway."

Reading Dr. Frazier's report alongside David Lochbaum's, it is not clear to me if Dr. Frazier has access to documents not available to David Lochbaum, possibly documents quoted in reports compiled by Boeing's litigation experts, which were not available to Lochbaum. If so, it must be noted that there is always another side to a story. Dr. Frazier is not presenting to the reader the views represented by plaintiffs' experts in the litigation against Boeing, who would have access to all documents available to defendants' experts.

B. Fraction of Radionuclide Inventory Released Offsite

Mr. Lochbaum notes that noble gases (krypton and xenon) are released relatively quickly from the fuel into the sodium coolant and that because of their very low solubility in sodium they pass rapidly from the sodium coolant into gas space (Lochbaum 2006 at p. 16). *However, he fails to note the indisputable scientific facts that radioiodine and cesium are released from the fuel much slower than noble gases are released and the radioiodine and cesium interact with the sodium coolant very efficiently to prevent their escape into any gas space (such as into the helium cover gas).*

Beyea response 59: *Note the claim of "indisputable scientific facts" in the above passage. Dr. Frazier claims it is scientifically indisputable that radioiodine and cesium interact with the sodium coolant very efficiently to prevent their escape. This passage provides another example of how Dr Frazier makes errors when he steps outside his field and fails to check himself by looking up references in the field. It is true that, if all the radioiodine and radiocesium entered the coolant gas and were well mixed, it would be hard for a large percentage to escape. However, these isotopes need not go into the coolant. They can rise to the surface of the coolant inside bubbles of sodium vapor. Moreover, given an overheating scenario, which is necessary to get a lot of these isotopes out of the fuel in the first place, vapor formation and bubbling is to be expected. Lochbaum quotes passages from AI documents indicating that vapor formation was likely.*[20]

These differences in reactor release rates and retention fractions in the sodium coolant are the very reasons that only krypton and xenon escaped during the 1959 SRE incident and that radioiodine and cesium were not released from the SRE reactor into the offsite environment.

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20 *"It is clear that still higher temperatures were reached in the region of failure, and it is likely that sodium boiling occurred in that region." Lochbaum citation to, AI – 1961a.*

Frazier-> **Mr. Lochbaum states that "the data do not permit a quantitative analysis and prompt a turn to a qualitative assessment." (Lochbaum 2006 at p.15). However, he proceeds to calculate (quantitatively) a range of release fractions.**

Beyea Response 60. *Dr. Frazier sees a contradiction between a qualitative analysis and a numerical range. I disagree. A qualitative analyses leads to a large range in estimated values, exactly what Lochbaum gives.*

He then uses his own "balancing factors" to conclude that a value of 15 % was closer to the actual release fraction for each of those radionuclides (Lochbaum 2006).

C. Consequences of Lochbaum's Release Estimates

If the amount of cesium (specifically cesium-137) indicated by Mr. Lochbaum's calculations had actually been released offsite as a consequence of the 1959 SRE incident, ***Frazier->*****there would have been (and would continue to be) measurable quantities of cesium-137 in the surface soil on and around the SRE site.<-**

Beyea Response 61. *Dr. Frazier repeats his assertion that a large release of radiocesium means high values in the soil close to the SSFL. As I have said before, this is based on his false premise stated by him earlier, that concentrations always decline with distance from the release point. His assumption is only true for ground level releases. In my report, I estimated how high the plume would have to rise to give predicted values consistent with a 300-Ci release, using the Gaussian plume model for typical meteorological parameters. Dr. Frazier has ignored my calculations, for reasons that are a mystery to me.*

However, concentrations of cesium-137 in soil have been measured at various times at numerous locations in the environs of the SRE site and none of those samples had concentrations of cesium-137 above the range of soil concentrations due to global fallout from atmospheric weapons testing.

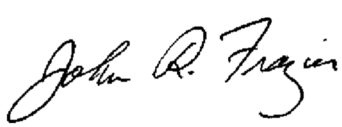
Beyea response 62: *Dr. Frazier makes assertions above about soil concentrations of radiocesium attributed to weapons testing fallout. He fails to state where he came up with soil concentrations due to global fallout, which Harold Beck, who knows much more about this issue than either Dr. Frazier or myself, tells me is not easy to do in complex terrain. Dr. Frazier also fails to indicate a standard error assigned to the supposed* *background levels, which would allow a scientist to state the level of radiocesium from atmospheric weapons tests that could be excluded by the measurement at, say, the 95% confidence limit. I note that the UCLA group working on SSFL also expressed some concern about what the fallout levels really were, suggesting more analysis is needed (UCLA 2006). I do not want to imply by my response to this section that, had Dr. Frazier made a full statistical calculation, he would have changed the thrust of his criticism of David Lochbaum. What may have been different is that he could not rule out a release that would have made Boeing uncomfortable. By my back-of-the-envelope calculations it would not be possible to rule out a ground release of the order of 10 to 30 Ci of radiocesium.*

Frazier->**Environmental monitoring data clearly show that releases of cesium-137 from the site as claimed by Mr. Lochbaum never occurred.**

Beyea response 63*: Dr Frazier can only make the claim that environmental monitoring rules out the Lochbaum releases, because Dr. Frazier (mistakenly) believes that soil concentrations must always decrease as one moves away from the site. This is not necessarily true for elevated releases, as I discussed earlier in Response # 61.*

Note that I have two more responses after Dr. Frazier's signature, responses 64 and 65.

Prepared and submitted by:



John R. Frazier, Ph.D.
Certified Health Physicist

ATTACHMENT A
CURRICULUM VITAE OF JOHN R. FRAZIER

JOHN R. FRAZIER, Ph.D., CHP

Professional Qualifications

Dr. Frazier has over 29 years of health physics experience in external and internal dosimetry, environmental dose assessment, radiation risk assessment, radiation spectroscopy, health physics training, bioassay, radiation detection and measurement, and radiological site characterization. Numerous federal agencies including the Nuclear Regulatory Commission (NRC), Environmental Protection Agency (EPA), U.S. Department of Agriculture (USDA), U.S. Department of Defense (DOD), and U.S. Department of Justice (DOJ) have sought his advice on a wide range of health physics and radiation protection topics from operational health physics program design to environmental radiation dose and risk assessments. He has also served as a consultant to private companies and individuals on numerous health physics issues. He is an elected member of the National Council on Radiation Protection and Measurements (NCRP). Dr. Frazier has made presentations on introductory and advanced health physics and radiation protection topics for professional society meetings, student groups, and public interest forums. ***Frazier speaking about himself->*****His publications are in the areas of fundamental interactions of radiation with matter, radiation detection instrumentation, radiological site assessments, and external and internal radiation dosimetry.**

Beyea response 64*: Dr. Frazier states in the underlined passage above that he has publications in four fields. I tried to match these fields with the 5 publications he actually lists later in his resume, but could not do so. The confusion may arise because he states that he has prepared or contributed to more than 100 publications in the fields of health physics and environmental sciences, but neither lists them nor gives an explanation as to why he has not listed them. I presume, then, that Dr. Frazier is counting these missing publications in his statement above. Or, perhaps, he sees greater breadth in the five publications that are listed than I do.*

Education

Ph.D., Physics, University of Tennessee, Knoxville, Tennessee; 1978.

M.S., Physics, University of Tennessee, Knoxville, Tennessee; 1973.

B.A., Physics, Berea College, Berea, Kentucky; 1970.

Registrations/Certifications

Certification by the American Board of Health Physics in 1981; recertified through

2009.

Experience and Background

2004- *Present* — *Independent Health Physics Consultant*

Dr. Frazier provides consultation services on a wide range of radiation protection issues for private companies, government agencies, and individuals. His principal areas of expertise are internal and external radiation exposure assessments, environmental radiation dose and radiological risk assessments from occupational and environmental exposures, and evaluations and assessments of all aspects of operational health physics programs.

1993-2004 — ***Senior Radiological Scientist, Auxier & Associates, Inc., Knoxville, Tennessee.***

Dr. Frazier served as senior consultant on radiation protection issues for private companies and government agencies. He performed assessments of internal and external radiation exposures, environmental radiation doses and radiological risks from occupational and environmental exposures. He also performed evaluations and assessments of all aspects of operational health physics programs. Dr. Frazier served as technical advisor to organizations that performed environmental radiological assessments and risk assessments and that provided occupational radiation protection services in government and industry.

1986-1993 — ***Senior Radiological Scientist, Nuclear Sciences, IT Corporation, Knoxville, Tennessee.***

Dr. Frazier served as senior radiological scientist and technical manager of the health physics consulting group within IT. He was responsible for health physics professional services provided by IT for federal, state, and local agencies, contractors, and private companies. These services included development of all aspects of the health physics programs for nuclear facilities, technical assessments and evaluations of existing health physics programs, and environmental and occupational radiation dose assessments. He served as technical advisor and task manager for radiological aspects of remedial investigations and feasibility studies (RI/FSs). He also served as manager and technical director for specific projects in areas that included design and implementation of environmental monitoring and sampling programs, assessment of operational health physics programs, and radiation dose and risk assessments for occupational exposures and environmental releases. Previous responsibilities included serving as senior technical consultant for upgrading Environmental Health and Safety Programs at the Department of Energy Rocky Flats Plant, Oak Ridge National Laboratory, and the Oak Ridge Y-12 Plant.

1980-1986 — ***Health Physicist, Oak Ridge Associated Universities, Oak Ridge, Tennessee.***

Dr. Frazier developed and coordinated Oak Ridge Associated Universities (ORAU) health physics training programs. He taught health physics and radiation protection courses for several hundred students each year at ORAU Professional Training

Programs. He developed new lectures, laboratory exercises, and training materials for health physics training for the Nuclear Regulatory Commission, Department of Energy, and corporate clients. In addition to his training responsibilities, Dr. Frazier served as division health physicist for the Manpower Education, Research, and Training Division of ORAU. He served as technical consultant to federal and state agencies, other training institutions, and ORAU clientele on environmental, health and safety issues. He evaluated radiation measurement and radiation protection instrumentation equipment.

1978-1980 ***ChiefRadiation Physics Section, Bureau ofRadiological Health, Rockville, Maryland.***

Dr. Frazier supervised research and support activities of a staff of seven health physics professionals and technicians. He planned and implemented radiation research projects pertaining to ionizing radiation detection/ measurement. He scheduled personnel requirements in accordance with the scope of such projects. He coordinated support for external radiation dosimetry by the Radiation Physics Section for all other branches in the Division of Electronic Products. He supervised and performed multi-point calibrations of radiation detection/ measurement instruments per month. Dr. Frazier also assisted in planning radiation dosimetric surveys of large numbers and types of ionizing radiation sources to reduce population exposure. He coordinated environmental radiation dosimetry for extended geographical areas using external radiation dosimeters.

1977-1980 ***Research Physicist, Bureau ofRadiological Health, Rockville, Maryland.***

Dr. Frazier calibrated X-ray detection/measurement instruments. He maintained radiation calibration secondary standards traceable to the National Bureau of Standards. He evaluated new X-Ray detection/measurement instruments with radio-frequency fields under controlled environmental conditions and a wide range of ionizing radiation fields. He also developed external radiation dosimetry techniques with both active and passive dosimeters.

Awards/Activities

Fellow, Health Physics Society, 2000
Elda E. Anderson Award, Health Physics Society, 1988
Senior Technical Associate, IT Corporation, 1988
Distinguished Technical Associate, IT Corporation, 1990
National Council on Radiation Protection and Measurements (NCRP)
  Council Member, 2002-2008
  Scientific Committee 46, 1999-2006
  Scientific Committee 2-1, 2004-2006

Professional Affiliations

Health Physics Society
  (Plenary Membership since 1981; President, 2002-3; President-Elect, 2001-2; Board of Directors, 1992-5; Treasurer-Elect, 1997-8; Treasurer, 1998-2000)
American Academy of Health Physics (Secretary, 1996-1997, Director, 1998)
East Tennessee Chapter of the Health Physics Society (Past President)
International Radiation Protection Association (Plenary Membership)

Publications

***Frazier speaking of himself:* Dr. Frazier has prepared or contributed to over 100 reports and publications in the fields of health physics and environmental science.**

Beyea Response 65. *Dr. Frazier mentions that he has prepared, or contributed to, over 100 reports, but only lists 5 in this section and no others anywhere else in his resume. When asked in a 1998 legal deposition about the discrepancy between the 100 or more reports and publications claimed above and the five actually listed below, Dr. Frazier explained:*

> *"Nearly all of those have been project-specific reports that are not peer-reviewed, other than within the confines of the project. Although they may be reviewed by external organizations, they are not peer-reviewed within, like, journals." (Frazier et al. 1998).*

However, Dr. Frazier did not explain why these publications were not listed on his resume. Without the list of missing publications, we have no idea of the publication's relevance, the magnitude of the effort, or the number of authors involved. Nor can we track them down to read them. He lists three reports among his five publications, so the mere fact that the missing 100 are reports, rather than journal articles, cannot be the explanation. Not having access to a list of the missing reports, let alone their content, I cannot use them to evaluate Dr. Frazier's breadth of expertise. I note that the fifth and last publication listed below was prepared in 1989. The last publication on the list on which Dr. Frazier was lead author was 1979. These publications were all prepared before pooling of experts was widely used in the nuclear field, which is perhaps the reason why Dr. Frazier was not familiar with the methodology.

List ofPublications

Frazier, J. R., "Negative Ion Resonances in the Fluorobenzenes and Biphenyl" Ph.D. Dissertation, University of Tennessee, Knoxville, Tennessee, 1978.

Frazier, J. R., "Low-Energy Electron Interactions with Organic Molecules: Negative Ion States of Fluorobenzenes," Journal of Chemical Physics, Vol. 69, No. 3807, 1978.

Frazier, J. R., "Performances of X-ray Measurement Instruments in RF Fields," HEW Publication (FDA) 78-8065 Rockville, Maryland, 1978.

Frazier, J. R., "A Dosimetry System for Evaluating Chest X-Ray Exposures," HEW Publication (FDA) 79-I107,1979.

Film Badge Dosimetry in Atmospheric Nuclear Tests, National Academy Press, Washington, D.C., 1989.

ATTACHMENT B
LIST OF DOCUMENTS
(Frazier's list)

List of Documents Reviewed (by Dr. Frazier)

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(Beyea 2006) Jan Beyea, Ph.D., "Feasibility of Developing Exposure Estimates for Use in Epidemiological Studies of Radioactive Emissions from the Santa Susana Field Laboratory, Report to the Santa Susana Field Laboratory Advisory Panel, A Project of the Tides Center", Revision 0b, October 5, 2006.

(Christian 2005) Jerry D. Christian, Ph.D., "Chemical Behavior of Iodine-131 during SRE Fuel Element Damage in July 1959 Response to Plaintiffs' Expert Witness Arjun Makhijani", May 26, 2005.

(Daniel 2005) John A. Daniel, Sr., "Investigation of Releases from Santa Susana Sodium Reactor Experiment in July 1959", May 27, 2005.

(EGG 1979) EG&G Energy Measurements Group, "Aerial Radiological Surveys of Rockwell International Facilities", EGG-1183-1751, October 1979.

(EPA 1998a) U.S. Environmental Protection Agency, Office of Radiation and Indoor Air, Memorandum from J. Griggs to T. Kelly, "Radiochemical Results for Bell Canyon Samples", November 20, 1998.

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(Hamilton 1997) Dr. Terry F. Hamilton, Technical Report, "An Investigation on the 137Cs Content of Soil Collected from the Boeing North America, Inc., Employees' Recreational and Fitness Center in Canoga Park (CA)", June 20, 1997.

(HPS 2004) Health Physics Society, "Radiation Risk in Perspective- Position Statement of the Health Physics Society", August 2004.

(Lochbaum 2006) David A. Lochbaum, "An Assessment of Potential Pathways for Release of Gaseous Radioactivity Following Fuel Damage During Run 14 at the Sodium Reactor Experiment", Prepared for the Santa Susana Field Laboratory Advisory Panel, October 5, 2006.

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(Ogden 1998) Ogden Environmental and Energy Services Co., Inc., "Bell Canyon Area Soil Sampling Report Ventura County, California, Volume I", October 1998.

(QST 1999) QST Environmental, Letter Report to GreenPark Ventures, LLC, "Results of Preliminary Soil Sampling at Runkle Ranch in Simi Valley, California", February 5, 1999.

(Rockwell 1994) Rockwell International/Rocketdyne Environmental Lab, "Gamma Spectrum Analysis Report", June and July 1994.

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Frazier JR, Baron FM, Scott EK. 1998. Deposition of Dr. John R. Frazier in Hall v. Babcock and Wilcox (Apollo case), Civil Action No. 94-0951, US District Court for the Western District of Pennsylvania,. Philadelphia, PA: Fred M. Baron, attorney for plaintiffs, Ellen K. Scott, attorney for defendants.

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