Beyea Response Document 2

Line-by-line responses by Jan Beyea, Ph.D. to comments by the Boeing Company on a) Beyea's report on projected health effects b) the report of David Lochbaum on releases, and c) the summary report of the SSFL Advisory Panel.

Supplementary material provided for the report,

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

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

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

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June 11, 2007

**pursuant to a contract with the Panel under a grant from the Citizens Monitoring and Technical Assistance Fund, c/o RESOLVE, 1255 23rd Street, NW, Suite 275, Washington, DC 20037. (http://www.mtafund.org/). Additional support came from funds provided for the Panel's work by the California State Legislature through a contract with the California Environmental Protection Agency. The statements and conclusions expressed in this report are those of the author and do not necessarily represent those of the SSFL Advisory Panel, the Tides Center, the CMTA Fund, or CAL-EPA. The mention of commercial products, their source, or their use in connection with material reported herein is not to be construed as actual or implied endorsement of such products.*

Note to Reader: All italicized and bolded sections are by Beyea;[1] all other sections are by Boeing.

In this part of my response, I have annotated the comments made by Boeing. All my responses are italicized and bolded; formatted with hanging indents. I begin with some general comments that deal with many of the key points raised in Boeing's response.

I agree completely with Boeing about the importance of putting the projected health-effects estimates in context of other radiation exposures, such as natural background radiation. The debate is not over issues of undue individual risk; it is a debate about the social responsibility of the owner, which is now the Boeing Company.

I also agree that press reports failed to capture the full range of my estimates, which included zero health effects as a very real possibility – just as real as the higher range of health effects projected. I very much regret this omission.

In criticizing Boeing, I do not mean to "demonize" the Company in any way. I recognize that Boeing is a major American institution. It has played an important role in US corporate history. It has many fine employees. The Company finds itself in a difficult position, having inherited problems of smaller companies it has acquired. Because of reports like mine, it could face additional litigation and resistance to clean-up activities, not all of which will necessarily be justified. If the Company doesn't vigorously defend itself, no one will. However, I don't think Boeing is going about its defense in the right way. It is overstating its case, which will inevitably lead to loss in public credibility. I think a "truth and reconciliation" approach would be more effective, but I am not in the Company's shoes. It will be up to third parties to make the judgment as to which of us has the best approach to dealing with the 1959 accident and its consequences, if any.

Readers should be aware that, in its response, Boeing is silent about a number of issues raised in my report:

Boeing is silent about the withholding of meteorological data. The withheld data includes

[1] To avoid confusion, I have removed the italics used by Boeing to highlight quotations it used.

both the (limited) 1959 data collected during the accident and data collected in the 1990s that give details on typical correlations between wind speed, stability class and direction – information needed to make standard projections of health effects.

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

Boeing is silent about the possibility that there was sufficient heat released to cause bubbling, which would have provided a pathway for radioactivity through the sodium coolant above the reactor core.

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

Boeing is largely silent about the possibility of elevated releases, which would have eliminated the usefulness of the best-known soil measurements in limiting the release magnitude for radiocesium. In one section, Boeing does acknowledge that I make a claim about elevated releases being difficult to detect, but claim I provide no evidence of this. In fact, I do provide such calculations in Chapter 2 of my report. (They have been expanded for the revised report. Furthermore, in reviewing my calculations, because of Boeing's criticism, I realized that I had not properly accounted for weather frequency data, which led to a downward revision of the upper 95%-confidence projection for total cancers by a factor of 2.

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

Boeing is silent about what happened to the filters in place during the July run. Measurement of the cesium activity on them, if indeed, filters were in place at the time, would give an excellent idea of the amount released.

November 3, 2006

Panel Members and Consultants
Committee to Bridge the Gap
1637 Butler Avenue, Suite 203
Los Angeles, CA 90025

Subject: Comments of The Boeing Company on the Reports of the Santa Susana Field Laboratory Advisory Panel: *the Panel Summary Report, An Assessment of Potential Pathways for Release of Gaseous Radioactivity Following Fuel Damage During Run 14 at the Sodium Reactor Experiment,* David A. Lochbaum, *Feasibility of Developing Exposure Markers for use in Epidemiologic Studies of Radioactive Emissions From the Santa Susana Field Laboratory,* Jan Beyea, Ph.D., *Geologic Features and their Potential Effects on Contaminant Migration, Santa Susana Field Laboratory,* Howard G. Wilshire, Ph.D., *An Analysis of the Design and Performance of the Clay Cap Used to Control Groundwater Recharge into the Fractured Bedrock Beneath the Former Sodium Burn Pit (FSDF) at the BoeingRocketdyne Santa Susana Field Laboratory,* William C. Bianchi, Ph.D., and *Land-use Conversion and its Potential Impact on Stream/Aquifer Hydraulics and Perchlorate Distribution in Simi Valley, California,* M. Ali Tabidian, Ph.D.

Dear Panel Members and Consultants:

The Boeing Company appreciates the opportunity to comment on the reports of the Santa Susana Field Laboratory (SSFL) Advisory Panel (AP). Our detailed comments on each report are provided in an attachment to this letter. However, Boeing has a number of general comments which are set forth below. Taken as a whole, these comments seriously question the validity of claims the AP has made; claims that are flawed, without scientific merit, and a great disservice to our employees and the community.

Over the years, we have provided many surveys and reports to the local, state, and federal agencies overseeing the cleanup of the site. These reports have extensively documented the effects of past site operations through detailed monitoring of air, water, and soil, and the data included in these reports have been used by government agencies to determine the health implications of SSFL operations for our employees and the community. We regularly prepare reports and provide data to the regulatory agencies. This information is available to the public.

Sodium Reactor Experiment
Evidence from the Sodium Reactor Experiment (SRE) post-accident measurements of sodium and cover gas indicate that no iodine-131 or cesium-137 escaped from the

sodium into the cover gas, and therefore neither of these fission products was released to the environment. This evidence is supported by a large amount of operational history and research into the retention of fission products (including I-131 and Cs-137) in sodium coolant, including experience from the Fermi-I and EBRII fuel damage incidents.

Beyea Response 1. ***Boeing may well be correct that releases were negligible, but other experts come to a different conclusion. I developed a likelihood distribution that incorporates the full range of expert views. Science is often "contested territory." The methods I used in my report to combine disparate expert opinion into an overall likelihood distribution were originally developed in the 1980s and 1990s to deal with these kinds of disputes among experts. I provide citations to this literature in my revised report. I should have cited it in my original report.***

The AP reports ignore the fact that measurements of the SRE cover gas indicated only xenon-133 and krypton-85 noble gases and no iodine-131 and no cesium-137. The AP reports ignore the fact that activity measurements of the gas hold-up tanks prior to venting, indicated only approximately 28 curies of Xe-133 and Kr-85 gases were released.

Beyea Response 2. ***A pathway through the hold-up tanks is not the only escape route, because the hold-up tanks were very likely bypassed for all or part of the accident.***

Both the Lochbaum and Beyea reports provide estimates of fission product release fractions that are nothing more than guesses.

Beyea Response 3. ***If the estimates by Lochbaum and Beyea are nothing more than guesses, so too are the estimates made by Boeing and its consultants that insignificant amounts of material escaped. Boeing can't have it both ways. Estimates made by Boeing's experts, Plaintiffs' experts in litigation against Boeing, and the Advisory Panel's experts all depend on unprovable assumptions and professional judgment. Different conclusions can be reached, depending on how an expert weights the conflicting evidence. In particular, Lochbaum provides a logical argument based on the record that supports a large release. It may be correct. On the other hand, one of Boeing's experts, Krsul, provides a very different logical argument to the effect that the release was small, also based on the record. His argument may be the one that is correct. There is no way to tell. The combined likelihood distribution I developed deals with this discrepancy among expert; it captures the state of knowledge we have of the accident. The responsibility for many of the gaps in knowledge fall on the shoulders of Boeing's predecessor, Atomics International. The current owner, Boeing,***

has responsibility for increasing uncertainty, as well, because Boeing continues to withhold information on wind directions during the 1959 accident and refuses to provide modern data on meteorological correlations between key variables at the site. With all due respect, it is astonishing that a Company can withhold data and then defend itself on the grounds that there is not enough information to make a precise estimate.

Mr. Lochbaum reasoned that if 30% of the fuel elements were damaged then an upper bound for fission product release was 30%. He acknowledged that a lower bound would be closer to 0%, so the best estimate or average would be (30% + 0%)/2 = 15%. He did not estimate release in terms of curies. Mr. Lochbaum does not account for the fact that evidence from other sodium cooled reactors has shown that iodine-131 and cesium-137 released from the fuel would be retained in the sodium coolant. Dr. Beyea's estimate was little better, instead relying on the 1957 Windscale release. Dr. Beyea omits the facts that Windscale was a once-though, air-cooled system, and that when the core was burning, there was an open release pathway directly to the environment. Dr. Beyea omits the fact that the SRE fuel was continually immersed in a closed-loop pool of sodium coolant which trapped iodine and cesium, as discussed above.

Beyea Response 4. ***I respond to these claims in my response to the detailed Boeing comments. Suffice it to say, that I dispute each and every statement made above by Boeing about my work. In particular, my release estimate was a likelihood distribution based on all expert opinion I could find, not the Windscale accident. Furthermore, had I relied on Windscale for release estimates, it would have been a reasonable approach, despite the differences in the two accidents. In its logic, Boeing ignores the possibility of boiling of the sodium coolant, which would have allowed a direct gaseous pathway through the coolant. It is not a statement of fact that sodium coolant trapped iodine and cesium. That is a debatable assertion by Boeing and its consultants.***

The Department of Health Services (DHS)[1] made the following statement in 1992 following the second of their cancer registry studies of the communities surrounding SSFL:

> "These analyses suggest that people living near the SSFL are not at increased risk for developing cancers associated with radiation exposure."

An expert panel of nationally-renowned epidemiologists was hired by the Department of Toxic Substances Control (DTSC) to review the three DHS cancer studies[2]. Their conclusion:

> "Three studies of cancer incidence in the vicinity of SSFL were reviewed... the combined evidence from all three does not indicate an increased rate of

California Department of Health Services, Cancer Incidence Near the Santa Susana Field Laboratory (1978-1989), March 27, 1992.

[2] Cal/EPA Department of Toxic Substances Control, Rocketdyne Inquiry - Summary of Findings and Report, August 1999.

cancer in the regions examined. The results do not support the presence of any major environmental hazard"

Beyea Response 5. ***I agree that the individual risk is small at SSFL due to the SRE accident. The social risk, however, is not necessarily small at the higher portion of the distribution of release estimates. The excess cancers I calculate are spread out over a huge population, which has a huge cancer burden independent of anything that could have come out of SSFL. Still, some people would have lost the "cancer lottery," if the higher release estimates were the ones that actually occurred. Those people's burdens should not be trivialized.***

Environmental sampling studies[3,4,5,6,7,8,9,10] performed over the last 14 years have unequivocally demonstrated that cesium-137 is not in the soils of communities surrounding SSFL at levels that differ significantly from local background. These studies demonstrate that cesium-137 releases of the size postulated by Dr. Beyea could not have occurred.

Beyea Response 6. ***This is a false statement. The studies mentioned by Boeing are not particularly informative about elevated radiocesium releases from the SSFL as I discuss in Chapter 2 of my report. The measurements do however severely limit the magnitude of a ground level release. To this extent, I agree with Boeing. However, by ignoring the possibility of elevated releases, Boeing indirectly concedes it has no response to the possibility of a large, elevated release. Boeing argues later that I did not provide sufficient backup for my claims about elevated release. Consequently, in the revised report, I have presented more details.***

Many of these studies have been conducted by organizations independent of Boeing. Those studies conducted by Boeing have been under the oversight of numerous regulatory agencies, including DHS, DTSC, and the Environmental Protection Agency (EPA).

Geologic Features

Groundwater characterization work at the SSFL has been on-going and continues today. The result of this work continues to support that groundwater plumes sourced from the SSFL lie within a few thousand feet of where the contaminants entered the ground because of the attenuating effects of molecular diffusion, sorption,

dispersion, and degradation. This finding is supported by thousands of rock core samples that have been collected to evaluate the occurrence and distribution of trichloroethylene (TCE).

The AP report is very narrow in its focus, but broad in its conclusions. The author(s) opine(s) on contaminant migration at SSFL by focusing on only one aspect of the site, the geology. By ignoring the vast majority of the scientific data that has been collected for the site from multiple scientific perspectives, the report arrives at conclusions that are contrary to the vast quantity of evidence that has been collected over the past 20 years.

Former Sodium Disposal Facility

The stated purpose of the Former Sodium Disposal Facility (FSDF) Interim Measure (IM) was to reduce the potential for soil and sediment containing the Constituents of

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[3] McLaren/Hart, *Additional Soil and Water Sampling at the Brandeis-Bardin Institute and Santa Monica Mountain Conservancy,* January 19, 1995 (http://apps.em.doe.gov/etec/7727 1995 0119 M111 AddSoilandWaterSamp.pdf.pdf).

[4] Environmental Protection Agency, *EPA Update, The U.S. EPA Announces Results of Rocketdvne 's Off-Site Sampling Program for the Santa Susana Field Laboratory,* July 1995 (http://apps.em.doe.gov/etec/EPAFS.PDF).

[5] Lawrence Livermore National Laboratory, *Soil Sampling for Cesium-137 at the Rocketdyne Recreation Center,* 1997.

[6] Ogden Environmental Services, *Bell Canyon Area Soil Sampling Report, Ventura County, California,* October 1998.

[7] Foster Wheeler Environmental Corporation, *Final Report, Runkle Ranch Site Investigation, Simi Valley, CA,* October 1999.

[8] QS1' Environmental, *Results of Preliminary Soil sampling at Runkle Ranch in Simi Valley, California,* February 5, 1999.

[9] Kleinfelder, Report *of Environmental Sampling, Ahmanson Ranch Project, County of Ventura, CA,* January 27, 2000.

[I°] Essentia Management Services, *Final Site Investigation Report – Soil Suitability Evaluation - Chatsworth Reservoir, Chatsworth, California,* Prepared for the Los Angeles Department of Water and Power, July 22, 2004.

Potential Concern from migrating from the FSDF and drainage channels offsite. This purpose has been achieved, and was accomplished through the (1) removal of soil and weathered bedrock at the soil/bedrock contact containing COPCs above the IM cleanup levels, and (2) backfilling, grading, and revegetating the IM remedial area.

Nowhere have SSFL technical reports stated that the IM fill would be impermeable or that no moisture would reach the soil moisture instrumentation. The DTSC approval letter of the infiltration monitoring work plan notes the backfill of the IM to be a "low permeability backfill cover." Finer grained soils (clays and silts) are lower in permeability than coarser grained soils (sands). The soil used was a finer-grained soil consistent with the classification requirements of the IM work plan.

Data shows that the agency-approved, low permeability backfill cover is performing as designed.

Storm Water

For clarification on this issue, storm water runoff from the area does leave the site, but extensive monitoring conducted both on-site and off-site has shown that concentrations in storm flows from the SSFL are typical of or even cleaner than concentrations in storm flows offsite. Monitoring conducted at other undeveloped off-site locations has shown exceedances of many of the same limits that are exceeded at the SSFL. Yet, these sites are nowhere near SSFL and have no history of contamination. In addition, several areas of the site with known perchlorate surface contamination have undergone extensive cleanups. The Happy Valley area is one example. There have been no exceedances of permit limits for perchlorate in storm flows at any site outfalls that leave the facility.

Perchlorate

The analysis contained in the AP report is purely speculative in that it attempts to relate the current occurrences of perchlorate in Simi Valley groundwater to the SSFL. This attempt to link the SSFL to the perchlorate occurrences in Simi Valley without a trail of detections from the SSFL to Simi Valley is purely conjecture and an unproven hypothesis. In fact, extensive data have been collected to evaluate the potential for perchlorate to have been transported from the SSFL into Simi Valley. Various types of samples of environmental media have been collected that include soil matrix, soil leachate, rock chips, bedrock, groundwater, seeps/springs, and surface water. These samples have been analyzed for perchlorate and demonstrate that the SSFL is not responsible for the detections of perchlorate in Simi Valley groundwater.

The Boeing Company appreciates your thorough consideration of all of our comments and looks forward to their incorporation in the final report. Should you

have any questions concerning these comments, please contact Blythe Jameson at 818/466-8793.

Sincerely,

Thomas D. Gallacher
Director
SSFL - Safety, Health & Environmental Affairs

BJ:je

cc:
The Honorable Barbara Boxer, United States Senator
The Honorable Dianne Feinstein, United States Senator
The Honorable Tom McClintock, California State Senator
The Honorable Sheila Kuehl, California State Senator
The Honorable Lloyd Levine, California State Assembly Member
The Honorable Fran Pavley, California State Assembly Member
The Honorable Keith Richman, California State Assembly Member
The Honorable Audra Strickland, California State Assembly Member
The Honorable Michael Antonovich, Los Angeles County Supervisor
The Honorable Greig Smith, Los Angeles City Council Member
The Honorable Judy Mikels, Ventura County Supervisor
The Honorable Linda Parks, Ventura County Supervisor
The Honorable Glen Becerra, Mayor Pro Tem, City of Simi Valley
Ms. Laura Behjan, City of Simi Valley
Mr. Burt Cooper, Agency for Toxic Substances and Disease Registry
Mr. Mike Lopez, Project Manager, Department of Energy
Mr. Gary Butner, Department of Health Services, Radiologic Health Branch
Mr. Watson Gin, Deputy Director, Department of Toxic Substances Control
Mr. Allen Elliott, National Aeronautics and Space Administration Mr.
John Beach, Environmental Protection Agency, Region IX
Mr. Jonathan Bishop, Executive Officer, Los Angeles Regional Water Quality Control Board
Mr. Michael Levy, State Water Resources Control Board
Mr. Michael Villegas, District Executive Officer, Ventura County Air Pollution and Control District
Mr. Brendan Huffman, President, Valley Industry and Commerce Association
Ms. Arlene Levin, Eastern Research Group
Ms. Carol Henderson, Bell Canyon Association
Mr. Gary Brennglass, Executive Director, Brandeis-Bardin Institute
Mr. John Fitzpatrick, Sr. Project Manager, Centex Homes

Ms. Sheila Rozsa, Mountain View Estates
Mr. Randy Wheeler, President, Runkle Ranch
Mr. Luis Porga, Summit Mobile Homes
Ms. Rorie Skei, Santa Monica Mountains Conservancy
Mr. Tim Miller, Sage Ranch

SHEA-104497

Boeing's Comments on the Advisory Panel Reports

"Report of the Santa Susana Field Laboratory Advisory Panel",

"An Assessment of Potential Pathways for Release of Gaseous Radioactivity Following Fuel Damage During Run 14 at the Sodium Reactor Experiment", David A. Lochbaum,

"Feasibility of Developing Exposure Markers for use in Epidemiologic Studies of Radioactive Emissions From the Santa Susana Field Laboratory", Jan Beyea, Ph.D.,

"Geologic Features and their Potential Effects on Contaminant Migration, Santa Susana Field Laboratory", Howard G. Wilshire, Ph.D.,

"An Analysis of the Design and Performance of the Clay Cap Used to Control Groundwater Recharge into the Fractured Bedrock Beneath the Former Sodium Burn Pit (FSDF) at the Boeing-Rocketdyne Santa Susana Field Laboratory", William C. Bianchi, Ph.D.,

"Land-use Conversion and its Potential Impact on Stream/Aquifer Hydraulics and Perchlorate Distribution in Simi Valley", California, M. Ali Tabidian, Ph.D

November 3, 2006

The Boeing Company
Santa Susana Field Laboratory

Contents

BEYEA:

***Beyea Response R0**. The following discussion about release pathways is based upon my understanding that the void above the sodium pool was vented into compressors, whose output was either the stack or storage tanks. The automatic equipment that was supposed to send compressed waste gas with high activity to the storage decay tanks malfunctioned and, prior to the accident, was replaced with a manual unit operated from the control room. Thus, there may have been a fairly direct path to the stack. The assumption that the pathway for escape had to go through the decay tanks is false. The filter in the stack path could be bypassed, so we do not know for sure whether a filter was in place or operational during the event. Boeing does not discuss this filter.*

I further understand that the overheating in the core was associated with the formation of sodium vapor, which blocked cooling. The origin of the initial vapor formation is unclear. It probably formed where fuel overheated, so that fission products would have gone into the vapor either as gases or particulates. If vapor condensed back into the coolant, so too would the radioactivity. On the other hand, if there were sufficient energy in the vapor, it would rise as a bubble. A thermodynamic analysis could be made, perhaps, to set some ranges on the possible lifetime of the bubbles of sodium and radioactive materials, to see if they would reach the top of the coolant. I am not aware of any such analysis. During a bubble's rise. some gas and particulates would condense out, complicating an estimate of how much would have reached the cover gas above the sodium coolant.

The Boeing Company

Santa Susana Field Laboratory

Comments on the Advisory Panel Reports on the 1959 Sodium Reactor Experiment Incident (including the Panel Summary Report, the Lochbaum Report, and the Beyea Report)

No.	Section	Comments
R-1	General **Beyea->**	Each of the three reports (referred to hereafter as the Panel Report, the Lochbaum Report, and the Beyea Report) alleges that the 1959 Sodium Reactor Experiment (SRE) incident released large quantities of iodine-131 and cesium-137, with resulting health impacts on the neighboring communities. This claim is inconsistent with the historical record, and is based on assumptions that are contrary to scientific principles and decades of environmental monitoring. The historical documents[1, 2, 3] and recent reviews[4] of the incident, demonstrate in fact that the SRE incident did not result in the release of iodine-131 or cesium-137 outside the reactor, let alone into the ambient environment. ***Beyea Response, R1a**: The views of Boeing and its consultants, largely developed for litigation purposes, were incorporated into the source term distribution that I developed for estimating health effects. Boeing's experts may well be correct. However, not all experts agree with Boeing. These other experts may also be correct. That is why I also incorporated their views (experts retained by plaintiffs in the litigation and by the SSFL advisory panel). The public and users of my report must understand that it is typical that scientists and engineers disagree. Science is contested territory, which is one of the reasons it is so fascinating. Note that I did not rely solely on Lochbaum's report. However, for radiocesium, which dominates the projected health effects, Lochbaum's report is key to setting the upper range that I project. If Lochbaum's report is removed from the analysis, the range of projected non-thyroid cancers drops by about a factor of ten. However, Boeing has not provided through its experts definitive arguments for excluding Lochbaum's analysis. Moreover, plaintiffs' experts never repudiated their positions, thereby implicitly rejecting Boeing's arguments about the physics and chemistry of the accident. There remain disparate views that must be included in any balanced review of the accident* ***The implication that Lochbaum's and my report were, "based on assumptions that are contrary to scientific principles," is not backed up by analysis, but appears to be related to bald assertions by one expert, Dr. John Frazier, with narrow expertise, largely irrelevant to the issues in debate. (See my response to Frazier (Beyea 2007) and Appendix 6 of my revised report.) I have, however, in the revision to my report, provided more supporting material from the scientific literature on combining the views of experts whose quantitative conclusions differ.*** ***The claim that our reports are refuted by "decades of environmental monitoring," ignores my original report, which distinguishes between elevated and ground level releases. Environmental monitoring cited by Boeing and its consultants was already incorporated into my report, serving to rule out large ground-level releases of radiocesium. It does not rule out large elevated releases. The fact that Boeing fails to mention this distinction indicates, perhaps, that they have no response to the possibility of elevated releases.***

Boeing's recent analysis[4] of the incident concluded that only 28 curies of noble gases (xenon-133 and krypton-85) were released in a controlled manner, in compliance with federal airborne release limits. This release would have resulted in a *maximum* off-site radiation exposure of 0.099 millirem, and an exposure at the location of the nearest resident of 0.018 millirem. To put these doses in context, the average person in the United States receives 360 millirem per year in *background* radiation exposure (most of which is from natural sources). This equates to 1 millirem daily dose received by the average person in the United States every day from background radiation. Thus, the *maximum* off-site radiation exposure from the SRE incident of 0.099 millirem was 10 times lower than the average person's daily exposure to background radiation.

These estimated maximum doses from the SRE incident are also low compared to the protective annual dose limit set by the Nuclear Regulatory Commission (NRC) and Department of Energy for unrestricted areas surrounding nuclear facilities (100 millirem/year) as well as the EPA limit for airborne releases (10 millirem/year).

In 2005, two independent studies were completed that confirmed Boeing's earlier findings that only small quantities of noble gases were released following the accident and that no iodine-131 or cesium-137 was released.

- "Chemical Behavior of Iodine-131 During the SRE Fuel Element Damage in July 1959. Response to Plaintiff's Expert Witness, Arjun Makhijani", Jerry D. Christian Ph.D., May 26, 2005
- "Investigation of Releases from Santa Susana Sodium Reactor Experiment in 1959", John A. Daniel Sr., May 27, 2005

	Beyea->	Dr. Jerry Christian is a past Scientific Fellow from the Idaho National Engineering and Environmental Laboratory (INEEL) and is an expert in nuclear fuel chemistry and the behavior of fission products in nuclear fuel. John Daniel participated in the decontamination and recovery of the Three Mile Island (TMI) nuclear plant. He is an expert on nuclear power plant safety analysis and fission product transport and behavior. ***<u>Beyea Response, R1b</u>: The experts mentioned by Boeing prepared reports for litigation. Their results were included in my likelihood distribution for curies released. There is, indeed, some likelihood that the dose estimates given by Boeing above are correct. However, I can't just take one side of the litigation into consideration. Nevertheless, for the revision to my report, I added Boeing's latest expert, John Krsul, to the set of experts used to develop likelihood distributions.*** ***I do take responsibility for the failure of the press to report the lower limit of zero cancers that I calculated. That was unfair to Boeing and its experts. It may also have caused undue concern among some residents. I should have been more forceful in my report in pointing out that a zero release was just as likely as a high release. I never expected such attention by non-experts.*** ***Of equal concern to me about the press reporting was the statement in one article that the estimated cancers were confined to a 60-square mile area. In fact, the 60-miles referred to distance from the plant not area. A 60-mile circle has an area of some 10,000 sq miles, not 60 square miles. As a result, I worry that some local residents were unduly alarmed. Ironically, the fact that any large radiocesium release would have had to be elevated means that local residents would have largely been spared exposure due to the "waterfall effect." Only about 10% of the figures I quoted were associated with distances less than 4 miles. The Advisory Panel's report had appropriate caveats about where the excess cancers were located, if they occurred, but the caveats did not get picked up in every press story.***

No.	Section	Comments
		The principal conclusions of these two independent studies were: • Only very limited melting of an iron-uranium eutectic (alloy) occurred, causing failure of the steel cladding. • Nearly all of the iodine-131 in the reactor stayed in the fuel as uranium tri-iodide, a solid. No elemental iodine-131 vapor was released. • Approximately 1 % of the iodine-131 (16 curies) was released from the fuel into the sodium coolant in the reactor core. It then formed sodium iodide, a solid, and stayed in the reactor coolant system. • Approximately 1 % of cesium-137 (28 curies) was released from the fuel into the sodium coolant in the reactor core, and all of this cesium-137 stayed in the reactor coolant system. • Measurements of the reactor cover gas indicated only noble gases (xenon-133 and krypton-85) were present. No iodine-131 or cesium-137 was detected in the cover gas, which is contrary to the alleged pathway for release through the stack, as theorized by the Lochbaum Report. • Only very limited quantities of noble gases (xenon-133 and krypton-85) were released to the environment from the stack. Several quotes from the historical record reinforce these conclusions: "Even though iodine is very volatile, it did not escape to the cover gas because it undoubtedly combined with the sodium as rapidly as it was evolved. No iodine was ever detected in reactor cover gas samples," (NAA-SR-4488(1), page IV-C-5). "Only Xe and Kr isotopes were identified in the reactor cover gas system. This confirms the previously held premise that the sodium coolant forms an effective trapping agent for all but rare gas [noble gas] isotopes," (NAA-SR-6890(3), page 23). "Examination of the recovered fuel slugs from damaged [fuel] elements showed no evidence of significant melting," (NAA-SR-6890(3), page 21). "With the exception of inert gases, Xe-133 and Kr-85, all of the fission fragments remained in the sodium ... " (NAA-SR-4488-Suppl(2), page III-20). (1) NAA-SR-4488, “SRE Fuel Element Damage – Interim Report”, A. A. Jarrett (Editor), November 15, 1959 (2) NAA-SR-4488 (Suppl.), “SRE Fuel Element Damage – Final Report”, 1961 (3) NAA-SR-6890, “Distribution of Fission Product Contamination in the SRE”, R. S. Hart, March 1, 1962 (4) Letter to Elizabeth Crawford from Phil Rutherford, “Sodium Reactor Experiment (SRE) Original Release Data”, January 21, 2005

No.	Section	Comments
R-2	General ***Beyea->***	Several key quotations from the "Introduction and Overview" section of Dr. Christian's report(1) follow, • "The increased temperature with uranium fuel slugs in contact with the stainless steel cladding caused rapid diffusion of uranium into and alloying with the stainless steel. At locations where the temperature exceeded the meslting point of the iron-uranium eutectic, 1337 °F (725 °C), this diffusion resulted in the formation of an alloy with some liquid phase present. This alloying ultimately resulted in failure of the cladding of some of the fuel elements, though the fuel did not melt ... The melting temperature of uranium, 2075°F (1135 °C), was not reached." ***Beyea response, R2:*** ***The absolute certainty with which this expert speaks in the passages quoted above and below is not credible. Reconstruction of any accident, let alone one that took place so many years ago, is uncertain. For instance, the fact that some of the fuel was found after the accident to be in a eutectic mixture does not mean all of it was. (See subsequent discussion for more details.) Experts are known from post-prediction analysis to be overconfident in excluding the full range of possibilities (Cooke 1991). Methods of combining expert opinion, such as those I use in my report, were designed to handle this tendency.*** • "As explained in the text below, the incident did not result in significant release of any fission products, including gases, from the failed-cladding fuels. Of the small quantities released from the fuel, most, including all of the released iodine, were trapped in the sodium. Only small fractions of xenon and krypton escaped from the fuel and through the sodium into the cover gas. Xenon and krypton are not soluble in or chemically reactive with sodium. About 1 % or less of failed element fission product inventory of non-volatiles, including iodine as a salt, was found in the sodium. No iodine-131 was found in the cover gas." • "The conclusion from all these considerations is that fission product iodine formed uranium tri-iodide and/or cesium iodide in the metallic fuel and was not released from the fuel as a gas. Based on considerations of the chemistry of iodine in the fuel that would make it behave similarly to other non-volatile fission products, on I-131 measured in the sodium, and on the lack of I-131 in the cover gas, only between 0.3 and 1.3 percent (depending on the assumed date of release) of the iodine-131 was released from the failed fuel elements. Of that released, all was captured and retained in the sodium coolant. No iodine was released to the stack. Details of the analyses are provided in the report." (1) "Chemical Behavior of Iodine-131 During the SRE Fuel Element Damage in July 1959. Response to Plaintiff's Expert Witness, Arjun Makhijani", Jerry D. Christian Ph.D., May 26, 2005
R-3	General	**Industry Experience for the Retention of Iodine-131 and Cesium-137 in Sodium** Evidence from the SRE post-accident measurements of sodium and cover gas indicates that no iodine-131 and cesium-137 escaped from the sodium into the cover gas, and therefore neither of these fission products was released to the environment. This evidence is supported by a large amount of operational history and research into fission product behavior in sodium coolant.

The International Atomic Energy Agency (IAEA)[1] stated in 1973,

> "Because of its chemical nature, iodine has a very high affinity for sodium. Thus it would be expected that essentially all of the iodine entering the primary coolant would immediately react with the sodium and be retained in the primary system.

Beyea->

Beyea Response, R3a: ***But what If all the iodine did not enter the coolant? If all the sodium coolant was in contact with the fuel, why was there an overheating problem in the first place? Only because of a flow restriction in the coolant that never led to boiling? Or, did boiling occur, allowing cesium and iodine vapor (or particulates) to rise through the coolant into the cover gas? (Later on in this section, Boeing concedes boiling is a possibility.) How violent was the overheating? That seems to be a key question. If you start from the premise that the overheating was modest, the Boeing scenario appears more reasonable. However, starting from that premise amounts to assuming the answer rather than deducing it.***

If the iodine and cesium went directly into the coolant there to remain, then the fraction of fuel iodine found in the coolant would be expected to be much higher than the fraction of fuel cesium, because iodine can escape easier from the metal fuel in the first place. Also, cesium is hundreds of time more volatile in sodium than is iodine (Clough and Wade 1970). Yet, the reverse situation occurred. A greater release fraction of cesium was found in the coolant than of iodine (Lochbaum 2006) – a result that no one has so far explained, indicating that the "official" model of the accident is incomplete. It is certainly hard to see how the iodine and the cesium entered the fuel directly, given the numerical results. In fact, Boeing's theory of the accident appears to be falsified by the data.

On the other hand, it is not clear how this cesium/iodine reversal could happen in a bubbling scenario either, but there is more room for surprises in such a scenario or any scenario where a large fraction of the material emitted from the fuel escapes the coolant and the reactor. For instance, perhaps the cesium vapor, unlike the iodine vapor, condensed on surfaces above the sodium, only to be washed back into coolant by the boiling liquid. A greater percentage of iodine vapor could then have escaped.

Until a convincing explanation for the cesium/iodine ratio in the coolant surfaces, the physics and chemistry of the accident is up for grabs, adding considerable uncertainty to the release magnitude.

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		breeder reactors] indicates that this is in fact the case." [p. 19] Castleman[29] observed in 1970 that, "The results of BR-5, SRE, Fermi and EBR-II incidents showed that most of the iodine released from the fuel is retained in the liquid sodium." [p. 381] The Fermi-I fuel element damage incident[3, 4] provides evidence that both iodine-131 and cesium-137 that may be released from fuel is retained in the sodium. The Enrico Fermi reactor had a sodium-cooled metal core like the SRE. On October 5, 1966, a broken off piece of zirconium baffle from the inlet area at the bottom of the core vessel resulted in flow restriction of the sodium and melting of one or more fuel elements [4, pp. 31-37]. This was a more severe condition than during the SRE incident in terms of higher fuel temperature, actual melting of the fuel, and severe boiling of the sodium around the failed fuel, all of which would have been more conducive to iodine releases from the fuel and through the sodium than in SRE. Qualitative and quantitative measurements of the fission products contained in the primary sodium coolant and the primary argon cover gas were made periodically after the Fermi incident. The only radioisotopes reported were xenon and krypton, both of which were used to estimate the amount of fuel damage [3, p. 80]. Iodine-131 was not reported as having been observed in the cover gas. Analysis of the sodium in Fermi showed the presence of cesium-137, iodine-131, and other radioisotopes. The percents of fuel inventory of Cs-137 and I-131 found in the Fermi sodium were identical, and similar to what was observed in SRE [3, Table VII, p. 82]. The conclusion was that about 1 or 2 percent of the available nonvolatile solid gamma-emitting fission products were released during melting in the Fermi incident. This is similar to the fractions of failed fuel inventories, including I-131, found in the SRE sodium. The cesium remains in the sodium because it is released from the metal fuel as elemental cesium metal or, possibly some as cesium iodide, CsI. When present at very low concentrations in excess sodium, thermodynamic calculations show that CsI will readily react with sodium to form sodium iodide, NaI and elemental cesium. This is borne out by experiments by Castleman, Tang, and Mackay. [2, p. 382; 401]. Sodium iodide is soluble in sodium and retained in solution at low concentrations [2, p. 382; 411]. Similar thermodynamic considerations show that uranium iodide in sodium converts to uranium and NaI. Cesium is very soluble in its sister alkali metal sodium.

		These observations from the Fermi fuel melting incident are consistent with observations of the SRE incident that show that no I-131 reached the cover gas and, just as significantly, the amount of I-131 captured by the sodium was similar to cesium, only 1 to 5 percent. The fact that only a fairly small fraction of iodine was found in the sodium and none in the cover gas demonstrates that very little iodine was released from the metal uranium fuel.
	Beyea->	***Beyea Response, R3b**: **The conclusion that very little iodine was released from the fuel is based on circular reasoning. Missing from this mass-balance argument is the possibility that a large amount of iodine escaped from the cover gas and the reactor itself. Furthermore, to say that the amount of Iodine captured was similar to cesium is to say 1% is similar to 5%, when a reverse ratio would have been expected. A rather weak effort to gloss over a contradictory fact.***
		The conditions in the Fermi incident would have been more conducive to iodine release from the fuel and, also, to bubbling through sodium into the cover gas than in the SRE. Iodine-131 was not found in the Fermi reactor cover gas nor substantially in the sodium.
	Beyea->	***Beyea response, R3c. Note that Boeing concedes in the above passage that bubbling is a possibility and, by implication, a concern. This is the only public mention of sodium vapor by Boeing or its consultants. Too damaging to their case to discuss, or just an oversight?***

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		(1) IAEA, "Control of Iodine in the Nuclear Industry", International Atomic Energy Agency, Technical Reports Series No. 148, June 1973. (2) Castleman A. W., "LMFBR Safety I – Fission Product Behavior in Sodium", Nuclear Safety , Vol. 11, No. 5, Sept. – Oct. 1970 (3) "October 5, 1966 Fuel Damage Incident at the Enrico Fermi Atomic Power Plant - Status as of February 24, 1967," NP- 16750 (1967). (4) "Report on the Fuel Melting Incident in the Enrico Fermi Atomic Power Plant on October 5, 1966," APDA-233 (December 15, 1968).
R-4	General	The Advisory Panel (AP) reports fail to acknowledge numerous conclusions that State and Federal agencies have made concerning SSFL and the surrounding communities. These are enumerated below. Taken as a whole, these statements by State and Federal agencies confirm that no environmental health hazard, or any elevated cancer rates, has been observed as a result of the activities at the Santa Susana Field Laboratory.
R-5	General **Beyea->**	The Agency for Toxic Substances and Disease Registry (ATSDR), in their 1999 study (http://www.atsdr.cdc.gov/HAC/PHA/santa/san toc.html), concluded: • "ATSDR has not identified an apparent public health hazard to the surrounding communities because people have not been, and are currently not being exposed to chemicals and radionuclides from the site at levels that are likely to result in adverse health effects." ***Beyea Response, R5*** ***As I stated in my report, ATSDR accepted on faith the analysis in the 1960s of the SRE event presented by employees of Atomics International, whose company's future was on the line. You just can't do that in forensic investigations.***
R-6	General **Beyea->**	The Environmental Protection Agency's (EPA) conclusion following the 1995 Off-Site Multimedia Study of the Brandeis-Bardin Institute and Santa Monica Mountains Conservancy (http://apps.em.doe.gov/etec/EPAFS.PDF) immediately to the north of the location of the SRE, was that, • "The radionuclides do not pose a threat to human health and the environment." ***Beyea Response, R7.*** ***I never said these levels at this location were a threat to human health and the environment. I never looked at this specific set of properties. My focus was on risks from 1959 through 1989, not post-1995 risks.***
R-7	General	The conclusion of EPA's 2003 Hazard Ranking Assessment of Area IV of SSFL (http://apps.em.doe.gov/etec/EPA-HRS.pdf) stated that ...

	Beyea->	• *"Radionuclides associated with historic Area IV research are not present at concentrations significantly above background in the soils surrounding residential communities."* ***Beyea Response, R7**. My concern about soil concentrations being elevated had to do with soil at distances further from the facility that the historic Area IV research. Elevated releases for most weather conditions would only have caused excess soil concentrations outside the Area IV boundary. Also, in contrast to cesium, releases of radioiodine would not have left a signal easily detected. Finally, levels of SSFL radionuclide exposure comparable to or even much less than background would have produced excess cancers, if the area of exposure was great enough, because even background radiation is projected to contribute to cancer rates. Although the increase in individual risk would be small from SSFL, the social responsibility would not necessarily be small. For instance, it might be appropriate for the responsible parties to contribute money to cancer research, thereby providing some potential relief to anyone exposed and providing partial compensation for the accident.* ***The fact that Boeing will not release meteorological data, either for the time of the accident or for later periods when correlations between wind speed, angle, and atmospheric stability class exist, raises the possibility that calculations using such data could leave room for significant individual risk. On the other hand, Boeing may just be responding to the professional paranoia of its legal advisors, hoping to raise the "due diligence" requirement for any attorneys and potential plaintiffs contemplating new litigation. (Once a case is filed, the meteorological data would be accessible though discovery.) As someone who regularly advises attorneys on the scientific merits of contemplated plaintiffs' litigation, I would recommend against using my report as a justification for believing that individual causation cases could prevail in a California court room. My report wasn't designed for that purpose. It was designed to advise epidemiologists on possible studies.*** ***The decision to participate in complex tort litigation should not be undertaken lightly by potential plaintiffs. It can take decades and I have seen it mess up people's lives. You really want to be sure you have a solid individual exposure case, before you jump into trying to prove individual causation. Thus, I became concerned when I read in one press report that some local citizens were rushing off to meet with attorneys upon reading about the Advisory Panel's report. Individual litigation might be justified, but not based on my report. The health effects I projected were either zero or they were spread out over a huge population. Only about 10% were projected to lie within 4 miles. The recent finding of an excess of thyroid cancer around the SSFL (Morgenstern et al. 2007) is more relevant to litigation decisions than my report.*** ***Class action suits are more appropriate scientifically for dealing with this kind of uncertain event. They are also less stressful on plaintiffs in my experience. However, class action suits have become much more difficult to get certified, I am told.***

R-8	General	The Department of Health Services made the following statement in 1992 following the second of their cancer registry studies of the communities surrounding SSFL, “These analyses suggest that people living near the SSFL are not at increased risk for developing cancers associated with radiation exposure.”

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		"Cancer Incidence Near the Santa Susana Field Laboratory (1978-1989)", California Department of Health Services, March 27, 1992. An expert panel of nationally-renowned epidemiologists was hired by the Department of Toxic Substances Control (DTSC) to review the three DHS cancer studies. Their conclusion was, "Three studies of cancer incidence in the vicinity of SSFL were reviewed..... the combined evidence from all three does not indicate an increased rate of cancer in the regions examined. The results do not support the presence of any major environmental hazard." "Rocketdyne Inquiry – Summary of Findings and Report", Cal/EPA Department of Toxic Substances Control", August 1999.
R-9	General	The EPA's conclusions following its own 2000-2001 surveys of 11 prior radiological facilities stated ... • "Previous DOE/Boeing surveys sampled in appropriate and representative locations." • "Measurements made in previous surveys were accurate." • "EPA concurs with the conclusions made by the Department of Energy (DOE) and Boeing Rocketdyne about the locations and levels of residual radioactivity." "Residual radioactivity does not exceed DOE and Nuclear Regulatory (NRC) established limits for unrestricted use."
R-10	General	The AP reports purport to be "independent" reviews of the SRE incident, but this is not a fair or accurate characterization. Dan Hirsch, the AP co-chair, is president of the Committee to Bridge the Gap (www.committeetobridgethegap.org), a group which has long opposed regulated activities at the SSFL. In fact, Mr. Hirsch's organization presently is suing The Boeing Company in federal court regarding activities at the SSFL, seeking declaratory and injunctive relief, imposition of civil penalties, and costs and attorneys fees. The AP turned to the Union of Concerned Scientists (www.ucsusa.org), an organization which opposes nuclear power, for the preparation of the Lochbaum Report. Jan Beyea is with an organization called

	Beyea->	Consulting in the Public Interest (www.cipi.com), whose web site advertises its services to "plaintiff's attorneys." ***Beyea response, R10: Because it is impossible to eliminate biases among experts, it is important to have a diverse group of analysts review an accident like SRE. I do not pretend to have all the answers. I don't think Boeing has them either; nor the other experts retained by the Advisory Panel. In my view, the truest picture will emerge, if we use a methodology that takes into account the full range of views. In my revised report and in my annotations of Dr. Frazier's report, I demonstrate that the combinatorial method I use is standard in the risk assessment field. The burden on the press, the public, and public officials with such an approach is to embrace the uncertainty and not focus only on the lower or upper limits.*** ***As for my consulting rules: I do not consult for corporations like Boeing or defend them in litigation, because I do not want, when the inevitable secrets from the past pop up, to end up having to defend the indefensible. Attitudes towards radiation were pretty cavalier in the past, particularly before the mid-1970s. In our legal system, Boeing is entitled to have good consultants defend their interests and Boeing can afford to find them. In my case, I have these romantic notions of defending the underdog.***
R-11	Beyea->	Some reports in the news media have stated that the AP studies used computer modeling to calculate how much radioactivity was released from the SRE accident. This is not correct. Neither Mr. Lochbaum nor Dr. Beyea used computer modeling to derive their estimates of radiation releases. Rather, Mr. Lochbaum simply chose the half-way point between 0 and 30 percent, and chose this percentage as his "release fraction" for the incident (which is then used by Dr. Beyea to derive his estimate regarding SRE releases). ***Beyea response, R11: This claim that computer modeling was not used to estimate release estimates is misleading. Lochbaum's report is but one of many reports, including those from experts retained by Boeing for litigation, that I used in my analysis. I also discount somewhat the upper range of Lochbaum's and other's radiocesium release estimates to partly account for soil measurements. Lochbaum's report is dominant in determining the upper limit, however, so focus on his report is appropriate. As for computer modeling, it is indeed used in my report, namely to combine the different release estimates and to estimate the projected range of health effects. In any case, the mere use of computer modeling by itself should not impress anyone. Complex models without validation are not necessarily any more useful than informed expert judgment. I try to make that point in my report. The most important underpinning of my report is not the computer modeling, but the historical and meteorological reviews that I undertook, as well as the combining of expert assessments.***

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R-12	Panel Report, Pages 5 and 6	Reference is made to the 1997 and 1999 UCLA reports of Rocketdyne radiation workers and rocket test stand workers. No reference is made, however, to the more extensive report, sponsored by Boeing and the United Aerospace Workers (UAW), that was released in April 2005. The Boeing/UAW study was performed by experienced radiation epidemiologists. The principal investigator was Dr. John Boice of the International Epidemiology Institute (IEI), and the study was overseen by a Science Committee of epidemiology and public health experts headed by Dr. John Peters of the University of Southern California. The IEI Research team found no consistent or credible evidence that employment at Rocketdyne had adversely affected worker mortality. The Science Committee likewise concluded that, based on the results of the study, • The Rocketdyne workforce had a much lower overall mortality than the rate observed in the California population • There is no evidence that working conditions caused increased mortality in the Rocketdyne workforce The report can be found at: http://www.boeing.com/aboutus/environment/santa susana/healthstudy.html.

R-13	Panel Report, Page 10	Reference is made in the Panel Report to several reactor incidents at SSFL. The incidents are discussed below. **AE6** In March 25, 1959, a release of fission gas within the AE-6 reactor occurred when an operational error was made during the transfer of gases from the reactor core to the holdup tank. This resulted in the release of a small amount of fission products into the reactor room and in the contamination of three members on the operating staff. The contamination was cleaned up quickly and effectively, and there were no measurable radiation exposures to any of the personnel involved. Calculations based on the operation of the reactor prior to the incident show that maximum release of fission gas in the reactor room would have been less than approximately 10 millicuries, principally Xe-135, and the building volume was sufficient to dilute the activity to a concentration essentially equal to the occupationally permitted concentration for continuous 40-hour/week exposure. There was no indication of any release to the environment. NAA-SR-MEMO 3757, “Release of Fission Gases from the AE-6 Reactor.”

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		S8ER (1964) During the operating life of the reactor core, 80% of the fuel swelled and the cladding developed cracks. This resulted in a slow escape of fission products into the coolant. All radioactivity was retained in the coolant system and cleaned up with the normal coolant cleaning systems. This was not an accident (or even incident), but was reported in the literature on sodium-cooled fuel rod operating experience. No release to the environment occurred. AI-AEC-MEMO-12790, "Survey of Fission and Corrosion Product Activity in Sodium or NaK Cooled Reactors", February 28, 1969. AI-AEC-13070. "SNAP 8 Summary Report." September 24, 1973. **S8DR (1969)** As with the above discussion on S8ER, similar fuel rod failures (e.g. clad swelling and cracking) occurred in the S8DR reactor. This was not an accident, and did not result in any release of radioactivity from the NaK coolant. No release to the environment occurred. Letter from M. Klein (USAEC) to J. J. Flaherty (AI), Untitled, 3206AT, October 29, 1969. AI-AEC-13071. "Summary of SNAP 8 Developmental Reactor (S8DR) Operations." June 22, 1973. AI-AEC-13070. "SNAP 8 Summary Report." September 24, 1973.
R-14	Panel Report, Page 10	Reference is made to plutonium allegedly detected in offsite soils immediately to the north of the SSFL. One location just to the north of the SSFL boundary had detectable, but low levels of plutonium-238 during the 1992 Brandeis Bardeen Institute/Santa Monica Mountains Conservancy sampling project. Subsequent sampling in the same location, however, failed to confirm any detectable plutonium-238. The land is now owned by Boeing. See report at ... http://apps.em.doe.gov/etec/7727 1995 0119 MHI AddSoilandWaterSamp.pdf.pdf
R-15	Panel Report, Pages 13	Reference is made in the Panel Report to the 1989 Dempsey review of the SSFL radiological monitoring program.

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	and 14	It is Boeing's view that the criticisms of the program in the Dempsey review were addressed, corrected, or answered in a Rockwell report, N001SRR140115, "Recent Reviews of Rocketdyne Environmental Monitoring Program," June 28, 1991. This document also contains two additional independent reviews of the program which in general respond to the Dempsey criticisms. ATSDR and UCLA were provided with a copy of this report for their studies.
R-16	Panel Report, Page 14	Reference is made to the filtering of water samples. The following addresses the issue of filtered vs. unfiltered water. Water with low turbidity (low suspended solids) has been shown to have no statistical difference between filtered and unfiltered samples (EPA groundwater study[1], DHS groundwater stud y[2], and Boeing surface water studies). Water with high turbidity (high suspended solids, muddy water) does result in significant differences in gross alpha activity (Boeing groundwater stud y[3]). However, subsequent uranium isotopic analysis has demonstrated that the uranium content of the suspended solids accounts for the difference. When uranium is subtracted from the gross alpha (as EPA protocols require) then alpha maximum contaminant levels (MCLs) are met. Inspection of the uranium isotopic ratios also demonstrates that the uranium is naturally occurring and not enriched or processed. 1. "Rocketdyne Technical Support/Field Oversight - Groundwater Split Sampling Report," prepared by Tetra Tech for EPA, Region 9, June 23, 1998. 2. "Ahmanson Ranch Groundwater Sampling of June 2003," Department of Health Services Radiologic Health Branch. 3. "SSFL Groundwater Monitoring Report for SSFL – Second Quarter 2006," Hailey & Aldrich, September 2006.
R-17	Panel Report, Page 17	The Panel Report reiterates Mr. Lochbaum's claim of large fractions of the reactor's fission product inventory being released. Mr. Lochbaum starts with the observation that 13 of 43 (or approximately 30%) of the fuel elements were damaged. He assumes that all of the parts of these 13 fuel elements were damaged and/or melted (although this assumption is not supported), and he therefore assumes that 30% of the core's fission product inventory was released to the environment. This is his "upper bound" estimate. He then acknowledges that a large fraction of the fission products would have been retained in the reactor system by a variety of means. He derives an unsupported estimate that the fraction of radioactivity released from the fuel into the sodium coolant would be 10%, and he then assumes that the release fraction from the cover gas to the environment would be 10% (for cesium-137) and 100% for iodine-131. Thus, the lower bound release therefore appears to be 0.3 x 0.1 x 0.1 = .003 = 0.3% for cesium-137 and 0.3 x 0.1 x 1.0 = .03 = 3% for iodine-131. He then says that the best estimate release would be the average of the upper and lower bound, o r ~ 15%. Mr. Lochbaum's release fractions do not account for the fact that any iodine-131 or

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		cesium-137 released from the fuel would have been retained by the sodium coolant. Because the sodium coolant would bind up any iodine-131 and cesium-137 upon contact, this coolant acted as a six-foot deep protective barrier between the reactor core and cover gas. Yet Mr. Lochbaum assumes that 100% of these fission products would have somehow migrated up through this pool of sodium above the core without coming into any contact with it.
R-18	Panel Report, Page 18 **Beyea->**	Mr. Lochbaum does not provide an estimate of iodine-131 or cesium-137 inventory, or an estimate of number of curies released. The Panel Report uses Mr. Lochbaum's release fraction estimates to imply the upper bound number of curies released was 13,000 curies of iodine-131 and 2,600 curies of cesium-137, and a best estimate release of 6,500 curies of iodine-131 and 1,300 curies of cesium-137. This would require a total core inventory of iodine-131 and cesium-137 to be ~43,000 curies and 8,700 curies respectively. The Panel states that these inventories are based on Atomics International data, but that is not entirely correct. Table IV of NAA-SR-6890[1] gives the iodine-131 and cesium-137 core inventories as 16,800 curies and 8,700 curies respectively. Thus the Panel has used the correct 1962 estimate for cesium-137, but has used a value for the iodine-131 that is a factor of 2.6 too large. Hence, the Panel's estimates of iodine-131 releases are too large by a factor of 2.6 even if Mr. Lochbaum's release fractions are correct, which they are not. ***Beyea response, R18:*** ***The extra radioiodine comes from left over iodine from previous runs, on the assumption that the fuel was not replaced. Furthermore, as I discuss in my report, the assumed, very low burn-up factor used to calculate the radioiodine inventory is not credible given the several weeks that the operators were trying to get the reactor to work right. Thus, I believe that the radioiodine inventory used by Lochbaum and the other analysts has a good chance of being way too low, which is why I allowed in my report for the possibility of higher burn-ups in the likelihood distribution.*** (1) NAA-SR-6890, "Distribution of Fission Product Contamination in the SRE", R.S. Hart, March 1, 1962.

R-19	Panel Report, Page 18 and 19	The Panel report compares its estimated release of iodine-131 with that of TMI (which released 17 curies of iodine-131). The implication is that the SRE was worse than TMI. The Panel Report claims that 6,500 curies of iodine-131 and 1,300 curies of cesium-137 were released following the SRE accident. Dr. Beyea offers yet another set of estimated releases of between 1,500 and 4,000 curies of iodine-131 and about 400 curies of cesium-137. **Beyea->** ***Beyea response, R19:*** ***The "other" release estimate for radioiodine mentioned by Boeing that I incorporated in my likelihood distribution comes from plaintiffs' experts in the SSFL litigation. The 400-curies of radiocesium mentioned by Boeing was the mean estimate of all the expert radiocesium opinions, which basically amounts to a reduction of the Lochbaum estimate. Note that the cesium numbers have dropped in the revised report.*** Both sets of estimates are incorrect. Boeing's documented measured release data shows that a total of 28 curies of noble gases (9 curies of krypton-85 and 19 curies of xenon-133) were released following the SRE accident. The reported iodine-131 release from TMI(1) was 17 curies and the reported noble gas release was 2.4 million to 13 million curies. Therefore, TMI was actually at least 86,000 to 460,000 times worse than the SRE release(2). The expected number of total additional cancer deaths from TMI was calculated to be 0.7 in a population of 2,000,000 living within 50 miles. This means that possibly zero, and most likely one person, would be expected to die of cancer from TMI.

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		Considering the much smaller SRE release of 28 curies of noble gases, there is no evidence that the SRE incident could have had any impact on community incidence of cancer. (1) All TMI data has been taken from the President's Commission on TMI. http://www.pddoc.com/tmi2/kemeny/index.html.
R-20	Panel Report, Page 19 and 20 **Beyea->**	The Panel argues that the risk from radiation exposure is an order of magnitude higher than that assumed by regulatory agencies. If the Panel's assertion were correct, the cancer risk from background radiation would be approximately 50% of the total cancer risk in society. ***<u>Beyea response, R20a</u>: Boeing is wrong on this point. I discuss the issue of background radiation and cancer risk coefficients in my report. The highest risk coefficient I use in calculating excess cancers is a factor of three times the value adopted by the BEIR VII committee, not an order of magnitude. My number is lower than stated in the Panel's introduction, because I combine the new results with the old. As for the impact on cancers from background radiation, I calculate that a 3-fold increase implies that background radiation would increase non-lung cancer mortality by 3 to 5%. The corresponding increase in non-lung cancer incidence should be comparable. Estimating the increase in lung cancer is complicated by the uncertainty in the radiation-smoking interaction. Had I used a factor of three times higher, bringing the increase up to an order of magnitude, I would have estimated a 9 to 15% increase in non-lung cancer rates, so I do not understand the origin of Boeing's 50% increase.*** ***There is another problem with the above language about the risk from background radiation. In epidemiology, it is possible to have multiple causes, which, if treated separately can appear to make the total risk appear to be greater than 100%. For an analogy, consider two partners in theft, John and Jim, who jointly pull off a string of robberies. An epidemiological study that looked at the correlation of each of them with the robberies would find them each 100% associated with the thefts. The total risk of robbery could then be (wrongly) totaled to 200%. In fact, the two thieves jointly contributed to the robberies and we would hold them both responsible for the thefts.***

The BEIR VII committee (http://www.nap.edu/books/030909156X/html) confirmed the position of the earlier BEIR V committee that the linear no threshold (LNT) model of radiation risk is appropriate and that there is no threshold.

- BEIR VII defines low doses of ionizing radiation as less than 100 mSv (10,000 mrem).
- BEIR VII states that *"at doses of 100 mSv (10,000 mrem) or less, statistical limitations make it difficult to evaluate cancer risk in humans."*
- BEIR VII states that *"at low doses the number of radiation induced cancers is small."*
- BEIR VII states that *"approximately one individual in 100 persons would be expected to develop cancer (solid cancer or leukemia) from a dose of 100 mSv (10,000 mrem) while approximately 42 of the 100 individuals would be expected to develop solid cancer or leukemia from other causes."*
- BEIR VII establishes fatal cancer risk of ~0.0056 per 100 mSv (10,000 mrem) for solid cancers and leukemia (average of male and female risks). This is almost identical to the fatal cancer risk from ICRP 60 (1990) of 0.005 per 100 mSv derived from BEIR V.

Nonetheless, it is important to understand that the LNT model is a hypothetical statistical model, and that its use at low dose rates is extremely conservative.

Beyea response, R20b: I don't think it is defensible any more to say the LNT is conservative at low doses. In fact, the Techa River cohort and the Cardis study show higher relative risks at low doses than the relative risks measured in the Atomic Bomb survivors, who had an average dose ten-times higher.

There is little or no scientific evidence that small variations in radiation exposure, much less than the variability in natural background radiation levels, result in any real or measurable increase in cancer risks. The following scientific, professional, and governmental bodies support the concept of a threshold at about 5,000 to 10,000 millirem above background, below which there is no cancer risk from radiation exposure. ***Misleading. Misleading. Misleading. These organizations do not support a threshold. This language By Boeing is fringe science, not worthy of Boeing.***

- The **National Academy of Sciences** states, "With few exceptions, however, [cancer] effects have been observed only at relatively high doses and high dose rates. Studies of populations, chronically exposed to low level radiation, such as those residing in regions of elevated natural Background radiation [10 - 100 times average US levels], have not shown

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	Beyea->	consistent or conclusive evidence of an associated increase in the risk of cancer." Health Effects of Low Levels of Ionizing Radiation. Committee on the Biological Effects of Ionizing Radiation (BEIR V), page 5. National Academy of Sciences, 1990. (http://newton.nap.edu/books/0309039959/html/5.html#pagetop) ***Beyea response, 20c:*** ***It is a total misrepresentation to say that the BEIR Committees' reports support a threshold. The latest BEIR report says just the opposite. No reason to expect a threshold. Furthermore, the quote from the 1990 BEIR V report on background radiation is selective and only gives one part of the puzzle they take into account. As I discuss in my report, these background studies would not be expected to show a statistically significant effect.***
	Beyea->	• The **Health Physics Society** states, "The Health Physics Society recommends against quantitative estimation of health risk below an individual dose of 5,000 millirem in one year or a lifetime dose of 10,000 millirem in addition to background radiation. There is substantial and convincing evidence of health risks at high dose. Below 10,000 millirem (which include occupational and environmental exposures), risks of health effects are either too small to be observed or are non-existent." Health Physics Society Position Statement on "Radiation Risk in Perspective." March 2001. (http://www.hps.org/documents/radiationrisk.pdf) ***Beyea response, 20d:*** ***This statement does not support a threshold, as Boeing claims. Furthermore, the Health Physics Society is not a scientific body. The science it cites is way out of date; its policy recommendation is harmful, even to Boeing's interests. The statement about risks below 10,000 millirem is falsified by the latest study on the Techa Cohort that shows linear effects way below 10,000 millirem. Not even a quadratic term in the direction of a threshold. The implication about lack of effects at low doses for workers has also been thrown into doubt by the Cardis study, where the mean dose was 2,000 millirem. When I first got into this business, the supposed level of detectable effects was 50,000 millirem. How much longer do we have to play this game? See also my discussion of the Health Physics Society position statements in my response to Frazier and in my revised report.*** ***It is ironic that Boeing's apparent position against the use of quantitative estimates of low-dose health risks is counter to what the Department of Energy is doing in connection with cleanup of the SSFL site. In the 2002 Environmental Assessment, DOE staff, in order to show that the proposed cleanup plan is safe, make exactly the same kind of calculation I make in my report:*** ***"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)*** ***Those responsible for cleaning up contaminated sites would have a difficult time justifying low-levels of residual contamination, without their being able to make such calculations. The Health Physics Society is recommending bad public and private policy. It can't be legitimate for DOE to make such calculations for the future in support of Boeing, but not be legitimate for me to make them when looking at the past.***

		• The **General Accounting Office** states, "According to a consensus of scientists, there is a lack of conclusive evidence of low level radiation effects below total exposures of about 5,000 to 10,000 millirem." GAO/RCED-00-152, Radiation Standards. Page 10. June 2000. (http://www.gao.gov/new.items/rc00152.pdf) ***Beyea response, 20e: This statement is out of date, given the release of the epidemiological studies on the Techa Cohort that shows linear effects down to levels below 5,000 millirem and no evidence of a quadratic term that would hint at a threshold. As more and more data is gathered, the detectable limit decreases. In addition, lack of evidence is not support for a threshold. It is a support for limitations in the data.***
		• The **American Nuclear Society** states, "It is the position of the American Nuclear Society that there is insufficient scientific evidence to support the use of the Linear No Threshold Hypothesis in the projection of the health effects of low-level radiation." Health Effects of Low-level Radiation. American Nuclear Society Position Statement No. 41. June 2001. (http://www.ans.org/pi/ps/docs/ps41. pdf) ***Beyea response, R20f: This is a self-serving statement by the American Nuclear Society denying the sufficiency of scientific evidence to support the use of the Linear No Threshold hypothesis. Cigarette companies took the same position on smoking.***
R-21	Panel Report, Page 19 and 20 and Beyea Report, Pages 5 and 6	The historical record and scientific literature demonstrates that only small quantities of xenon-133 and krypton-85 gases were released following the SRE accident and that large quantities of iodine-131 and cesium-137 were not released as claimed by the AP reports. There is no evidence that the SRE incident resulted in adverse health effects in the community. Nevertheless, it is important to acknowledge the statement made in the Panel report on page 20 at the end of section 3. It is repeated in its entirety here. "At the same time, the reader must be reminded that these cancers, if they occurred, would have been amidst a population of several million people and over a time period of many decades (life time of residents exposed to the 1959 releases or to cesium remaining in soil). Dr. Beyea's analysis concludes that much of the population dose could have been delivered at significant distances from the site – such as Los Angeles – where many more people live than live nearby. Although the estimated individual doses, and cancer risks, are smaller at greater distances, the total number of cancers produced are larger due to the population size. The ability of epidemiological studies to identify these cancers, if they exist, in a population that large, is limited, given the uncertainty about where the exposures occurred and the great mobility in the population."

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	-	This statement is a rewording of Dr. Beyea's cautionary statement on page 5 and 6 of his report. This also is repeated in its entirety here, *"These cancers would have occurred among a background of millions of cancers in the population exposed in the LA Basin, including a contribution from natural background radioactivity that would have exceeded the contribution from SSFL in aggregate."* These statements acknowledge that estimated theoretical cancers were calculated based on computed population doses (in person-rem). This is a consequence of misapplying the LNT model of radiation risk. The model says that if 1,000 people receive 10 rem exposure each (10,000 person-rem) then 10 radiation induced cancers would result. The model also says that if 10,000,000 people receive 0.001 rem (1 millirem) exposure each (also 10,000 person-rem) then 10 radiation induced cancers would also result. Thus, the LNT model potentially can predict large numbers of theoretical cancers if very large numbers of people are exposed to very low levels of radiation. This is counter-intuitive and is the reason why radiation professionals avoid using population doses to compute theoretical cancers. ***Beyea Response, R21a:*** ***I don't see why use of the LNT at low doses is counterintuitive. A small risk spread over many people still leads to excess cancers. There is still a social responsibility here. It would be eminently reasonable to have parties responsible for projected excess cancers contribute to cancer research in an amount proportional to the value of what a jury might award for a cancer. Possibly the award should be discounted by a DDREF, if a jury could be convinced of the reasonableness of the DDREF concept in face of counterarguments.***
	Beyea->	***I note that there are different views about why "radiation professionals" avoid computing total projected cancers. This is the first time I have ever come across "counter-intuitiveness" given as a reason. Another possible explanation for the reluctance to compute such numbers is that they are very bad publicity for those who pay the bills of radiation professionals. Still another reason is that such calculations may confuse the public. Indeed, in my experience, many members of the public assume that their individual risk is high, when total cancers are high, no matter how large the population over which the number is spread. This causes undues stress, particularly if there is no remedy. On the other hand, failure to penalize parties for such releases means that there is no deterrent for future releases.*** It is also instructive to expand upon the cautionary words in these paragraphs. Let us assume that the population in the Los Angeles area over the four and a half decades since the SRE accident is 8,000,000. This is consistent with the population data used by Dr. Beyea. In a population of that size we would expect approximately 3,360,000 cancers to occur during their collective lifetimes (the risk of contracting cancer in the US is approximately 42%). Assuming that the LNT model of radiation risk is valid at exposures similar to background radiation, the number of theoretical cancers induced from exposure to background radiation in 8,000,000 lifetimes is approximately 168,000 (~5% of total cancer rate). As Dr. Beyea acknowledges on pages 5 and 6 of his report, his predicted 260 additional cancers are low compared to not only the actual expected number of total cancers in the population but also low compared to the theoretical number of cancers that the LNT model would attribute to background radiation exposure

		Looking at it from another perspective, the population thyroid dose from iodine-131 of 65,000 person-rem and population whole body dose from cesium-137 of 75,000 person-rem, is low compared to the population dose of 112,800,000 person-rem from 300 millirem/year background radiation to 8,000,000 people for 47 years since the accident. ***Beyea Response, R21b: I agree with putting my calculations in perspective by comparing them to the numbers expected from natural background radiation. The individual risk is small; the social risk is not. It is important to keep these two contrary ideas in mind. The small percentage of persons who would have gotten extra damage to their cells that would have caused, or increased the onset date of, their cancer were unlucky. We don't want to completely ignore their bad luck.*** ***This whole debate, by the way, is one of public policy, not science.*** Finally, if Mr. Hirsch's assertion that radiation risk is actually an order of magnitude higher than that assumed by regulatory agencies, one would have to conclude that 1,680,000 people would develop cancer from background radiation. ***Beyea Response, R21c: There is not necessarily a single factor that causes a particular cancer. Background radiation may be a contributing factor to a large number of cancer cases.***
R-22	Lochbaum Report, Title Page	The title of Mr. Lochbaum's report is "An Assessment of Potential Pathways for Release of *Gaseous* Radioactivity Following Fuel Damage During Run 14 at the Sodium Reactor Experiment," (emphasis added). But the Lochbaum Report discusses the release of iodine-131, which forms a solid, uranium tri-iodide, when produced by U-235 fission. Even if small quantities of

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		molecular or atomic iodine were to be released directly into liquid sodium, ***<u>Beyea Response, R22a</u>: But was radioidine and radiocesium released directly into the sodium, or were these isotopes released into sodium vapor bubbles, which may have risen to the surface of the coolant, carrying the radioisotopes through the liquid coolant?*** **Beyea->** it is well known that iodine readily reacts with sodium to form a solid sodium iodide, which stays in the sodium system until it either plates out or is removed by the cold trap. Elemental cesium is also solid as are its various molecular salts. Thus, Mr. Lochbaum does not attempt to estimate the release quantities of the only gaseous fission products that were released during the incident. **Beyea->** ***<u>Beyea Response, R22b</u>: Cesium is a solid or gas depending on the temperature. We are not talking about room temperature here. Cesium can form a vapor, if hot enough. It can also be released as a component of small particulates.***
R-23	Lochbaum Report, Page 1	Mr. Lochbaum starts with the observation that 13 of 43 (or approximately 30%) of the fuel elements were damaged. He assumes that all of the parts of these 13 fuel elements were damaged and/or melted (although this assumption is not supported), and he therefore assumes that 30% of the core's fission product inventory was released to the environment. This is his "upper bound" estimate. He then acknowledges that a large fraction of the fission products would have been retained in the reactor system by a variety of means. He derives an unsupported estimate that the fraction of radiation released from the fuel into the sodium coolant would be 10%, and he then assumes that the release fraction from the cover gas to the environment would be 10% (for cesium-137) and 100% for iodine-131. Thus, the lower bound release therefore appears to be 0.3 x 0.1 x 0.1 = .003 = 0.3% for cesium-137 and 0.3 x 0.1 x 1.0 = .03 = 3% for iodine-131. He then says that the best estimate release would be the average of the upper and lower bound, or ~ 15%. Mr. Lochbaum's release fractions do not account for the fact that any iodine-131 or cesium-137 released from the fuel would have been retained by the sodium coolant. ***<u>Not if there was bubbling</u>***. Because the sodium coolant would bind up any iodine-131 and cesium- 137 upon contact, this coolant acted as a six-foot deep protective barrier between the reactor core and cover gas. Yet Mr. Lochbaum assumes that 100% of these fission products would have somehow migrated up through this pool of sodium above the core without coming into any contact with it. **Beyea->** **Beyea->** ***<u>Beyea response, R-23</u>: A bubbling scenario allows for passage through the sodium coolant. Certainly, some fraction of iodine and cesium inside a bubble of sodium vapor would have reacted with the bubble surface. However, the upper limit number assumes the rate of reaction with bubble surface is small compared to the rate of passage into the cover gas.***

R-24	Lochbaum Report, Page 5 and 7	Several quotes from the original AI reports(1, 2, 3) are made including, "... no radiological hazard was presented to the environs," and "... no radiological emergency of any nature occurred." These statements reflected the facts known at the time (which have been confirmed by later analyses such as those by Christian(4) and Daniel(5)), that only low levels of gaseous Xe-133 and krypton-85 had been vented in compliance within federal airborne limits such that off-site doses would be low, and not represent a hazard to the community. (1) NAA-SR-4488, "SRE Fuel Element Damage – Interim Report", A. A. Jarrett (Editor), November 15, 1959 (2) NAA-SR-4488 (Suppl.), "SRE Fuel Element Damage – Final Report", 1961 (3) NAA-SR-6890, "Distribution of Fission Product Contamination in the SRE", R. S. Hart, March 1, 1962 (4) "Chemical Behavior of Iodine-131 During the SRE Fuel Element Damage in July 1959. Response to Plaintiff's Expert Witness, Arjun Makhijani", Jerry D.

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		Christian Ph.D., May 26, 2005 (5) "Investigation of Releases from Santa Susana Sodium Reactor Experiment in 1959", John A. Daniel Sr., May 27, 2005
R-25	Lochbaum Report, Page 9	The report states, *"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."* There is well-documented evidence of what was vented through the hold-up tanks. Contemporaneous records(1) from November 1959 indicate that 17 separate ventings of the gaseous hold-up tanks occurred between the date of fuel damage in July and September 30th, when hold-up tank activity reached normal levels. This inter-office letter documents the fact that approximately 28 curies of noble gases were released during a 10-week period. Activity concentration of the hold-up tanks (in terms of μCi/cc) was measured prior to each venting operation. With knowledge of the volume of each hold-up tank, the total activity released in each vent operation could be calculated (in terms of μCi). By summing each vent operation the total release in terms of Ci (curies) could be calculated. **Beyea->** ***Beyea Response, R25: Sounds like an interesting letter about the venting of the hold-up tanks. The biggest problem with relying on such a letter is that not all radioactivity would necessarily have exited by way of the hold-up tanks. As I understand it from conversations with Gordon Thompson, the automatic equipment that was supposed to send compressed waste gas with high activity to the hold-up tanks malfunctioned prior to the accident and was replaced with a manual apparatus operated from the control room. Pathways also existed that would allow uncompressed gaseous radioactive waste to be released to the atmosphere via the stack. It is my further understanding that Atomics International itself concluded that the gaseous-waste system must have been in bypass mode on July 15th as a result of inadvertent action in the control room. Thus, there may have been a fairly direct path to the stack. If these operational details are correctly described, Boeing would be making a debatable assumption about the primacy of the hold-up tanks. As for the 28-curies released from the hold-up tanks, the assumption is made that all of the ventings were recorded during the confusion of the accident. Also, bear in mind that the document under discussion was likely produced by defendants in litigation. We are not hearing about documents produced by the other side. Plaintiffs' experts were presumably shown this same document; yet they did not fold their case.***

		This inter-office letter was made available to Elizabeth Crawford (Staff Assistant to Ventura County Supervisor, Linda Parks) on January 21, 2005. Judy Mikels (Ventura County Supervisor) and Mary Weisbrock (Save Open Space) were also sent copies of the letter(2). The inter-office letter was probably not available to Mr. Lochbaum during the conduct of his study. (1) Atomics International Inter-Office Letter from G. Borg to W. L. Fisher, "Quarterly (July through September 1959) Report of Activity Released to Atmosphere," November 20 1959. (2) Letter to Elizabeth Crawford from Phil Rutherford, "Sodium Reactor Experiment (SRE) Original Release Data," January 21, 2005.
R-26	Lochbaum Report, Page 10 and 13	Lochbaum quotes the 1959 AI report, "... *the results of a high bay air sample showed that the high bay activity level was 2 x 10^{-9} µCi/cm^3."* and reproduces the High Bay Airborne Area Activity chart. Mr. Lochbaum uses this information in reference to his argument that high activity readings in the high bay above the reactor refueling deck were evidence of an additional pathway for release through the HEPA filtered ventilation system. Lochbaum states, *"That large amounts of radioactivity reached the high bay area is illustrated in the figure titled, High Bay Area Airborne Activity."* The high-bay activity readings used by Mr. Lochbaum are not particularly high and are, in general, less than the current NRCs 2,000 working hour averaged occupational airborne limits (10 CFR 20 Appendix B, Table 1,

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http://www.nrc.gov/reading-rm/doc-collections/cfr/part020/part020-appb.html).

Phase	Isotope	NRC 2,000 working hour averaged occupational limit (μCi/cm3)	High Bay Activity (μCi/cm3)			
			July 12	July 13	July 21	July 23
Gas	Xe-133	10-			10- 10	
Gas		10		3.6		1.3
Vapor	I-131	2	10-6	a	(gaseous)	x
Particulate (solid)	Cs-137	6 x 10		10	2 x 10- (particulate)	10-

The only day with differential airborne activity for particulates and gaseous radionuclides was July 21. The particulate activity is less that the 2,000 working hour NRC occupational limit, and the gaseous activity is less than the likely major source of gaseous activity namely Xe-133 and Kr-85.

The relative concentration of particulates vs. gases is small at 1 in 500 to 1 in 5,000, showing that particulates (e.g. potential cesium-137) were present in much lower quantities than gases (e.g. most likely Xe-133 and Kr-85).

The other activity values are not identified as either gaseous or particulate. An air sample collected using an air pump to collect contamination on filter paper would be measuring particulates only. An air sample collected using an air pump to collect contamination on activated charcoal would be measuring gases and particulates. A grab air sample would measure combined gaseous and any still-suspended particulates. Assuming these activities are particulate and gas combined, then all non-differentiated values are much less than the noble gas occupational limit of 10^{-4} μCi/cm^3. Assuming the ratio of particulate to gaseous activity is similar to the July 21 sample, then the particulate contribution will be less than the Cs-137 2,000 working hour NRC occupational limit. It should also be noted that the periods of elevated high bay activity as indicated by the count rate graph are relatively brief, which means that when averaged over the 2,000 working hour year, the airborne activities are very low compared to the occupational limits.

Beyea-> As Mr. Lochbaum describes the ventilation system at the top of page 10, the SRE is designed as a negative pressure system such that air flow travels from the outside environment, through office and administrative areas, to reactor areas such as the high bay re-fuelling deck. Furthermore, air from the reactor areas is exhausted to the outside through high efficiency particulate air (HEPA) filters. **If the HEPA filters are not bypassed.**

In summary, the relatively low airborne activity in the high bay, coupled with the negative pressure building design and the use of HEPA filters, preclude any significant activity from exiting the high bay.

<table>
<tr><th>No.</th><th>Section</th><th>Comments</th></tr>
<tr><td></td><td></td><td>The NRCs current public airborne limits (10 CFR 20 Appendix B, Table 2 are 5 x 10^{-7} μCi/cm^3 for Xe-133 and 7 x 10^{-7} μCi/cm^3 for Kr-85 averaged over a calendar year (8,760 hours). Thus the policy of maintaining vented noble gas effluent to < 1 x 10-7 μCi/cm^3 was protective even by today's standards.</td></tr>
<tr><td>R-27</td><td>Lochbaum Report, Page 13

Beyea-></td><td>Mr. Lochbaum claims "That large amounts of radioactivity reached the helium cover gas above the reactor pool is evident from the table titled Activity History of the Reactor Cover Gas."
The 1962 AI report(1) which was available to Mr. Lochbaum states that only Xe-133 and Kr-85 were identified in the cover gas. Table VII of the report includes the following measured data.
<table>
<tr><th>Isotope</th><th>Cover Gas Concentration (μCi/cm^3)</th><th>Total Cover Gas Inventory (curies)</th></tr>
<tr><td>Xe-133</td><td>7.4</td><td>47</td></tr>
<tr><td>Kr-85</td><td>0.016</td><td>0.2</td></tr>
</table>
If iodine-131 and cesium-137 had been released to the cover gas as alleged in all three AP reports, then these would have been readily detected in the various cover gas samples taken following the accident. Mr. Hirsch, Mr. Lochbaum, and Dr. Beyea claim that all the iodine-131 and cesium-137 had escaped though the cover gas system without leaving any detectable amount by the time samples were taken. This is unrealistic.

<u>Beyea Response, R27</u>: First of all, I do not claim that large amounts of excess material <u>definitely</u> escaped the reactor. What I do claim is that there is a significant probability that it did. There is also a significant probability that it did not. As for detecting Cesium-137, there are at least 3 ways it could have been detected in the cover gas. First, a sample of gas could have been put in a counting apparatus. Depending on how the measurements were made, particulate Cesium-137 could have been detected, as Boeing suggests. However, if the gamma energy windows on the counter were set based on the assumption that only the standard noble gases were present, cesium 137 particulates could have been missed. Furthermore, cesium-137 as a fine particulate may not have been present at the times measurements were taken (there were gaps in coverage). Second, Cs-137 particulates may have plated out on the surfaces above the pool. No measurements have been reported of this possibility, as far as I know. Finally, measuring the cesium caught by the ventilation filters would have been the easiest way to tell if cesium had gotten out of the gaseous capture and release system. The measurements could have been made long after the crisis of the SRE event. Surprisingly, no measurements of the filter radioactivity appear to exist. Boeing is silent about this point. Perhaps, there was no filter in place at the time; perhaps it was bypassed; perhaps it was too hot to measure the isotopic composition.

(1) NAA-SR-6890, "Distribution of Fission Product Contamination in the SRE", R. S. Hart, March 1, 1962</td></tr>
</table>

R-28	Lochbaum Report, Page 13 ***See Beyea Response to R-25.***	Mr. Lochbaum states, "That large amounts of radioactivity reached the gaseous storage tanks is evident from the table titled "Radioactive Concentrations in Gas Decay Tanks," and "No data was found on either the radiation levels at the stack release point or on the number, timing, and radioactivity levels of releases from the gaseous storage tanks." There is well documented evidence of what was vented through the hold-up tanks. Contemporaneous records(1) from November 1959 indicate that 17 separate ventings of the gaseous hold-up tanks occurred between the date of fuel damage in July and September 30th, when hold-up tank activity reached normal levels. This inter-office letter documents the fact that approximately 28 curies of noble gases were released during a 10-week period. Activity concentration of the hold-up tanks (in terms of μCi/cc) was measured prior to each venting operation. With knowledge of the volume of each hold-up tank, the total activity released in each vent operation could be calculated (in terms of μCi). By summing each vent operation the total release in terms of Ci (curies) could be calculated. This inter-office letter was made available to Elizabeth Crawford (Staff Assistant to

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		Ventura County Supervisor, Linda Parks) on January 21, 2005. Judy Mikels (Ventura County Supervisor) and Mary Weisbrock (Save Open Space) were provided copies of the letter(2). (1) Atomics International Inter-Office Letter from G. Borg to W. L. Fisher, "Quarterly (July through September 1959) Report of Activity Released to Atmosphere", November 20 1959. (2) Letter to Elizabeth Crawford from Phil Rutherford, "Sodium Reactor Experiment (SRE) Original Release Data", January 21, 2005
R-29 **Beyea**	Lochbaum Report, Page 13 ***Beyea Response, R29: As I state in my report, the ventilation surfaces were cleaned post accident and may actually have been replaced prior to 1966, when measure-ments were made.***	Mr. Lochbaum states, *"The only information [relative to potential releases from the hold-up tanks] – albeit indirect – covered the radiation levels inside the ventilation system ductwork. This data, from 1966, indicated the radiation levels measured in ductwork upstream of filters was 2 to 20 times the radiation levels measured downstream of the filters. The data clearly demonstrate (a) the ventilation system filters were effective in removing radioactivity from the process flows, and (b) the ventilation system filters did not remove all radioactivity from the process flows."* The "radiation levels" to which Mr. Lochbaum refers are actually "contamination levels." They are measures of radioactive material not measures of radiation levels. The upstream contamination levels (before the filter) range from 756 to 10,181 dpm/100 cm^2. The downstream contamination levels (after the filter) range from 129 to 1,293 dpm/100 cm^2. Assuming that the contamination was due to cesium-137 with a 30-year half life, these levels would not have decayed appreciably in the 7 years since the accident (1959 to 1966). These levels either lower than, or equivalent to, the acceptable levels for "release for unrestricted use" found in Regulatory Guide 1.86(1) of 5,000, 15,000, and 1,000 dpm/100 cm^2 for average, maximum, and removable, â/ã contamination respectively. These relatively modest levels of contamination are not indicative, as Mr. Lochbaum implies, of the passage and release of thousands of curies of cesium-137. dpm/ 100 cm^2 = disintegration per minute per 100 cm_2 (1) U.S. Nuclear Regulatory Commission Regulatory Guide 1.86, "Termination of Operating Licenses for Nuclear Reactors."

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R-30 Beyea	Lochbaum Report, Page 17 ***Beyea Response, R30: I have dealt with the neglect of bubbling by Boeing earlier***	Mr Lochbaum ends his report as he starts it, with a description of his scientific method of estimating the percentage of iodine-131 and cesium-137 released. Mr. Lochbaum makes his observation that 13 of 43 (or approximately 30%) of the fuel elements were damaged. He assumes that all of parts of these 13 fuel elements were damaged (although this assumption is not supported), and he therefore assumes that 30% of the core's fission product inventory was released to the environment. This is his "upper bound" estimate. He then acknowledges that a large fraction of the fission products would have been retained in the reactor system by a variety of means. He derives an unsupported estimate that the fraction of radiation released from the fuel into the sodium coolant would be 10%, and he then assumes that the release fraction from the cover gas to the environment would be 10% (for cesium-137) and 100% for iodine-131. Thus, the lower bound release therefore appears to be 0.3 x 0.1 x 0.1 = .003 = 0.3% for cesium-137 and 0.3 x 0.1 x 1.0 = .03 = 3% for iodine-131. He then says that the best estimate release would be the average of the upper and lower bound or, ~ 15%. Mr. Lochbaum's release fractions do not account for the fact that any iodine-131 or cesium-137 released from the fuel would have been retained by the sodium coolant. Because the sodium coolant would bind up any iodine-131 and cesium-137 upon contact, this coolant acted as a six-foot deep protective barrier between the reactor core and cover gas. Yet Mr. Lochbaum assumes that 100% of these fission products would have somehow migrated up through this pool of sodium above the core without coming into any contact with it.
R-31	Beyea Report **Beyea->**	Dr. Beyea's report contains accusations of deliberate withholding of data, destruction of data, falsification of data, and cover-up by the plant owners and operators. Specific allegations and personal comments are made regarding Dr. Chauncey Starr, the president of Atomics International at the time of the accident. Boeing does not consider these comments by Dr. Beyea to be appropriate for a scientific study, and therefore will not respond to them. ***Beyea Response, R31**: I am engaged in a forensic investigation. In a forensic investigation, one does not take all statements in documents at face value, especially when the authors had a lot at stake. One looks for inconsistencies. This is a big difference between my approach and that taken by Boeing's experts, who take all statements by AI at face value and assume that all information has been provided openly and freely. It is not surprising that I reach different conclusions than do Boeing's experts.* ***I am not saying that Starr fudged records, but that the operators, whose job might be on the line, let alone the future of the company, may have either deliberately or accidentally misread the scale on a meter. They may have peeked under the rock, rather than boldly lifted it up. MORE→***

		Beyea response R31 (Con't). ***I knew Starr professionally; I was on committees with him and I attended round-table meetings with him. He was a rationale supporter of nuclear power. He did some interesting papers on risk. I respected him, even though I often disagreed with him. That does not mean he could never have made a mistake or never tried to protect his company from ruin. Had the partial meltdown become public, his company's reactor design would likely have been eliminated from commercial possibilities. We know there was a cover up, because of the press release that denied the existence of even a release of radioactive noble gases. Boeing, in their response, concedes the fact that the press release was "not forthcoming." That is a cover up. How serious was the cover up? We do not know for sure. The seriousness of the cover-up is the unresolved question, not its existence. I do not mean to use the phrase "cover-up" pejoratively, by the way. I am using it in the forensic sense. I am not aware of any more polite term.*** ***Furthermore, there is internal evidence of some fiddling with the numbers, namely the reported fuel burn-up, which was uncharacteristically low. It was reported to be many times lower than any other period, even though the operators were pushing the reactor for weeks. Boeing is silent on this issue, which I discussed in my report, indicating they have no good answer. In any case, if there were any such fiddling with the burn-up estimates, it must have happened very far down the chain of command and it would have had to have happened right away. Starr would have been unlikely to have known about it. My major complaint about Starr's behavior was an apparent failure to notify public health authorities of a possible release, which would have led, I presume, to extensive measurements in milk and soil. This failure is forensically relevant to my report.*** ***Let me repeat, I am not saying that all of these possible cover-up activities listed were, without a doubt, actually taken. There is a good chance they were not, which is why they are handled as part of the likelihood distribution for releases. The question of whether or not any actions by the operators led to large radiocesium releases can be resolved through suitable measurements at distances of 5-15 miles from the facility.***
R-32	Beyea Report, Page 4	Dr. Beyea makes the statement, *"existing radiocesium measurements are not adequate to determine the magnitude of any elevated releases."* Environmental sampling studies performed over the last 14 years have unequivocally demonstrated that cesium-137 is not in the soils of communities surrounding SSFL at levels that differ from local background. These studies demonstrate that cesium-137 releases of the size postulated by Dr. Beyea could not have occurred. **Beyea->** ***Beyea Response, R32a*** ***The studies cited by Boeing do not demonstrate that cesium-137 releases of the size postulated by the experts I cite could not have occurred. In Chapter 2 of my report, I present calculations to show that elevated releases, as opposed to ground level releases, are not ruled out by on-site soil measurements.***

		Many of these studies have been conducted by organizations independent of Boeing. Those studies conducted by Boeing have been under the oversight of numerous regulatory agencies, including the California Department of Health Services (DHS), the California Department of Toxic Substances Control (DTSC) and the Environmental Protection Agency (EPA). The studies include but are not limited to, (1) McLaren/Hart, “Additional Soil and Water Sampling at the Brandeis-Bardin Institute and Santa Monica Mountain Conservancy”, January 19, 1995 (http://apps.em.doe.gov/etec/7727 1995 0119 MHI AddSoilandWaterSamp.pdf.pdf)

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(2) Environmental Protection Agency, "EPA Update. The U.S. EPA Announces Results of Rocketdyne's Off-Site Sampling Program for the Santa Susana Field Laboratory." July 1995. (http://apps.em.doe.gov/etec/EPAFS.PDF)

(3) Lawrence Livermore National Laboratory, "Soil Sampling for Cesium-137 at the Rocketdyne Recreation Center," 1997.

(4) Ogden Environmental Services. "Bell Canyon Area Soil Sampling Report. Ventura County, California," October 1998.

(5) Foster Wheeler Environmental Corporation. "Final Report. Runkle Ranch Site Investigation. Simi Valley, CA," October 1999.

(6) QST Environmental, "Results of Preliminary Soil sampling at Runkle Ranch in Simi Valley, California," February 5, 1999.

(7) Kleinfelder, "Report of Environmental Sampling. Ahmanson Ranch Project. County of Ventura, CA," January 27, 2000.

(8) Essentia Management Services, "Final Site Investigation Report – Soil Suitability Evaluation - Chatsworth Reservoir, Chatsworth, California," Prepared for the Los Angeles Department of Water and Power, July 22, 2004.

With one exception, no samples from the above studies have exceeded the local cesium-137 background established by Reference 1 above.

The one exception identified in Reference 1 was one localized area immediately to the north of the prior Building 4059 in Area IV of SSFL. The cesium-137 background established in Reference 1 above (Table 38, 1995 report) is,

Range	<0.03 to 0.213 pCi/g
Mean	0.087 pCi/g
St. Deviation	0.062 pCi/g
5_{th} to 95^{th} percentile	<0.03 to 0.21 pCi/g

Using non-parametric statistical tests to compare background distributions to sampled area distributions, McLaren-Hart determined that only one area (Building 4059 watershed) was contaminated with cesium-137 with the following statistics,

Range	<0.077 to 0.385 pCi/g
Mean	0.20 pCi/g
St. Deviation	0.08 pCi/g
5^{th} to 95^{th} percentile	0.04 to 0.36 pCi/g

Thus the mean cesium-137 was approximately twice that of local background.

The EPA stated in a fact-sheet (Reference 2) following the BBI/SMMC sampling that these low levels of radionuclides are less than the 1-in-a-million cancer risk level. EPA stated that, *"EPA has determined that the radionuclides do not pose a threat to human health or the environment."* Boeing has since purchased this land from the Brandeis Bardin Institute.

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	Beyea->	Dr. Beyea does not reference any of these studies in his extensive list of almost 200 references. He does not cite any support for his statement that *"existing radiocesium measurements are not adequate to determine the magnitude of any elevated releases."* ***Beyea Response 32b.*** *I do indeed provide support for my statements about elevated releases. See Tables 2-8 through 2-10 in the original report, which show predicted soil concentration as a function of plume rise. I state after the Tables:* ***"Looking at the Tables, and bearing in mind the likely factor of ten uncertainty with angle, it seems quite unlikely that a release of 300-Ci could have gone undetected or hidden for a plume rise less than 150-200 meters. Or, if it did, it would be quite easy to find the fingerprint today with a systematic search. Rather than argue about the precise cutoffs implied by the above Tables, it might be wiser to undertake soil measurements. For plume rises above 150 meters, it should be possible to find the fingerprint of the release at distances that have not apparently been sampled around SSFL."*** ***I also state: "Before undertaking an epidemiological study to see if large amounts of radiocesium were released, it would make sense to first send a detector-equipped helicopter in circles around SSFL and into the regions of LA beneath the return flows."*** ***In the revised report, partly to respond to Boeing's criticism and partly to deal with new data uncovered for LA parks, I have gone into this issue even further, accounting for frequency information on stability classes, although I was forced to rely on data collected at the Burbank Airport,***
R-33	Beyea Report, Page 5 **Beyea->**	Dr. Beyea states *"the average number of predicted cancers was 260 with a 95%-confidence range of 0 to 1800."* This statement has been reported in the Los Angeles Times as: *"predicted cancers were between 260 and 1800,"* (LA Times October 6, 2006). ***Beyea Response, R33:*** ***I very much regret the fact that this mistake appeared in the press. As I stated earlier:*** ***I do take responsibility for the failure of the press to report the lower limit of zero cancers that I calculated. That was unfair to Boeing and its experts. It may also have caused undue concern among some residents. I should have been more forceful in my report in pointing out that a zero release was just as likely as a high release. I never expected such attention by non-experts.***

R-34	Beyea Report, Pages 5 and 6	Dr. Beyea states, *"These cancers would have occurred among a background of millions of cancers in the population exposed in the LA Basin, including a contribution from natural background radioactivity that would have exceeded the contribution from SSFL in aggregate."* These statements acknowledge that estimated theoretical cancers were calculated based on computed population doses (in person-rem). The figures reported misapply the LNT model of radiation risk. The model says that if 1,000 people receive 10 rem exposure each (10,000 person-rem) then 10 radiation induced cancers would result. The model also says that if 10,000,000 people receive 0.001 rem (1 millirem) exposure each (also 10,000 person-rem) then 10 radiation induced cancers would also result. Thus the LNT model potentially can predict large numbers of theoretical cancers if very large numbers of people are exposed to very low levels of radiation. This is counter-intuitive and is the reason why radiation professionals avoid using population doses to compute theoretical cancers. **Beyea->** ***<u>Beyea Response, R34a:</u> As I said earlier: I don't see why use of the LNT is counterintuitive. A small individual risk spread over many people still leads to excess cancers. As I also stated earlier, there is a social responsibility here. It would be eminently reasonable to have parties responsible for projected excess cancers contribute to cancer research in an amount proportional to the value of what a jury might award for a cancer, possibly discounted by a DDREF, if a jury could be convinced of the reasonableness of the DDREF concept in face of counterarguments.*** ***I note that there are different views about why "radiation professionals" avoid computing total projected cancers. This is the first time I have ever heard an explanation that relies on the idea that the use of the LNT is counter-intuitive. Another, more cynical, possibility is that such calculations are very bad publicity for those who pay the bills of radiation professionals. Still another reason is that such calculations may confuse the public. Indeed, in my experience, many members of the public assume that their individual risk is high, when total cancers are high, no matter how large the population over which the number is spread. This causes undue stress, particularly if there is no remedy. On the other hand, a policy that fails to penalize parties for such releases fails to provide a deterrent for future releases.***

	Beyea->	It is also instructive to expand upon the cautionary words in these paragraphs. Let us assume that the population in the Los Angeles area over the four and a half decades since the SRE accident is 10,000,000. In a population of that size we would expect approximately 4,200,000 cancers to occur during their collective lifetimes (the risk of contracting cancer in the US is approximately 42%). Assuming that the LNT model of radiation risk is valid at exposures similar to background radiation, the number of theoretical cancers induced from exposure to background radiation in 10,000,000 lifetimes is approximately 210,000 (~5% of total cancer rate). Dr. Beyea states on pages 5 and 6 of his report that his predicted 260 excess cancers (which is based on his incorrect estimates of radiation exposure) are low compared to not only the actual expected number of total cancers in the population, and are also low compared to the theoretical number of cancers that the LNT model would attribute to background radiation exposure. ***<u>Beyea response, R34b:</u> I agree that my cancer estimates are low compared to the theoretical number of cancers that the LNT model would attribute to background radiation exposure. I agree that this is an important point to stress. I have not checked the other calculations presented above by Boeing.*** Looking at it from another perspective, the population thyroid dose from iodine-131 of 65,000 person-rem and population whole body dose from cesium-137 of 75,000 person-rem, is low compared to the population dose of 141,000,000 person-rem from 300 millirem/year background radiation to 10,000,000 people for 47 years since the accident.

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R-35	Beyea Report, Page 9	Dr. Beyea states, *"before undertaking an expensive epidemiological study, it would seem wiser to first undertake measurements of radiocesium in soil at locations around the plant, so as to narrow the great uncertainties that make current dose estimates of marginal usefulness for epidemiology. In particular, the existing radiocesium measurements are not adequate to determine the magnitude of any elevated releases."* Many Boeing sponsored and independent studies have been conducted at locations around the plant that included cesium-137 soil analysis. No evidence of cesium-137 soil contamination, that would have resulted from the release thousands of curies of cesium-137, has been found. See comment on Beyea Report page 4 above. **Beyea->** ***Beyea response, R35:*** ***All of the soil measurements to which Boeing refers above were made close to the facility, which puts them under the waterfall, so to speak, for elevated releases. As I discussed in Chapter 2 of my report, they are too close to be informative about elevated releases, although the expanded analysis in the revised report does indicates that they rule out elevated releases for unstable atmospheric conditions, which occur about 26% of the time in July. Also, for the revised report, Harold L. Beck supplied me with measurements made in the 1980s that I had not previously seen. These measurements are sufficiently far from the SSFl site to be informative about elevated releases, at least for certain angles. These measurements had nothing to do with SSFL, so were not restricted to distances near the plant.***
R-36	Beyea Report, Page 13	Dr. Beyea states, *"From the beginning, management played down the seriousness of the event, as indicated by the press statement that was issued by Atomics International on August 29, 1959 and circulated by the US Atomic Energy Commission (AI 1959).* *"During Inspection of fuel elements on July 26 at the Sodium Reactor Experiment.....a parted fuel element was observed. The fuel element damage is not an indication of unsafe reactor conditions. No release of radioactive materials to the plant or its environs occurred..."* *In the press release, the number of damaged fuel elements was understated and the leakage of radioactivity from the stack was not mentioned."* Boeing acknowledges that the Atomics International press release following the accident was vague and not fully informative.

R-37	Beyea Report, Page 13	Dr. Beyea states, *"No post-event analysis of the amount of radioactivity on the ventilation filters is available, which is the first place one would look to get an idea of the amount released, taking into account filter efficiency .... Yet, measurements made after decommissioning of the amount of surface contamination before and after the filters imply that there was a filter in place."* These statements are somewhat contradictory. The important facts about the measured data related to the HEPA filter system ventilation ducts are as follows. The upstream contamination levels (before the filter) range from 756 to 10,181 dpm/100 cm^2. The downstream contamination levels (after the filter) range from 129 to 1,293 dpm/100 cm^2. Assuming that the contamination was due to cesium-137 with a 30-year half life, these levels would not have decayed appreciably in the 7 years since the accident (1959 to 1966). These levels either lower than, or equivalent to, the acceptable levels for "release for unrestricted use" found in Regulatory Guide 1.86*) of 5,000, 15,000, and 1,000 dpm/100 cm^2 for average, maximum, and removable, â/ã contamination respectively. These relatively modest

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	Beyea->	levels of contamination are not indicative of the passage and release of thousands of curies of cesium-137. ***<u>Beyea response, R37:</u> True, if one has a reasonable value for deposition velocity inside the ventilation system and the exhaust speed, one can infer the amount of cesium that passed by sections of the ducts. I don't have good values for these unknowns. Furthermore, as I state in my report, the ducts were apparently cleaned, and possibly replaced prior to the measurements that were taken years later. The obvious way to have obtained an estimate at the time of the accident would have been to make measurements on the filter, assuming one was in place or not bypassed. Estimating the amount of cesium that passed through the filter only requires knowledge of the filter efficiency for cesium and a measurement of the amount of cesium on the filter. Very simple. Perhaps, the filter was too hot to allow the necessary measurements to be made. Perhaps, no one thought to make such measurements. Perhaps, the measurements were deep-sixed, because the results didn't seem credible. Perhaps, the filter was in bypass mode or empty, waiting for a replacement unit.***

R-38	Beyea Report, Page 15 **Beyea->** **Beyea->**	Dr. Beyea cites the 1957 Windscale reactor accident in the U.K. as his primary source by which to estimate the SRE release. Windscale released 20,000 curies of iodine-131. ***Beyea response, R38a:*** ***This is a misunderstanding. Windscale is not my primary source for the SRE release, although at least one press report came to this conclusion. Hopefully, I have been clearer about this in my revised report. What I said was that Windscale should have been the basis for Atomics International deciding on whether or not to notify public health authorities and whether or not to make offsite measurements of soil and milk on its own.*** By using the ratio of the thermal power levels (9-to-1), the 50% retention factor for the Windscale filters and the alleged non-operation of SRE filters, Dr Beyea calculates that the SRE released 4,400 curies of iodine-131. This assessment overlooks several crucial differences between the Windscale accident and the SRE accident. (1) Windscale was air-cooled and following the accident there was a direct pathway from the damaged core to the outside environment through the stack filters, which became inoperable due to the intense heat. In contrast, the SRE uranium fuel continued to be immersed and cooled in a 50,000 gallon pool of liquid sodium. ***Beyea Response, R38b:*** ***There may have been a direct path through air or sodium vapor to the outside environment through the stack filters at SRE as well as at Windscale, if a) there was sufficient boiling at SRE to send gas bubbles through the sodium coolant to the blanket gas, and b) the manual feed to the holding tanks was in bypass mode.*** ***If the filter at SRE was operable, where are the post-accident measurements of the filter?*** .

		(2) The graphite moderator surrounding the Windscale uranium fuel actually burned in the air "cooling" flow for several days. The air coolant therefore became an oxidant which exacerbated and prolonged the fire. The SRE graphite did not burn since there was no oxygen in the system to initiate a fire. The SRE graphite and uranium fuel continued to be cooled and immersed in a 50,000 gallon pool of liquid sodium **Beyea->** ***Beyea response, R38c: We do not know how violent the boiling was in the SRE's sodium coolant due to reduced cooling capability. There was certainly sufficient power in the reactor to cause boiling.*** (3) The burning graphite in Windscale led to significant melting of uranium fuel. In contrast, very little of the uranium fuel in the SRE melted *("Examination of the recovered fuel slugs from damaged [fuel] elements showed no evidence of significant melting,"* (NAA-SR-6890, "Distribution of Fission Product Contamination in the SRE", R.S. Hart, March 1, 1962, page 21). **Beyea->** ***Beyea response, R38d: The extent of the melting is in dispute. Furthermore, melting is not required to get large release fractions. Near-melting is sufficient, as I discuss in Appendix 5 of my revised report and in my narrative response to Boeing.*** (4) *"Even though iodine is very volatile, it did not escape to the cover gas because it undoubtedly combined with the sodium as rapidly as it was evolved. No iodine was ever detected in reactor cover gas samples,"* (NAA-SR-4488, "SRE Fuel **Beyea->** ***Beyea response, R38e: The "undoubtedly" in the above sentence is too strong. As I have stated above, iodine inside bubbles of sodium vapor could have escaped the sodium coolant, depending on the amount of heat being diverted into vapor formation. I don't know if anyone even looked for iodine in the reactor cover gas samples.*** Element Damage – Interim Report," A. A. Jarrett (Editor), page IV-C-5, November 15, 1959). In contrast, iodine-131 escaping from the Windscale fuel had a direct pathway to the outside environment. The Windscale data, therefore, is not useful for estimating releases from the SRE **Beyea->** ***Beyea response, R38f: Although I did not use Windscale other than as a benchmark from which to scale releases using other experts' source terms, I disagree with Boeing's statement that Windscale is not useful***
R-39	Beyea Report, Pages 18 And 19	Dr. Beyea further supports his estimate of iodine-131 release by reference to three other studies either commissioned by the AP or commissioned by plaintiff's attorneys in the litigation, *"O'Connor et. al. vs. The Boeing Company."* (1) Dr. Beyea refers to "Releases of Hazardous Material from the Santa Susana Field Laboratory," Gordon Thompson, Executive Director of the Institute for Resource and Security Studies (http://www.irss-usa.org/). IRSS is another anti-nuclear organization. This document was commissioned by the AP but has not been published either on the IRSS website or the Advisory panel website. Boeing is therefore not able to comment on its assumptions or methodology.

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	Beyea->	(2) Dr. Beyea uses the estimates of Mr. Lochbaum of the anti-nuclear Union of Concerned Scientists. Mr. Lochbaum's report has already been critiqued above. (3) Lastly Dr. Beyea uses newspaper reports of estimates made by another anti-nuclear expert hired by plaintiffs' attorneys' in the litigation, *"O'Connor et. al. vs. The Boeing Company."* The sources used by Dr.Beyea to estimate the distribution of iodine-131 and cesium-137 releases cannot be considered unbiased. The use of these reports skews Dr. Beyea's modeling of exposure and cancer risk. ***Beyea response, R39:*** ***I include a balanced set of experts in determining a likelihood distributions for releases. If the experts Boeing doesn't like were the only sources I had used, my results would indeed be skewed. However, these sources to which Boeing objects are balanced by experts hired by Boeing and by Atomics International. In my revision, I have added one of Boeing's new experts, explicitly hired by Boeing to review the Advisory Panel's report.*** ***The whole point of my methodology is to take into account the full range of expert opinions. To pass over my approach, as Boeing does above, suggests to me that the audience for this passage was not the Advisory Panel or its experts, but persons who have not read my report, such as Boeing's senior management and Board of Directors.***
R-40	Beyea Report, Page 30 **Beyea->**	Dr. Beyea claims that cesium-137 has been measured at 240 times background (24 pCi/g) outside the SSFL fence. This is incorrect. The 2003 Annual Site Environmental Report page 5-13 (http://apps.em.doe.gov/etec/ASER2003.pdf) to which he refers is discussing the Radioactive Material Handling Facility (RMHF) fence, not the SSFL fence. The area was on-site, not off-site. The discussion included the fact that the area was remediated ***Beyea response, R40:*** ***I have corrected this error in my revised report. I thank Boeing for pointing it out.***

R-41	Beyea Report, Page 30	Dr. Beyea postulates a situation where 1,000 curies of cesium-137 is spread over 10,000 square kilometers and would result in an average concentration of 0.25 pCi/g, which would be 2.5 times the average background of 0.1 pCi/g. He then claims that he was not able to find any evidence of the 0.1 pCi/g in the literature. Local cesium-137 background was established by the McLaren-Hart study[1] (Table 38) and is, Range <0.03 to 0.213 pCi/g Mean 0.087 pCi/g St. Deviation 0.062 pCi/g 5th to 95th percentile <0.03 to 0.21 pCi/g This is considerably below literature sources for U.S. cesium-137 in soil.
	Beyea->	***Beyea Response, R41a:*** ***In saying that I could find no evidence for the 0.1 pCi/g value for average background around SSFL, I meant the peer-reviewed literature. I should have been clearer. Because deposition from weapons tests tends to be proportional to rainfall, Southern California has one of the lowest levels of radiocesium in the country. The lower pattern for Southern California is visible in the maps produced in the National Academies Fallout study (NAS/IOM 1998). Relying on the McLaren-Hart study, as Boeing does, for the range in soil concentrations around SSFl assumes there is no contribution from the SSFL facility -- an assumption I would like to get away from. I note that the concentrations in the mountains may be quite different from the lowlands, because the meteorology, rainfall, and fog situations are so complicated there.***
		Argonne National Laboratory (ANL)[2] states, *"The concentration of cesium-137 in surface soil from [weapons test] fallout ranges from about 0.1 to 1 picocurie (pCi)/g, averaging less than 0.4 pCi/g."* The EPA[3] quotes 0.7 pCi/g as an average U.S. background, with a range of 0.1 to 3.5 pCi/g. EPA derived its background data from NCRP 94[4] which was published in 1987. Therefore, these data may need to be decayed by a factor of $e^{-19/30}$ or 0.64.
	Beyea->	***Beyea Response, R41b. Southern California has a low background rate because of low rainfall. It is not appropriate to use values for Southern California that are averages over the entire country.*** Dr. Beyea's reliance on 0.1 pCi/g does not recognize that the upper range of local background is 0.2 pCi/g. This is very close to his postulated contamination level of 0.25 pCi/g.

Furthermore, the further from SSFL we go, the more we need to rely on the literature values for U.S. cesium-137, which are considerably more variable and considerably higher than local background.

Beyea Response, R41c. I repeat: Southern California has a low background rate because of low rainfall. It is not appropriate to use values for the entire country. To assume that the range of background values found on-site is reflective of only rainfall is to assume, rather than prove, there were no significant releases.

Therefore Dr. Beyea's expectations of being able to distinguish his postulated contamination level of 0.25 pCi/g above

Beyea response, R41d: Disagree. It is possible to find a signal of 0.25 pCi/g. See next response. For elevated releases, we are interested in measurements in the greater Los Angeles area and comparable distances in Ventura County. As for the upper range of local contamination being 0.2 pCi/g, this assumes no contamination from SSFL. Much of this discussion has been made moot by data sent to me by Harold L. Beck after release of my original report. These newly identified measurements, taken in greater LA, estimate total pCi in the soil column, so can be more directly compared to model predictions of soil deposition. They show the kind of measurements that could put to rest the debate about releases greater than around 30 Curies. By themselves, they severely limit elevated releases of radiocesium in certain directions.

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	Beyea->	background will be problematic at best and impossible at worst. ***Beyea Response, R41e: When one fits an expected pattern to data with errors, it is straightforward to pull a signal out of the noise, provided one has enough data points (Beyea et al. 2006). The number of data points needed, i.e., number of measurements, depends on the range of variation in the background and the uncertainty in the predicted pattern of the signal one wants to find. Reading between the lines, I take it that Boeing does not want to end up paying the bills for such a study.***
		(2) Argonne National Laboratory, "Human Health Factsheet – Cesium," August 2005, http://www.ead.anl.gov/pub/doc/cesium.pdf.
		(3) EPA 402-R-96-011A, "Technical Support Document for the Development of Radionuclide Cleanup levels in Soil," Appendix O, Table O-, page O-9, http://www.epa.gov/radiation/docs/cleanup/402-r-96-011 a.htm.
		(4) NCRP-94, "Exposure of the Population in the United States and Canada from Natural Background Radiation," National Council on Radiation Protection and Measurements, 1987.
R-42	Beyea Report, Page 30	Dr. Beyea claims that 0.25 pCi/g in soil gives an exposure of 0.1 rem or 100 millirem over a period of 30 years, without providing a source for his statement. The EPA's Dose Compliance Concentration website (http://epa-
		dccs.ornl.gov/dose search.shtml) allows us to compute the effective exposure as a
	Beyea->	Function of soil contamination for residential scenarios (which is appropriate for suburban Los Angeles). This EPA online calculator computes that 0.25 pCi/g of cesium-137 in soil will give a first year dose of 0.1777 millirem. The 30th year dose will be ~ 0.0888 millirem. The average dose over 30 years will be ~0.1333 millirem/y or a total dose of 4 millirem over a 30 year period, not 100 millirem. Dr. Beyea has therefore overestimated exposures by a factor of 25. ***Beyea Response, R42: To a certain extent, we are comparing apples and oranges here. The EPA website looks at doses after material has penetrated deeply into the ground. And, they are measuring the future dose. I am measuring the retrospective dose. This explains most of the discrepancy between Boeing's numbers and mine. After the hypothetical release in 1959, the Cesium-137 (and Cesium-134, which I also included) is all deposited at the surface, so has a very high soil concentration. Over time, it penetrates into the soil, reducing the surface soil concentration. The initial dose rate is higher too, because of the reduced shielding. After 30 years, the dose rate has declined considerably—to about 1/10th of its original value for weapons fallout (Bunzl et al. 1997). I should have been clearer about this in the original version of my study, Boeing's numbers apply to the dose over the next 30 years, which is reduced from the first 30 year's dose, although the total number of exposed people increases.***

R-43	Beyea Report, Page 30	Dr. Beyea claims that 0.25 pCi/g in soil gives an exposure of 0.1 rem or 100 millirem over a period of 30 years, which is equivalent to a cancer risk of 1-in-10,000 to 1-in-3,000, without providing a source for his statement. The EPA's Preliminary Remediation Goal website (http://epa-prgs.ornl.gov/radionuclides/prg search.shtml) allows us to compute the theoretical cancer risk as a function of soil contamination for residential scenarios (which is appropriate for suburban Los Angeles). This EPA online calculator computes that 0.0597 pCi/g of cesium in soil will give a cancer risk of 1-in-1,000,000 for a 30 year exposure period. By ratioing, 0.25 pCi/g of cesium-137 will give a cancer risk of 4.2 x 10^{-5}, or 4.2-in-100,000. Therefore, Dr. Beyea has overestimated cancer risks by a factor of between 2.4 and 7.9. Beyea-> ***Beyea response, R43:*** ***There is no overestimating of cancer risks, only a lack of clarity in my report about dose timing. I have clarified the ambiguity in my calculations for the revised report about the timing of the received dose, which was prior to the soil contamination declining to 0.1 pCi/g. I recongnize that I should have been clearer in the original version. The 0.25 pCi/g is the final surface contamination after 30 years, not the value over the exposure period, which is higher. As a result, the EPA web calculator used by Boeing is not directly applicable. EPA does make a more optimistic assumption than I do, however, in the default values they enter in the boxes on the web page. Their default value assume a building shielding factor of 0.4 during the time a person is indoors. In contrast, I assume a value close to one, on the assumption that the building is made of wood, not brick. In addition, in reviewing my calculations as a result of the Boeing comments, I realized there was an inconsistency in my computation of the soil concentration and the new measurement data of which I made use (Bunzl et al. 1997). I simply spread the final cesium uniformly over a 10-cm soil depth, which would have been appropriate under the old paradigm. In fact, the new data suggests that cesium from a close-in source does not penetrate so fast. This means the surface concentration will be higher than I assumed after 30 years for the same release and release conditions. It means that the dose assigned to a 30-year surface contamination will be lower than I originally reported. One result is that monitoring data will set even stricter limits on releases than I concluded in the first version of my report. There are no implications for my original dose calculations, since I made the calculations for a total amount initially deposited, not the amount in the top cm of soil.***

R-44	Beyea Report, Page 30	Dr. Beyea claims that radiation risk coefficients are either 0.0015 or 0.003 cancers per rem. Actually, he apparently meant to say "per person-rem." ***Yes.*** These figures appear to be in disagreement with the radiation risk coefficients from both BEIR V and BEIR VII. BEIR V (http://www.nap.edu/books/0309039959/html/)
		risk coefficients were 0.0005 fatal cancers per person-rem, and 0.0006 cancer incidence per person-rem. The more recently published BEIR VII report (http://newton.nap.edu/catalog/11340.html and
		Beyea Response, R44: ***With its historical figures, Boeing has demonstrated for me how projected cancer risks have steadily increased with time as more data has been acquired. The latest BEIR VII radiation risk numbers are about a factor of two higher than the 1990 values provided by BEIR V. The difference would be a factor of three, without the factor of 1.5 that BEIR VII added for a DDREF. As I discuss in my report, the latest epidemiological data, which BEIR VII only had time to discuss casually, suggests that BEIR VII's data is too optimistic, particularly in its use of a DDREF. At the time of my original report, I was sure that the DDREF was dead, but in putting the question to Owen Hoffman, I learned that he estimates that, when animal and in vitro cell data are included with the new human epidemiological data, the new epidemiological results will only reduce the DDREF by 25., As a result, in my revised report, I have left BEIR VII's DDREF in place for my lower risk number.*** ***Note that I specifically stated in my report that I was using higher risk coefficients than BEIR VII. I gave results for two values. The lower number equaled BEIR VII without the factor of 1.5 DDREF. The higher number represented an average of the new human epidemiological data and the old. (In the revised report, I put back the DDREF.)***

No.	Section	Comments
	Beyea->	http://newton.nap.edu/execsumm pdf/11340) reported coefficients of 0.00057 fatal cancer per person-rem and 0.00114 cancer incidence per person-rem.
R-45	Beyea Report, Page 30 **Beyea->**	If Dr. Beyea has used the same dose and risk coefficients (discussed above) in his computer modeling as he used in his "back of the envelope" calculations then the hypothetical public exposures and cancers are grossly exaggerated even assuming the releases are correct, which they are not. ***Beyea Response, R45; There is no exaggeration in the cancers calculated. Boeing is relying here on old epidemiological data. Every decade the cancer risk coefficients increase, as more is learned about radiation risks.***
R-46	Beyea Report, Page 40, Table 3-1 **Beyea->**	In Table 3-1, the maximum hypothetical individual thyroid exposure from a release of 10,000 curies is given as 6.18 rem. Using the ICRP 60 risk coefficient for fatal thyroid cancer of 0.000008, the maximum hypothetical individual fatal risk is 0.000049. This is small compared to the U.S. fatal risk of thyroid cancer of 0.0005. ***Beyea Response, R46. I agree the individual's relative risk is small. In my revised report I indicate that the individual risk of excess cancer is 1 in 700 within 4 miles. The individual risk is lower further out. However, the population risk, and hence the social risk, is not trivial.***
R-47	Beyea Report, Page 41, Table 3-4 **Beyea->**	In Table 3-4, the maximum hypothetical individual whole body exposure from a release of 300 curies is given as 7.36 rem. Using the ICRP 60 fatal risk coefficient for all cancers of 0.0005, the maximum hypothetical individual fatal risk is 0.0037. This is small compared to the U.S. fatal cancer risk of 0.23. ***Beyea Response, R47. I agree the individual's relative risk is small. See previous response. However, the population risk (total cancers), and hence the social risk, is not trivial.***

R-48	Beyea Report, Page 54, Table 4-2	Table 4-2 provides the hypothetical cancers from exposure to cesium-137 as function of distance from the SSFL up to 100 km. A similar table was not provided for iodine-131. Inspection of the numbers shows that the larger the annulus modeled (or distance from the site), the larger the number of hypothetical cancers. Thus, even though individual doses would tend to decrease with distance from the SSFL site, the population increases with distance. Therefore, the collective or population dose in person-rem increases without bound. ***Beyea Response, R48a. The statement that the collective (population) dose increases without bound is not correct. The airborne material begins to deplete significantly (exponentially) and the increase in population dose gets rather small after 100 km, primarily because of the population decline. The collective dose is bounded. I show the contribution to person-rem by distance in my revised report. See Table 3-13. I state there: "Another 10% of the person-rem and corresponding health effects were committed between 6.5 and 12 km. About 2/3rds of the total, whole-body person rem was committed within 75 km. More than 80% of the whole-body dose was committed within 100 km."*** **Beyea->** If Dr. Beyea had expanded his analysis to 500, 1,000, or 5,000 km, he would have calculated even more hypothetical cancers. ***Beyea response, R48b: There is not much of an increase at this site in extending the cutoff distance beyond the greater LA area. 20% of the total out to 2,000 km occurs after 100 km. As for beyond, 2,000 km, which I did not consider, almost all of the material has deposited by then. There would be very little increase beyond 2,000 km.*** **Beyea->** This illustrates the fallacy of modeling large populations exposed to very small doses to calculate pubic health effects. ***Beyea response, R48c: It is not a fallacy to model large populations exposed to very small doses. It is a way of measuring the social risk and estimating how much resources responsible parties should pay towards amelioration, say to cancer research, which would help those still alive with any SSFL-related cancer, including any SSFI-related cancers that have not yet been diagnosed.*** **Beyea->** http://www.hps.org/documents/riskassessment.pdf

The HPS states,
"Collective dose (the sum of individual doses in a defined exposed population expressed as person-rem) has been a useful index for quantifying dose in large populations and in comparing the magnitude of exposures from different radiation sources. However, collective dose may aggregate information excessively, for example, a large dose to a small number of people is not equivalent to a small dose to many people, even if the collective doses are the same. Thus, for populations in which almost all individuals are estimated to receive a lifetime dose of less than 10 rem above background, collective dose is a highly speculative and uncertain measure of risk and should not be used for the purpose of estimating population health risks."

Beyea Response, R48d: ***This is a policy statement, not a scientific statement. Ignoring doses below 10-rem above background is socially irresponsible in my personal view. It is also scientifically suspect. We now have a number of studies that show excess cancers down well below 10-rem. Are risks below 10-rem highly speculative, as the HPS statement says? Hardly. I showed one of the new dose response curves in my report, which I attach at the end of this response, located before my citations to the literature. It should be borne in mind that a large and influential segment of the membership of the Health Physics Society make their living working in nuclear facilities. The Health Physics Society is a professional society that tries to advance the welfare of its members. They put out an independent journal, which is one of the major journals in the radiation field, but their policy statements do not rise to the same level of independence maintained by the magazine.***

Beyea->

Beyea Response R48d (continued). ***Note that there was vigorous debate within the Society about setting a calculation cutoff:***

> ***"This position statement, noteworthy not only for its succinctness and directness, was also noteworthy for the rather strong negative reaction it engendered in some of the HPS membership, who felt that the position put forth by the Society was inconsistent with the LNT hypothesis and ALARA." (Boerner and Kathren 2005).***

The latest National Research Council report on the health effects of ionizing radiation (BEIR VII) made headlines with its statement that risks of radiation extended very low. No threshold. I see this as a scientific rebuke to the basis on which the HPS makes its recommendations to ignore doses less than 10-rem.

As I have said repeatedly in this response, cumulative doses measure the total social risk and can be used to assess a financial penalty after an accident. Funds given to cancer research, in proportion to the total predicted cancers, can help the small percentage of (living) persons who lost the dose lottery, even if we cannot identify them individually. In this regard, note that not all cancers from SSFL would have yet been diagnosed.

At the same time, the public must be educated that the individual excess risk is low when individual excess doses are low. If the HPS would spend its time publicizing this distinction, its reputation among the public would be improved. Nuclear critics also have a responsibility for making the distinction. The HPS statement, I presume, was partly stimulated by some nuclear critics who in the past have failed to balance the high-sounding total health effects numbers with the low-sounding individual risk numbers.

Dose response found in the Techa River cohort. Reprinted from (Krestinina et al. 2005).

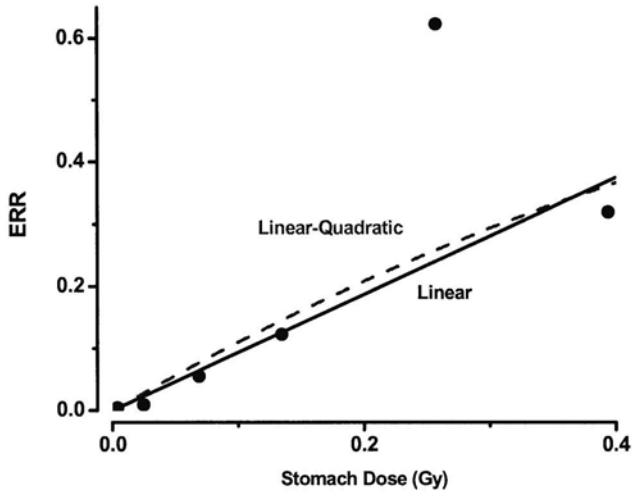

FIG. 1. ETRC solid cancer dose response.

Beyea Citations.

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