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

Report to the Santa Susana Field Laboratory Advisory Panel, a Project of The Tides Center (http://www.ssflpanel.org)*

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Revision History.

Revision 0b. 10/4/2006. This revision was printed prior to final editing in order to make the report available for a meeting of the Santa Susana Field Laboratory Advisory Panel on October 5. As a result, this version likely contains typographical errors. In addition, some of the Tables presenting results for scenarios when all SSFL releases penetrate the inversion layer are not included.

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Summary.

This report provides exposure estimates to assist epidemiologists in deciding on the effectiveness of studies of radiation-induced disease around the Santa Susana Field Laboratory (SSFL). Developing such estimates was a difficult challenge, because the site is extremely complex from the meteorological perspective, there is limited information available about releases of radioactivity in the 1950s and 60s, and some meteorological data is withheld by the plant owner. As a result, the estimates in this report are limited to scoping calculations that carry a wide range of uncertainty, complicating their use for estimation of statistical power. Nevertheless, they represent the current state of knowledge about the accident and its consequences, as reflected in the opinion of experts who have analyzed the event.

Methods: The same historical review techniques were used that the author has relied on for epidemiological studies of traffic pollution and risk assessments at other locations. Bayesian techniques have been used to deal with uncertainties. At SSFL, it has been necessary to apply Bayesian techniques to deal with the possibility of deliberate withholding or destruction of data. Recent radiation epidemiology studies have been reviewed and incorporated into updated risk coefficients. Calculations of transport of radioactivity were guided by the results of studies of the complex meteorology of the LA basin.

Results applicable to epidemiological studies: No validated dispersion model exists to handle the complex meteorology at this mountainous site. Limited data is available about release timing and magnitude. As a result, failure to find an association in an epidemiological study would not be informative, possibly reflecting nothing but a bad exposure model. Probability distributions developed for released radioiodine and radiocesium, taking into account wide differences in expert assessments, appear to follow distributions with maxima near the origin and long tails. Combining the radioactivity source terms with estimates of dispersion, and multiplying by the appropriate dose conversion coefficients, which are also uncertain, leads to individual doses with distributions with even longer tails. Undertaking a major, multi-million dollar, epidemiological study with these dose distributions would amount to a risky gamble. It would be wiser to first undertake measurements of radiocesium in soil at locations around the plant, so as to narrow the great uncertainties which 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 . Given the range of possible releases of radiocesium based on

engineering calculations and experience at other reactors during the 1950s, it should be relatively easy to find the fingerprint of any large releases using helicopters equipped with modern gamma detectors, although it may be necessary to look 5 to 20 km from the facility and even beyond, based on some of the meteorological results. For instance, there is a possibility that most of the releases were carried above the inversion layer remaining aloft as they traveled North away from SSFL. However, the return flow would have passed over populated areas in the Eastern part of the LA basin, where fumigation to the ground would have been possible, causing exposure.

After such soil measurements are carried out, the wisdom of performing a sophisticated epidemiological study could be revisited. In the absence of such a logical progression, it may be possible to use generic meteorological principles, grounded in past tracking of wind patterns in complex California terrain, to identify regions of higher projected dose, which would allow for simpler (and less expensive) ecologic studies to be carried out. This would be similar to, but go beyond, what Morgenstern et al. have done to date and beyond reliance on the exposure contours provided by Cohen et al.

<u>Results for projected health effects</u>: With epidemiological studies at this site so problematic, projected estimates of health effects, even with their great uncertainties, may be the most informative indicator of the impacts of releases of radioactivity at the SSFL. Such estimates involve subjective judgments on the part of analysts, but, as long as the assumptions are acknowledged, and the analysis incorporates a wide range of opinion, modeling calculations appear to represent the best answers that can be expected given the current state of scientific knowledge and the lack of contemporaneous measurements available in the public record. Inhalation of radioiodine and groundshine from deposited radiocesium are predicted to have dominated the exposures, with milk a relatively minor source at this site. To get an estimate of the range of cancers that might have occurred, 20,000 simulations were run using different combinations of assumptions favored by different experts and different values of uncertain parameters. The number of excess cancers averaged over all 20,000 simulations was 260. In 25% of the simulations, there were 8 or less excess cancers. In half of the scenarios, the number of cancers was 50 or more. In 2.5% of them, the number of predicted cancers was 1800 or more. Summarizing the results: the average number of predicted cancers was 260 with a 95%-confidence range of 0 to 1800. The range could be narrowed considerably by making soil measurements in the right places. 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. There is also the possibility that the released radioactivity could have been a contributing factor, as opposed to the major cause, of additional cancers beyond the totals listed above.

Ch. 1. Introduction.

The Sodium Reactor Experiment (SRE) was a pilot, 20,000 kwt, sodium cooled, graphite-moderated reactor. Details of the reactor given before the accident can be found in Parkins (Parkins 1955). The SRE incorporated many of the “feature believed to be desirable in a full-scale central station power plant of this type” (Parkins 1955). Subsequent commercial power reactors were envisioned to produce ten times the thermal power (Starr 1955). The SRE was located at the Santa Susana Field Laboratory (SSFL) in Ventura County, California, on mountains above the greater Los Angeles area. The amount of radioactivity released offsite over the years is controversial, suggesting the possible need for an epidemiological study of the surrounding population.

In this report, three possible exposure estimates are considered for possible use in epidemiological studies of the population surrounding the SSFL:

1) Exposure estimates produced by geographic modeling. Geographic exposure modeling as used in this report refers to the generation of exposure estimates that are specific to each individual. The availability of such estimates, if they are reasonably accurate, allows epidemiologists to go beyond group-level exposure analysis. Geographic modeling starts with an assumed release estimate (source term) that is used as input to a meteorological dispersion model. The output of the meteorological dispersion model includes crop contamination and individualized inhalation dose. Crop contamination is tracked to individuals through a food pathway model.

   The focus for environmental exposure modeling in this report is on the July 1959 accident that occurred in the Sodium Reactor Experiment (SRE).[1] It is generally considered to represent the event or process with the largest single potential for release of

[1] Particularly, Run 14, which began at 0650 hrs on 12 July 1959 and ended at 1120 hrs on 26 July (Thompson 2004b). All of the engineered release pathways for gaseous radioactive waste led to the SRE stack. Photographs show that the stack projected a few feet above the roof of the main SRE building. A picture of the building housing the SRE can be found at, http://www.atsdr.cdc.gov/HAC/PHA/santa/san_p1.html#_1_14. Thompson describes various pathways for release of radioactivity: “One pathway led through a dual-purpose gas system that is described below. The second pathway led through or around the rotatable shield. This shield was penetrated by various plugs. The occurrence of elevated readings of airborne radioactivity in the High Bay during Run 14 demonstrates that the seals around these plugs, and/or at the perimeter of the rotatable shield, were not perfectly effective. The second pathway continued through the large open space of the High Bay. From this space the escaping radioactive material could have reached the atmosphere through the reactor-building ventilation system or, conceivably, around the edges of imperfectly-sealed doors in the building“ (Thompson 2004b).

radioactivity (Thompson 2004b). Other releases from SRE or other facilities can be handled in a manner similar to what is presented in this report.

Because the SSFL SITE is extremely complex from a meteorological perspective, because no model validation has been undertaken, because no onsite meteorological data exists for the period of the supposedly largest release from SSFL, and because no contemporaneous environmental measurements are available, it will be extremely challenging to undertake a successful epidemiological study in the area using exposure modeling methodology designed to give individualized exposure estimates (geographic modeling). If exposure modeling is to be considered in the face of so much missing information, then a "Bayesian" approach (Bedford and Cooke 2001) would seem most appropriate and is adopted here. (See discussion after 3rd item in list.)

2) Exposure estimates defined by residence in specific geographic areas. Even if it is not feasible to generate reasonably accurate estimates of individual exposure, it may be possible to assign to individuals group-level values based on averages over geographic areas. Most of the steps are the same in assigning average exposures as in geographic modeling. Using the methodology of this report, the only difference between the needed calculations would be in how releases were related to exposure categories. In this second method, instead of trying to estimate exposure levels down to the individual residence, an analyst may simply assign by region generic categories, such as high, medium, and low exposure, possibly using expert (subjective) judgment to draw the regional boundaries. If the variance around the true exposure within a region is small, little is lost by going to a group level analysis. On the other hand, if a significant number of persons residing in the "high"-exposure region, actually received a low dose, and vice versa, then a true association with health effects could be masked or a false correlation could be found. Although use of average, group-level values in epidemiological studies can weaken the inferences that can be drawn from an association, or lack of one, between assigned dose and health effects, there may be no other choice, particularly if the dispersion modeling is complex and has not been validated. As discussed in Appendix 2, the terrain around the SSFL is extremely complex, making it difficult to model pollution transport. Nevertheless, the many studies that have been carried out over the years of the wind flow patterns in the Los Angeles Basin provide some general principles that can be used to identify geographic areas of high and low exposure from releases at SSFL. These patterns could be used to establish a limited number of group-level exposure models that

could be tested against health data. In particular, radioactivity released from the SSFL location would have produced distinctly higher inhalation exposures in the western part of the San Fernando Valley compared to the Eastern part. Exposures in Simi Valley might also have been magnified by the terrain. Assigning categorical variables indicative of residence in broad geographic areas at time of release would allow analysts to look for gross differences in disease prevalence caused by radioiodine exposure. This method is particularly appropriate for I-131 exposure at SSFL, because inhalation exposure at this site appears to dominate over milk dose, which should reduce individual variance around average exposure assigned by area. Whether or not doses from SSFL were high enough to make an epidemiological study feasible is another matter. Note that the geographic-area method may not work as well for exposure from long-lived radiocesium, because people change residence over time.

3) <u>Exposure estimates proportional to environmental measurements of long-lived radiocesium at residence or extrapolated to residence</u>. If exposure modeling is deemed infeasible for use in a hypothetical epidemiological study at SSFL, the possibility of making new measurements to find footprints of long-lived, radiocesium released from SSFL should be considered. These measurements could be used to obtain exposure surrogates for use in an epidemiological study or could be used to "calibrate" a geographic model (Beyea et al. 2006). Based on the report that is to follow, 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. Given the range of possible releases of radiocesium based on engineering calculations and experience at other reactors during the 1950s that are discussed in Chapter 2, it should be relatively easy to find the fingerprint of any large releases using helicopters equipped with modern gamma detectors, although it may be necessary to look 5 to 20 km from the facility and even beyond, based on some of the meteorological results. For instance, there is a possibility that most of the releases were carried above the inversion layer remaining aloft as they traveled North away from SSFL. However, the return flow would have passed over populated areas in the Eastern part of the LA basin, where fumigation to the ground would have been possible, causing exposures. Another interesting place to take measurements of residual Cs-137 would be

in undisturbed locations, such as attics, crawl spaces, and underutilized storage spaces. Such spaces may not be too prevalent in the study area, however. Although a number of studies indicate that Cs-137 at fallout levels of concentration can be detected in such situations (Cizdziel et al. 1999; Ilacqua et al. 2003; Lioy et al. 2002), it has not been possible for this report to work out the detailed feasibility of the approach for the SSFL site.

Were funds unlimited, all three of the above approaches would be undertaken, assuming a decision was reached to proceed with an epidemiological study. Presumably, a recommendation to proceed could be made for a variety of reasons, including findings that, 1) a "power" analysis suggested there was a reasonable chance that a statistically significant effect could be found, 2) no power analysis was deemed sufficiently reliable, or 3) an epidemiological study was deemed necessary for public reassurance. Hopefully, the information in this report will prove helpful in deciding on a recommendation.

<u>Bayesian approach to exposure modeling</u>: In a Bayesian approach, as much as possible, numbers and parameters used in modeling doses are treated as "random variables" with an uncertainty distribution (Bedford and Cooke 2001).[2] To estimate the shape of uncertainty distributions for parameters, one starts from generic estimates gleaned from the literature (Nair et al. 2000; USEPA 1996). These kinds of generic estimates are partially subjective, since they require judgment on the part of the analyst in selecting and weighting data.[3] In some cases, sufficient information is available to allow the distributions to be based on a range of expert views. An example is the distribution developed by the US Nuclear Regulatory Commission for "deposition velocity" (USNRC 1995), which is a key parameter in exposure modeling of airborne releases.

An analysis that admittedly contains subjective elements cannot be expected to resolve controversy in contested risk assessments (Slovic 1999). In fact, a Bayesian dose assessment is likely to be perceived as biased by one or more set of stakeholders. However, if performed *prior* to an epidemiological analysis, a Bayesian dose assessment can still be very useful to an

[2] For this report, no distinction is made between variability and uncertainty. Uncertainty, which is distinguished from variability in that it could be reduced with more information, is the major problem at SSFL, not variability. Still, variability would have to be addressed, once the approach to uncertainty was resolved. At that point, uncertainty and variability would be kept distinct (Hoffman and Hammonds 1994).
[3] "The principal difference between Bayesian and maximum likelihood methods is that the Bayesian method requires the specification of a prior distribution…One of the principal objections from non-Bayesians to the Bayesian paradigm is that the choice of a prior is subjective. In their opinion this means that it is impossible to make objectively sustainable inferences. In practice, many Bayesians try to find a prior over which there is consensus" (Bedford and Cooke 2001), Page 70.

epidemiologist, because a dose analyst, even if personally biased, is not capable of manufacturing an association between estimated dose and health effects without access to the health data. Only, if an analyst could change the subjective parts of the dose assessment with knowledge of the initial results of the fits to health data could bias lead to the manufacturing of a false association with disease. A greater concern to an epidemiologist, based on the data limitations that are analyzed in this report, may be the possibility that a dose analyst is capable of "manufacturing" the absence of an association due to misclassification of dose, thereby producing a false negative result. Thus, an epidemiologist cannot avoid the responsibility of examining the vested interests of any dose analyst relied upon.

If there should turn out to be contested Bayesian dose models at SSFL, the epidemiologist can, in principle and within funding limitations, run all of them against the health data. In the SSFL case, the alternate dose model from the plant owner is likely to be one with releases so small that no health effects could possibly appear, so it is already incorporated in the null hypothesis.

A key step in Bayesian analysis is to use available, site-specific information to "update" parameter uncertainty distributions before making exposure (dose) estimates (Mallick et al. 2002). For instance, the likelihood of low values in the distribution for particle deposition velocity might be reduced in magnitude, based on measurements of offsite or onsite radioactivity on the ground.

In the SSFL case, there are no reliable and systematic field measurements to use for updating, with the possible exception of relatively recent measurements of long-lived radiocesium, which might set some limits on the release magnitude of radiocesium. See the recently released UCLA report on SSFL for a useful compilation of these recent measurements (UCLA 2006). In contrast, there is historical information available about the actions of the plant operators during and after the accident, which, in the context of the times, could tell us something about the likelihood that certain parameters fell in certain ranges. For instance, evidence of a cover-up would suggest that parameter values provided by the plant operators are likely to come, not from the center of generic parameter distributions, but from the side that leads to the most favorable results for the operators. Similarly, numbers not provided at all are likely to have true values that would be less favorable to the operator than average or default values. To perform a quantitative updating of a parameter distribution using Bayes' theorem and historical information about the SRE, it would be necessary to subjectively estimate the likelihood that the operator would have presented biased information for a parameter as a function of the parameter's value. Evidence of a cover-up would be especially relevant to the task.

How strong would the motivation have been for a cover-up? SRE developers, such as Chauncey Starr, promoted this reactor type as one that would permit “the plant configuration to be easily arranged to contain radioactivity under all circumstances” (Starr 1955). Such strong claims would be dashed, were a serious release to occur at the SRE and become public.

As a result of the possibility of biased information, it has been necessary to explore in some detail the actions of the plant operators and what they must have known at the time of the accident (see Appendix 1). Without such a review, it would be difficult to know how to weight information that came from the plant operators and how to fill in gaps about missing information that normally would be provided by the plant operator as part of the engineering and safety record.

Updating modeling parameters based on judgments of human actions is not standard in Bayesian analysis, although perfectly consistent with the methodology. There seems little alternative at SSFL to taking this step, given the restrictions on information that have been imposed on this site, as will be discussed later in the report. At a minimum, a careful assessment of the state of scientific knowledge about accident consequences in 1959, along with an assessment of what the operators must have known at the time, will be helpful in deciding on what site-specific pieces of information should be included or excluded from the analysis.

Ch. 2. Release estimates for the SRE.

Different analysts have reached different conclusions about the magnitude of the 1959 SRE release. The closest estimate we have to an official, government release estimate is the conclusion of the Agency for Toxic Substances and Disease Registry (ATSDR) that "Only the noble gas fission products made it to the helium cover gas and were held for decay before being vented to the atmosphere" (ATSDR 1999).

Thus, according to ATSDR, the release of radioiodine and radiocesium was zero. However, ATSDR simply cited studies by the plant operator (Hart 1962b). Trusting the plant owner and SSFL contractors for estimates of release behavior does not seem a wise approach given the history, as discussed below, of withholding information at this and other facilities. It would be unreasonable to expect that the company's official account of the accident would have been free from management censorship. 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. See Thompson for a discussion of the actual damage and the mention of increased stack radioactivity (Thompson 2004b). Thus, Atomics International lied to the public. The obvious next question is, "What other information was kept secret, either by the operators running the reactor at the time of the event or by management?" No contemporaneous measurements of offsite ground activity have been reported. 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. Instead, we are told that the automatic filters were not working (Thompson 2004b). Yet, measurements made after decommissioning of the amount of surface contamination before and after the filters (Carroll et al. 1983) imply that there was a filter in place.

Even today, information such as wind frequency data for the site is withheld from the general public and independent analysts. We know this from Freedom-of-Information requests made by Thompson[4] (Thompson 2005) and requests made by an engineer working for Ventura County.[5] The withholding of full meteorological data is extremely serious. For instance, it limits this report to scoping calculations, using summary wind data. Wind speed frequencies by atmospheric stability class, which are normally used in risk assessments, are not available. Even those who have been given access to this joint frequency data, such as a group at UCLA (UCLA 2006), have not, apparently, been given data on the frequency of calms,[6] which can dominate exposures. Handling calms is one of the most difficult and most important decisions to make in dispersion modeling, because exposures vary inversely with wind speed. Many of the quantitative results in this report are conditional upon the missing data on calms being quantitatively unimportant.

At least with the wind data, we know explicitly that data has been withheld. In other cases, it is not so clear. Deciding on what calculations to attempt in the face of possible cover-ups is difficult for an analyst, which is one of the reasons, perhaps, that cover-ups are so tempting. It is surprisingly rare for assessments of accident releases and risk assessments to speak of cover-ups. When data is found to have been falsified or tampered with, risk analysts will not use it, but rarely will they use the discovery to call into question other data not specifically proved to be false. Dishonesty and cover-ups are difficult subjects for engineers and scientists to handle, falling outside their training. It is considered impolite, pejorative, and possibly libelous to raise the question of deliberate withholding of data or incompetence. Yet, it is human nature to cover up our mistakes. It is so common that we have myths about those who do not. Recall the story of George Washington voluntarily confessing to chopping down a cherry tree. It is also human to avoid digging too deeply, when we are afraid the results might reflect badly on us. An analysis of the SSFL releases would be incomplete, if if did not deal with possible withholding of information or failure to pursue important lines of evidence. Bear in mind that evidence of hiding mistakes does not mean that everything was kept secret or "spun." In fact, evidence of a cover-up or failure to pursue evidence may not be very informative about

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[4] A request for the full meteorological data set was made to, and turned down by, a UCLA group, because after making inquiries they were told that Boeing claimed the data was confidential (Thompson 2004a). Since the full meteorological data set had been given to ATSDR,[4] Gordon Thompson filed a Freedom of Information request with ATSDR. However, ATSDR responded that they could find no documents pertaining to the request (CDC/ATSDR 2004). Thompson also made a FOIA-request to DOE. DOE responded similarly (NNSA 2003).

[5] "Boeing declined to provide me with any information about meteorological data collected at its site. Sincerely, Karl E. Krause, Manager, Engineering Division" See appendix 2. Boeing has made the data available to ATSDR and a group at UCLA. There is no evidence that either group has made a critical analysis of the met data to check such critical factors as the low wind speed data.

[6] The summary wind rose data in the report have no data for wind speeds less than 1 mph.

the magnitude of the actions, because of the tendency in our species to hide or play down even the littlest mistake. In any case, not all analysts agree with ATSDR on releases, as will be discussed below. Before discussing engineering approaches to estimating possible releases at SSFL, it is useful to consider what the experience at other comparable reactors can tell us about releases during accidents at graphite-moderated reactors in a comparable time period. Rather than trust information provided by those with a vested interest in playing down any release, we might want to rely on an analogy with other situations.

For instance, post-accident ground measurements led to an estimate for the Windscale accident of a release of 20,000 Ci of I-131 and 600 Ci of Cs-137. The Windscale accident occurred in the UK at a graphite-moderate reactor two years earlier than the SRE accident. Radioiodine was measured on the ground at Windscale and in milk, leading to restrictions on milk consumption.

As the only other relevant sample point in the universe of graphite-moderated reactors that experienced significant damage to fuel elements without the protection of a secondary containment system,[7] the Windscale release magnitude is appropriate for use in scoping calculations. The 1966 accident at the Fermi I reactor, while useful for informing engineering calculations of releases at SRE (Lochbaum 2005), is not relevant to scaling releases at SRE to the environment, because Fermi I had a secondary containment system (Lochbaum 2005), a safety feature possessed neither by SRE nor Windscale.

One option for using the Windscale Accident as a predictor of releases at SRE is to use the Windscale releases directly. A second option would be to translate the Windscale experience to I-131 emissions at SRE by scaling the Windscale release by the relative thermal powers of the two reactors (9-to-1), while simultaneously adjusting for the retention of radioactivity on the stack filters at Windscale. According to Atomics International, the SRE filters were bypassed during the release (Thompson 2004b),[8] Taking them at their word at this point, and noting that the thermal power at Windscale was 9 times the power at SRE (UKAEA Undated) and the Windscale filters held back 50% of the radioactivity (Taylor 1981), the net scale factor would be a factor of 4.5, producing a net SRE release estimate of 4,400-Ci of radioiodine. If, on the other

[7] The SRE core sustained 33% damage (Thompson 2004b), while the Windscale pile sustained damage to 9.3% of the fuel elements (UKAEA Undated), (Wikipedia 2005). Although the Chernobyl reactor also had a graphite moderator, it is not as relevant to the earlier SRE. In any case, the release fraction from Chernobyl was higher than the release fraction from Windscale: 50 - 60 % of the radioiodine in the reactor core at the time of the accident, and about 20 - 40 % of the radiocesium http://dwb.unl.edu/Teacher/NSF/C03/C03Links/www.iaea.or.at/worldatom/thisweek/preview/chernobyl/conclsn9.html.

[8] The SRE did not have automatic filters working in July of 1959. They did have the option in the control room to manually route the stack releases through filters (Thompson 2004b).

hand, filters were in place, and if they behaved as the filters at Windscale, the release estimate would have been expected to drop to 2,200 Ci of Radioiodine.

If one also accounts for the fact that, at SRE, 33% of the fuel was damaged (Thompson 2004b), while only 9.2% was damaged at Windscale (UKAEA Undated; Wikipedia 2005), the ratio of damaged fuel between the two accidents was 3.5. Thus, one would expect 3.5 X 4,400-Ci of I-131 to have been released, or 15,600-Ci. This is close enough to 20,000-Ci that the two estimates can be combined.

A more refined scaling estimate could be made by accounting for different power histories in the two reactors and, therefore, different losses of I-131 through radioactive decay. If one takes this approach, and accepts the official AI data on average power, the release from SSFL would be expected to be smaller. See Table 2-1, which compares the average reported power for Run 14, the time when most releases are thought to have occurred, to all previous runs.

Table 2-1. Reported summary of SRE power operation (Hart 1962a). Note inconsistency in time interval for runs 2 and 4.

Run	Time Interval	Actual operating days	Average thermal power (Mw)	Total irradiation during run (Mwd)	Total irradiation accumulated since startup (Mwd)
1	7/9/57-7/15/57				
2	7/25/57 - 7/26/57	5.7	3.93	22.6	22.6
3	11/7/57 - 11/29/57	12.6	6.2	78.2	100.8
4	5/21/58-5/28/58	13.3	8.73	116.2	217
5	7/18/58 - 5/28/58	11.5	17.7	203.8	420
6	8/8/58 -9/1/58	22	17.9	394	814.8
7	9/8/58 -9/25/58	17.2	17.8	306	1120.8
8	11/29/58 - 1/29/59	37	16.15	597.8	1718.6
9	2/14/59-2/26/59	11.5	11	126.5	1845.1
10	3/6/59-3/7/59	0.6	5.3	3.1	1848.2
11	3/13/59- 4/6/59	23.6	12.4	293.5	2141
12	5/14/59-5/24/59	9.7	15.9	154.3	2295.4
13	5/27/59 - 6/3/59	6.6	17.3	114.3	2409.7
14	712/59 - 7/26/59	14.2	1.1	16.1	2425.8

The supposed average power during Run 14 was 4- to 18-times lower than the average power generated during all previous runs. True, the average power should have been lower, because the operators were having trouble with the reactor. In fact, they repeatedly failed to get to high power. They kept trying in such a rapid manner, without allowing time to figure out what might go wrong, that it surprised one later commentator (Thompson and Beckerley 1964), who wrote,

> "During that time so many difficulties were encountered that, at least, in retrospect, it is quite clear that the reactor should have been shut down and the problems sovled properly. Continuing to run in the face of a known Tetralin leak, repeated scrams, equipment failures, rising radioactivity release, and unexplained transient effects is difficult to justify."

Perhaps, the operators' urgency was fueled by the desire to send real electric power to the grid, as part of a planned demonstration. Was it reckless behavior on the part of the operators? Hard to say otherwise. Would the operators be criticized later by management for their actions? Quite possibly. Would they report every mistake they made to management? Not likely. Furthermore, it is not plausible that the operators would have spent 14 days trying to get the power up beyond a small fraction of the rated power level. If they had not been able to get a reasonable power level after many days, it should have been clear that something was damaged and would have to be fixed. A more likely explanation is that the operators were able, from time to time, to get to high power, which fueled their optimism.

The only permanent evidence of the reactor power would have been a strip chart that recorded the output of a neutron monitor. The scale of the monitor was adjustable over seven decades (Starr 1955), so the scale of the chart was probably noted by hand on the strips, if at all. Some sections of the output strips were analyzed in subsequent reports, so were not destroyed or altered immediately. However, the operators could have reported an overall false average power level, either directly or by changing the scale on the strips. Alternatively, they could have removed certain sections of the strip chart. All of this would have been done to prevent subsequent criticism for reckless behavior. After all, a major blow had been delivered to the dreams of the company and blame would have been expected from management.

The original charts are not publicly available. Whether they were available for the Boeing litigation[9] is unknown. At this point, given the suspicious claim of reduced power for 14 days, some likelihood must be assigned to the possibility that the true average power was higher than 1 Mw, which would mean a higher inventory of I-131. Note that the impact of the average power during Run 14 on radiocesium inventories is quite small, because of the long half lives of Cs-137 and Cs-134.

The bottom line, when it comes to using releases from the Windscale Accident as a surrogate, is that one can get numbers over a wide range, depending on how one approaches the

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[9] Laurence O'Connor et al. v. Boeing North America, Inc., Case 2: 97-CV- 01554, US District Court, Central District of California.

calculation. Therefore, we turn to site-specific engineering analyses for further guidance. Nevertheless, we have used the Windscale release as our basic source term in Appendix 2 for making quantitative calculations at SSFL, obtaining dose estimates for other releases by scaling to the curies estimated to have been released in site-specific analyses.

Site-specific source term for releases of I-131. In a report from the Institute for Resource and Security Studies (IRSS) prepared for the Santa Susana Field Laboratory Advisory Panel, Gordon Thompson estimated for radioiodine, 1,100- and/or 2,500-Ci as possible upper limit releases (Thompson 2004b), Table VI.5-2. Thompson made these estimates assuming the average power for Run 14 given by Atomics International was correct. Under this assumption, he obtained the estimates shown in Table 2-2 for the I-131 inventory in the reactor for different time periods. Note that by the end of the first two days, most of the I-131 is residual radioactivity left over from earlier runs – radioactivity that has not fully decayed.

Table 2-2. Reproduced from Table VI.1-3 of (Thompson 2004b).
"Estimated I-131 Inventory in SRE I Reactor Core During Run 14 in July 1959"

Source of I-131	I-131 Inventory (Ci) at End of First 2 Days of Run 14	I-131 Inventory (Ci) at End of Run 14
From Run 11	56	20
From Run 12	2,910	1,030
From Run 13	5,750	2,040
From Run 14	4,570	20,370
Total	13,290	23,460

At end of first two days, the percentage of I-131 from earlier runs is 66%, but only 13% at the end of Run 14, twelve days later. Thus, depending on the timing of the release, the past I-131 makes a different relative contribution. For instance, to get a 2-fold increase in inventory at the end of the first two days, the average power during Run 14 would have had to have been 4 times the listed value, whereas only approximately twice the average power would be needed to double the I-131 inventory at the end of the 14 days. To get a 3-fold increase at the end of the first two days, one needs a 6 times higher power. To get a 4-fold increase, one needs approximately an 8-fold increase in average power, which brings the average power level in range of levels reached when the reactor was not having so many problems. Given the fact that the reactor operators were having considerable difficulty in getting high power, an 8-fold increase is likely an upper limit on average power. This means a factor of 4 increase in inventory in the first two days is probably also an upper limit. When considering, later in this report, the possibility that the average power reported by AI was incorrect, this factor of four increase has been taken as an upper limit, on the assumption that most of the release and "reckless" behavior probably occurred in the first two days.[10] Other scenarios are possible. For instance, perhaps, the operators were able to get the reactor up to near full power after the first few days and kept running it at high levels, continuously releasing radioiodine, later hiding the fact that they acted so recklessly, Thus, other analysts might take a different view than is taken here and would have to scale the results presented in this report accordingly.

Gordon Thompson is not the only person to have made independent, site-specific estimates of releases of radioiodine. For example, David Lochbaum, in a report also prepared for the Santa Susana Field Laboratory Advisory Panel, has argued that releases of Iodine-131 from Run 14 of SRE ranged from 3 to 30% of the inventory, which amounts to a range of 600 to 6,000 Ci, using Thompson's core inventory figure of 20,000 Ci (See Table 2-2).

In addition to these public estimates by Thompson and Lochbaum, release estimates were also prepared as part of litigation against Boeing at SSFL.[11] According to news reports, Plaintiffs' experts argued for a release of iodine that was 15 to 260 times as much as was emitted in the Three Mile Island accident.[12] These relative numbers imply an absolute release at SRE of 200 to 4000 Curies, since 15-curies was estimated to have been released at TMI (Beyea 1984). Defendants' experts no doubt argued for a much smaller release, apparently of the order of 1 Ci of radioiodine, which is treated for this report as essentially a zero release estimate. None of the reports filed by any of these experts in litigation, whether on behalf of plaintiffs or on behalf of defendants, have been made public and, unfortunately, will probably never be made public, now

[10] The first two days are particularly important for another reason. The ratio of I-133 in newly produced fuel is particularly high.

[11] Laurence O'Connor et al. v. Boeing North America, Inc., Case 2: 97-CV- 01554, US District Court, Central District of California.

12 Ventura County Star, February 26, 2004, By Roberta Freeman as posted on http://www.cappellonoel.com/news/rocketdyne/suitclaims.cfm According to the Los Angeles Times, "Arjun Makhijani, a nuclear engineer and president of the Institute for Energy and Environmental Research in Takoma Park, Md., and Bernd Franke, an expert on nuclear contamination and scientific director of the institute in Heidelberg, Germany, made the conclusions." Los Angeles Times, February 24, 2004, By Amanda Covarrubias, as posted on http://www.cappellonoel.com/news/rocketdyne/effectsoflab.cfm.

that a settlement has been reached in the litigation.[13] Settlement usually implies sealed expert reports, which means that a great deal of valuable research will be lost to public view.

The various estimates discussed above, which range from essentially zero to 6,000, along with the possibility that the true average power was greater than 1 Mw, provide the basis for generating an uncertainty distribution for the parameter, "release magnitude." Consistent with our Bayesian approach, each release magnitude needs to be assigned a prior likelihood number, based on professional (and hence subjective) judgment.

The graphs of source-term likelihoods to be presented in this section do not represent true probabilities that have been measured or computed based on validated models. Instead, they are meant to indicate our current state of knowledge of releases, as reflected in the views of experts who have studied evidence about the releases. There is a difference between truth and our state of knowledge of the truth. These likelihood distributions can change drastically, if important new evidence is discovered, for example, new evidence on cesium ground contamination across the LA region. They can change dramatically, if a new expert is brought into the set under consideration. If the resulting graph is broad, it indicates that there is considerable uncertainty about the releases among experts.

To distinguish between these Bayesian calculations and probabilities determined from repeated measurements of similar events, it is standard to use the term, "likelihood," instead of "probability." An alternate term is "inferred probability."

Why not stop at listing the experts and their divergent views, rather than trying to compute inferred probability curves? The advantage of a likelihood distribution is that it allows one to combine uncertainty in other parts of the risk and consequence calculations. All the uncertainties can't go in one direction matching one particular preconception. Combining uncertainties reins in the degree of speculation that is plausible.

Why not try to find the perfect expert who knows the truth? It is a hopeless approach to try to look for the perfect expert, discarding the views of those who have some weakness, limitation, or bias in their analysis. In fact, all expert opinions and reports have weaknesses and limitations. All experts have biases based on their career experiences, which amount to no more than a sample of one. Anyone who has ever been involved in a hotly contested legal battle knows that flaws can be found in every published article on which experts rely, let alone in an expert's report.

[13] The fact that a settlement has been reached does not provide any obvious information that can be used in Bayesian updating, particularly because the amount of the settlement has not been released. Furthermore, the case was about chemicals, not just radiation. A history of the case through August 10, 2005 can be found in (Tevrzian 2005).

Weaknesses or flaws in some parts of an expert's arguments do not necessarily mean that the expert's conclusions are wrong, but such evidence always provides a tempting excuse to discard those whose views run counter to our preconceptions. To be objective, we must guard against this natural tendency to judge experts who agree with our preconceptions by their strengths and to judge those who do not by their weaknesses. Otherwise, it becomes too easy to ignore contrary opinions, leading to strong and sometimes bitter divergence of views.

As an alternative to completely discarding expert views, it is preferable to assign weights to their views based on subjective assessments of their relative reliabilities. What are the advantages? First, assigning weights forces one to pay more attention to views opposite to one's own. Second, the weights can be changed easily, allowing other relative assessments to be plugged into the calculations. Third, and perhaps most important, the process takes into account the sociological learnings about expert knowledge, namely that experts tend to underestimate the probabilities at the low ends of the curve (Cooke 1991). By combining the views of experts, there is a better chance of not excluding the extremes. Finally, weighting allows us to take into account the possibility of cover-ups in a balanced, unemotional way.

How would distributions of release estimates be used in an epidemiological study? Each source term would be used as input to a model, generating dose estimates for each study subject or geographic area, which in turn would be regressed against health data. The corresponding strengths of association (regression slopes) would be averaged using the assigned, source-term likelihoods as weights. Confidence limits would also be adjusted to account for the source-term likelihoods. Possibly, an attempt would be made to correct the association strength for measurement error (Carroll et al. 1998; Kerber et al. 1993; Mallick et al. 2002; Thomas et al. 1993). As discussed later in this report, the p-values for the association might not change at all, if no other modeling parameters varied with release magnitude.

How might the prior likelihood values be updated when operator actions are taken into account? It is first necessary to make an assessment of those actions. According to Thompson's review, routine environmental monitoring reported on and around the SSFL site during 1959 found no evidence of a release from the SRE reactor. "A special environmental survey around the SRE site was performed on 5 June 1959, subsequent to the SRE wash cell incident of 4 June 1959." No special survey was reported during and after Run 14 (Thompson 2004b). The non-existence of a special survey in the public record during and after Run 14 is suspicious.

The failure to make detailed contemporaneous environmental measurements of radioiodine contamination, or the decision to keep private any measurements that were made, must be interpreted in the context of the Windscale accident. If a radioiodine release were to happen at an Atomics International site, the reputation of the AI reactor type would have been ruined. As will be discussed later, it is highly unlikely that the SSFL management could have been unaware of the Windscale events and the release there of radioiodine. If there were no release during the SRE of sufficient magnitude to concern anyone, why didn't the operators present groundshine measurements that would have demonstrated the absence of a significant release? Perhaps, management was concerned that the measurement of radioiodine from weapons tests would have confused the situation. Perhaps so; perhaps not. Why were there no reports of the amount of radioactivity caught on filters? The argument that the filters were not running automatically sounds hollow, given the reduced radioactivity found from smears taken beyond the filter location during decommissioning (Carroll et al. 1983). As a result of these considerations, any likelihood distribution developed for release estimates that contains zero should be updated in a Bayesian analysis to account for the non-zero probability that the operators would have suppressed information about a finite release to protect the reputation of their reactor brand. Of course, estimates by the plant owners are not the only ones subject to updating. Attorneys may have "shopped" for experts, with plaintiffs only presenting to the court the highest release estimates made by credible experts and defendants only presenting the lowest. (Credible experts in this context are defined as those likely to survive a scientific challenge before the judge under the so-called Daubert rules (Beyea and Berger 2001). Although defendants usually don't have to defend against a Daubert challenge, they do need to impress a judge, if the criticisms against plaintiffs are to have any traction.)

Because the expert reports from the Boeing litigation are not available for review, which would allow the various arguments to be critically assessed, it is necessary to give the two sides equal weight. Were defendants merely to have accepted AI arguments for a negligible release, as did ATSDR, the likelihood of their estimate would also be a candidate for updating. However, it is unlikely that Boeing's attorneys would have stopped with such a limited analysis.

Based on the information available for the various estimators, an initial likelihood distribution was assigned to each of the analysts, as listed in Table 2-3. A description of the updated distribution is also included in the Table. The possibility of an increase in the iodine inventory, included as part of the updated distributions, was handled by assigning likelihood numbers to larger inventories. A 40% likelihood was assigned to the average power of 1 Mw as reported by AI, which generated the radioiodine inventories shown previously in Table 2-2; a 40% likelihood that the inventory was twice as high; a 16% likelihood it was 3-times as high; and a 4% likelihood that it was 4-times as high. As shown in subsequent Tables, the net effect of including these likelihoods is to double the magnitude of the net distribution for the radioiodine release. The likelihood numbers represent a balance between various possible scenarios, with a 4-fold increase in inventory taken as a practical upper limit. The primary goal in assigning likelihood weights in this report is to give epidemiologists the best information that can be deduced for use in power calculations. The same techniques have been followed that the author has used for exposure assessments in non-controversial situations (Beyea et al. 2006). A secondary goal is to give exposed populations some idea of the likely range in health effects predicted based on our current state of scientific knowledge. If new information comes to light at any time, the calculations can be redone by changing values in spreadsheets. Also, other analysts are free to pick other likelihood values, and the consequences computed in this report can be rerun accordingly. Alternatively, a rough scaling can be undertaken.

As stated earlier, the impact on the radiocesium source term of misstatement of the average power during Run 14 is negligible, because the cesium inventory was largely determined by earlier runs, where there would not have been an incentive for the operators to cover up their mistakes.

Table 2-3. First illustrative likelihood distribution for curies of I-131 released in July 1959 at SRE based on existing release estimates only (Illustrative Radioiodine Source Term I)

Magnitude (Ci of I-131)	Origin of estimate	Prior weight[a)]	Assumed initial distribution shape	Updated weight[b)]	Updated distribution shape
0	ATSDR estimate (ATSDR 1999), based uncritically on (Hart 1962a)	1	Point mass at origin	0.3	No change
<=1	Defendants' experts in Boeing litigation	1	Point mass at origin	1	" "
1100 to 3600	Two estimates by Thompson, which are combined into one curve (Thompson 2004b),	1	Uniform distribution to 1100 Ci, with step downward from 1100 to 3600	1	Maximum of distribution varied by inventory distribution (see text)
200 to 4000	Plaintiffs' experts in Boeing litigation	1	Uniform distribution	1	" "
600 to 6000	(Lochbaum 2005)	1	Triangular distribution with peak at 3000	1	" "

a) "Non-informative" priors
b) See text

Table 2-4 shows how the combined source term distribution changes with different assumptions.

Table 2-4. Illustrative Radioiodine Source Term I. Sensitivity of Curies released to different assumptions. No hypothetical middle-of-road experts included.				
Likelihood percentage	Before varying inventory distribution	After varying inventory distribution	Removing point masses at zero	Removing highest distribution
2.5	0	0	180	0
5	0	0	350	0
median	1150	1850	3200	1100
Mean	1500	2800	4000	1990
95	4000	9,350	10,250	7100
97.5	4600	10,900	11,700	8600

The above approach in Illustrative Source Term # 1 can be criticized for leaving out middle-of-the road views. The analysts who have studied the accident have been selected non-randomly. It is reasonable to expect that there may be experts in the world who would come in with different release estimates, were they asked, somewhere in between the values calculated by experts for plaintiffs, nuclear critics, and defendants in the Boeing litigation. In Illustrative Source Term II, shown in Table 2-5, hypothetical experts are added to the mix. For instance, the ATSDR might have commissioned an expert study. The National Research Council might have been funded by Congress to review the situation. Possibly, an academic group might have solicited and received a grant. Not all such additional, hypothetical assessments would necessarily come out below Plaintiffs' experts, since new information and approaches might arise, but certainly some of them would. This second source term is included to check whether the addition of middle-of-the road views would make any major change in the conclusions of this report. Fortunately, it does not, although adding the hypothetical experts listed in Table 2-5 may well give a more realistic shape to the source-term distribution.

Table 2-5. Second illustrative likelihood distribution for curies of I-131 released in July 1959 at SRE based on existing release estimates with addition of hypothetical middle-of-road experts (Illustrative Radioiodine Source Term II)					
Magnitude (Ci of I-131)	Origin of estimate	Prior weight	Assumed distribution shape	Updated weight	Updated distribution shape
Various	See Table 2-3 for the various 5 estimates	5	Various (See Table 2-3)	4.3	Various
0 to 600	Hypothetical Expert I	1	Uniform distribution	1	Maximum value varied by inventory distribution
100 to 1000	Hypothetical Expert 2,	1	" "	1	" "
0 to 1700	Hypothetical expert 3	1	" "	1	" "

Table 2-6 shows the resulting parameters of the distribution obtained from combining the individual distributions in Table 2-5. Table 2-7 compares how the parameters change with and without the hypothetical middle-of-the-road experts added.

Table 2-6. Illustrative Radioiodine Source Term II. Sensitivity of Curies released to different assumptions. Includes addition of hypothetical, middle-of-road experts				
Likelihood percentage	Before varying inventory distribution	After varying inventory distribution	Removing point masses at zero	Removing highest distribution
2.5	0	0	50	0
5	0	0	100	0
Median	600	1050	1500	750
Mean	1130	2100	2600	1550
95	3650	7800	8400	6200
97.5	4200	9850	10,300	7450

Table 2-7. Comparison of Illustrative Radioiodine Source Terms I & II				
Likelihood percentile	Source Term I before varying inventory	Source Term II before varying inventory	Source Term I after varying inventory	Source Term II after varying inventory
2.5	0	0	0	0
5	0	0	0	0
Median	1150	600	1850	1050
Mean	1500	1130	2800	2100
95	4000	3650	9,350	7800
97.5	4600	4200	10,900	9850

Figure 2-1 shows the actual distributions to which Tables 2-4 and 2-6 refer. In looking at the source-term curves, with their maxima at low releases, it looks like the low-release experts have been given undue weight. However, when one looks at a graph that governs health effects, such as Figure 2-2, which shows dose multiplied by release, it can be seen that those with higher estimates make major contributions to the distribution. Note that the sum of the values in Figure 2-2 equals the average dose for each curve, respectively.

Note that the curves have a jerky appearance due to approximations. The updated curve is approximately exponential (linear on a log scale), which may well reflect a limiting trend when adding distributions from multiple experts who are forming opinions in the face of extremely limited information.

Figure 2-1. The vertical axis has been truncated. The likelihood for the zero value in the original distribution rises to 0.22.

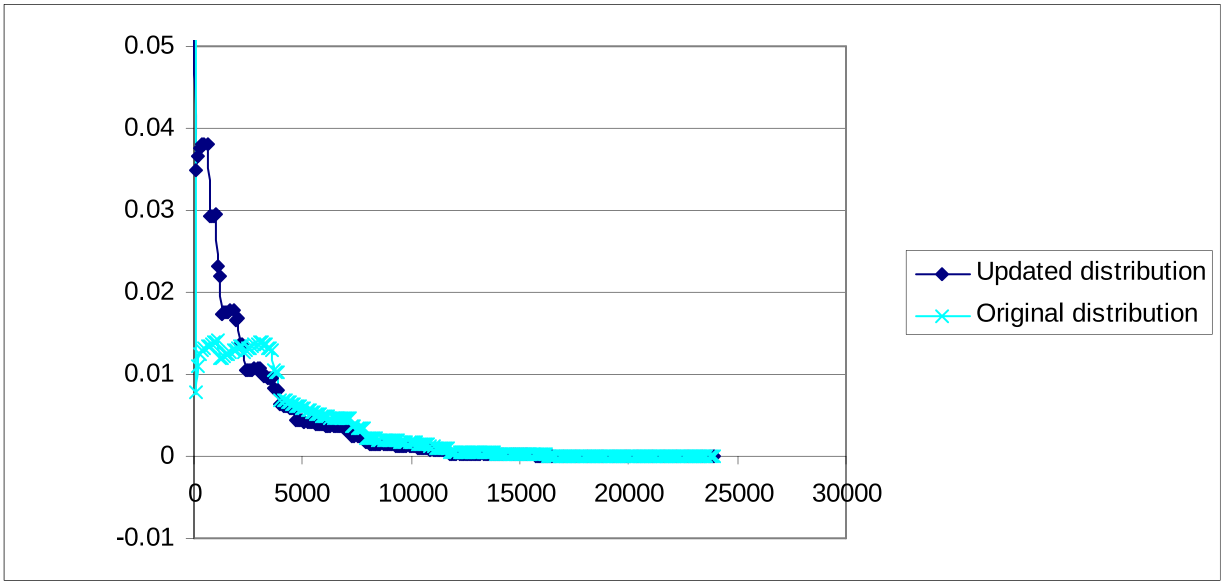
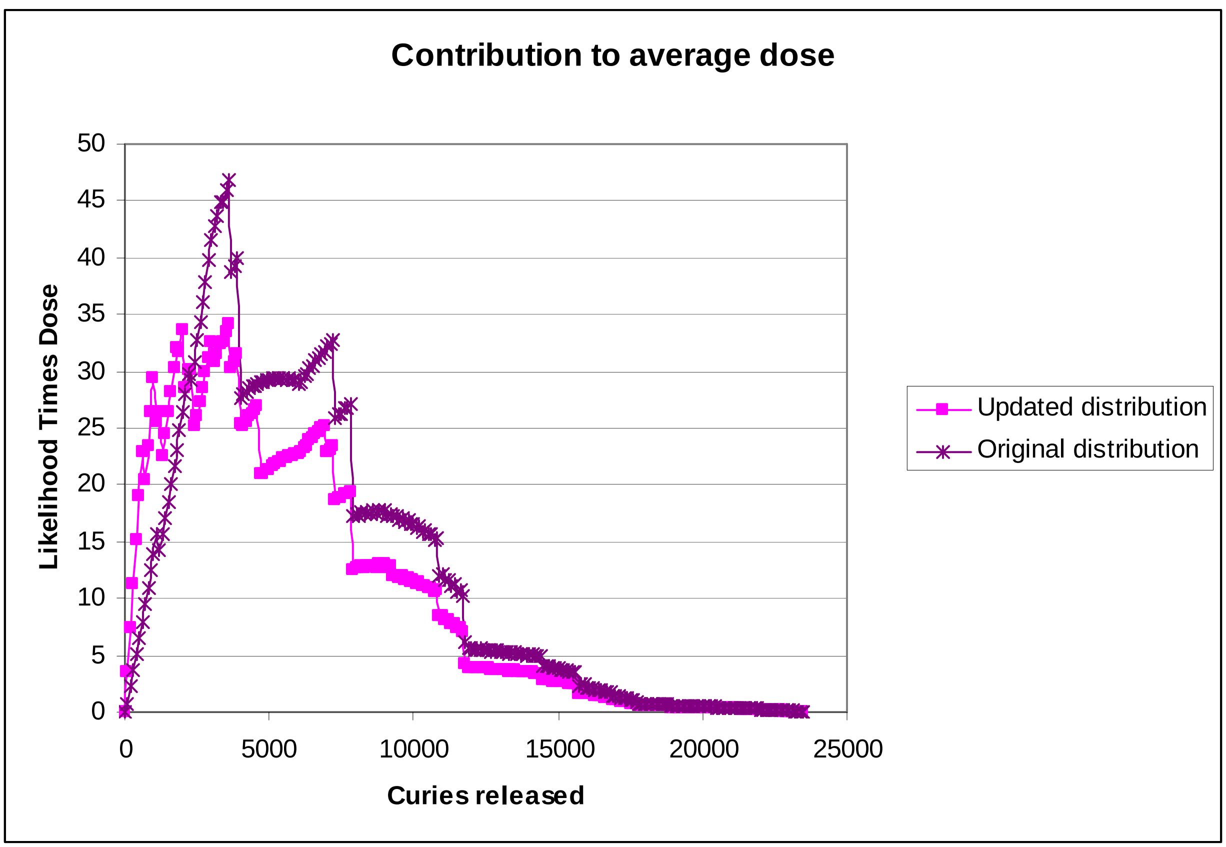


Figure 2-2.



For actual use in an epidemiological study, it would be desirable to have the assignment of likelihoods reflect the judgment of a study team or committee, bearing in mind that experts have a tendency towards overconfidence, which means a tendency to underestimate the tails of a distribution (Cooke 1991). In any case, uncertainty in the scale of the release is a systematic error, which is generally not a problem for an epidemiological study (Thomas et al. 1993). The exact shape of the curve is not likely to have a big effect on the whether or not an association between dose and health effects is deemed statistically significant. On the other hand, the exact shape of the upper part of the curve might influence the decision to proceed with an epidemiological study in the first place (i.e., it might affect the calculation of study power). This is particularly true, if the doses turn out to be too low to justify a study to learn more about radiation effects in general, so that the only reason to do an epidemiological study is to double check the assumptions made in the dose calculations.

Iodine-133. Thompson estimated I-131 inventories, but was never asked by the dosimetrists to account for I-133, which can also contribute to thyroid dose, especially to the inhalation dose. With its shorter half-life of 21 hours, I-133 makes a relatively smaller contribution to milk and leafy vegetable doses, so inclusion of I-133 can often be ignored in dose reconstructions. At SRE, however, the reactor was started and stopped for intervals, with operations lasting for times short enough that I-131 never came close to equilibrium. In any case, Thompson has provided equations that can be used to determine the amount of I-133 that would have been in the core.

For a mature core, there are twice as many curies of I-133 as I-131 (Beyea 1980), but I-133 is only 16.6% as effective as I-131 in producing a dose after inhalation (USEPA 1988). So, for a mature-core accident, I-133 will add 33% as much dose as I-131 (ignoring decay of I-133 during transport, which is reasonable to do for persons residing within 10 miles of SRE).

For the first two days of the SRE event, ignoring for the moment the I-131 left over from earlier runs, the ratio of newly produced curies of I-133/I-131 will be 10 times higher than the ratio for a mature core, using the equations in Thompson's, Table II.2-1. Accounting for the left over I-131, which is two thirds of the total at the end of day 2, the ratio of I-133/I-131 will be 3.33 for this period, producing a 57% additional dose. The corresponding addition at the end of Run 14 is 45%. We take 50% as the average increase. In general, then, when using the scoping calculations in Appendix 2 to estimate individual doses, population doses, and health effects from an I-131 source term at SRE, it is appropriate to multiply the numbers by a factor of 1.5 to account for the I-133 that would have been released along with the I-131. This 1.5-scaling factor is incorporated in the dose calculations presented in the next chapter. However, to scale health effects numbers given in Appendix 2, it is necessary to explicitly account for the addition of I-133. For example, to estimate the health effects resulting from a 2,500-Ci release of I-131, multiply by 1.5 to get 3,750 "effective" curies.

Note that the calculations used to derive this factor of 1.5 are based on two assumptions. First, that second order decay chains are unimportant in the reactor and 2) that the use of inhalation conversion coefficients for adults, not children, is adequate. Possibly, the ratios could change, were the smaller size of the child's thyroid taken into account.

Cs-137. For radiocesium releases, we have three expert estimates to work with, at least initially. First, there is the ATDSR estimate of zero released, based on an AI report. Second, there is an estimate by Gordon Thompson of 29-Ci, which he considered an upper limit release for radiocesium, based on inferences from the amount of deposited radioactivity found on ventilation ducts during the 1969 decommissioning, as well as consideration of certain soil measurements. On the other hand, some cleaning of the ventilation ducts may have taken place after the event before the decommissioning (personal communication, Dan Hirsch), which could have affected the 1966 measurements. Although decontaminating surfaces beyond a factor of 10 –20 is difficult (Beyea et al. 2004), (Chanin and Murfin 1996), consideration of decontamination efforts could affect release estimates using the Thompson approach by an order of magnitude. Also, it is possible that some duct sections had actually been replaced. As for soil measurements of cesium contamination, this is a potentially a powerful constraint on releases, if the measurements are thorough, which they are not yet. Thus, it is of interest to turn to additional engineering estimates of releases to see what can be learned.

Lochbaum's corresponding range for release of cesium-137 is 0.3 to 30%, which corresponds to 24 to 2400 Ci, based on Thompson's SRE core inventory of 7,900 Ci given in his Table II.2-2 (Thompson 2004b). Lochbaum gave a best estimate at 15% (1200 Ci), so his distribution is represented as a triangular distribution with its peak at 1200 Ci. Lochbaum based his result on experiments that have been carried out with sodium reactors and the Fermi I accident. However, he did not consider possible constraints that might be set by cesium ground contamination measurements in his "best" estimate. Presumably, a hypothetical expert combining the approaches to cesium releases followed by Thompson and Lochbaum would come out with an estimate somewhere in between their values. Such a hypothetical expert is considered in developing an overall, combined source-term distribution.

Although there are inadequate environmental measurements to constrain releases of I-131, the situation for Cs-137 is a bit better. Typical background values measured were 0.1 picocuries per gram (pCi/g) (Thompson 2004b), Table IV-2. Values up to about 6 times background (0.6 pCi/g) have been measured at properties offsite (UCLA 2006), and values up to 240 times background have been measured outside the SSFL fence (24 pCi/g) (Boeing 2004).

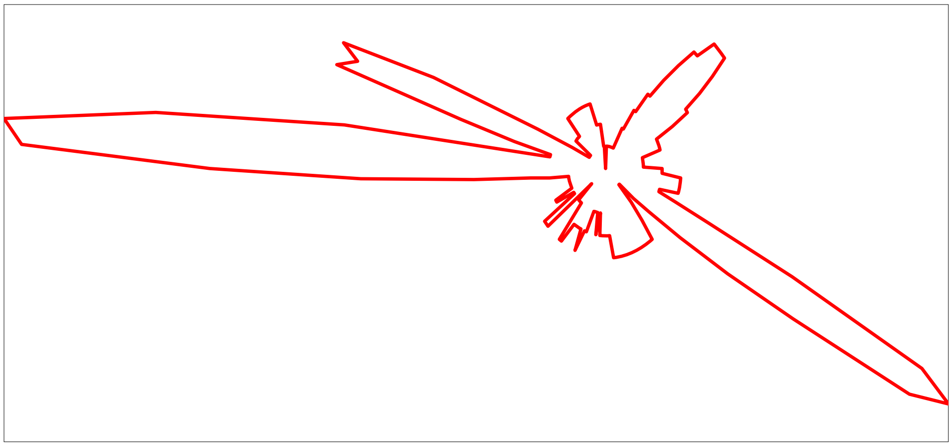
To put these numbers in some perspective, consider spreading, 30 years ago, 1000-Ci of cesium-137 uniformly across a 10,000 km^2 area, e.g., a 100-km by 100-km square. Initially, 10 pCi would be deposited every square centimeter, which would decay to 5 pCi/cm^2 by today. Assuming the radiocesium migrated 10 cm into soil with a density of 2 g/cm^3, the average soil concentration would be 0.25 pCi/g, which is 2.5 times the nominal background from weapons tests in the LA region.[14] It would be detectable with modern equipment. The dose attributable to this deposition would be 0.1-rem over 30 years, which is comparable to one year's worth of the non-radon component of natural background radioactivity. The cancer risk from this dose would be be 1-in-10,000 to 1-in-3,000, depending on which of two risk coefficients one accepts, as discussed in later chapters (0.0015 to 0.003 cancers per rem). Were the population in this hypothetical area to be 10 million, there would be 1000- to 3000-cancers attributable to the 30-years of exposure.

The above, "back-of-the-envelope," calculations have ignored the removal to the sea of soil that would have occurred, especially from slopes, during the intermittent rain storms that occur in the region (Ng and Patterson 1982). Such runoff would not be expected to be great enough to remove the detectable fingerprint that would be left in the intermittent stream beds carrying the runoff, nor in the ocean sediments that would receive it.

In a realistic release scenario, the material would not be deposited uniformly; instead, there would be highs and low based on variations in wind patterns and increased vertical dispersion with distance. Figure 2-3 shows a realistic contour for a ground-level release that hypothetically took place with intermittent release bursts occurring during different wind directions. Note that the cesium and iodine might not have been released equally at equal times, so the patterns might differ for the two isotopes. For an elevated release, there would be low concentrations close to the release point, causing an inner contour to appear.

[14] Although 0.1 pCi/g is a reasonable value for the residual amount of weapons-test radiocesium in the soil for Southern California, no definitive evidence of that value was located in the literature. Further checking is appropriate.

Figure 2-3. Hypothetical deposition contour follow a ground-level release.



The following Tables show variations in soil concentration that would be expected as a function of distance, plume rise, and deposition velocity for a 300-Ci release using a Gaussian plume model (Viegele and Head 1978), (Hamby 2002). The first Table presents calculations for 1.6 km from the release point. The second and third Tables present calculations for 5.5 and 10 km, respectively. The calculations were made assuming a uniform release over all directions, such as might occur for a continuous release assuming a circular wind rose. In fact, because winds do not blow uniformly in all directions, the concentrations shown in the Tables could be expected to vary +- a factor of 3, depending on the wind direction from SSFL. Furthermore, it is unlikely that the release occurred with equal intensity over a long period; instead a pattern such as shown in the above Figure is more likely. Finally, the calculations have been made for the most common atmospheric stability class (neutral stability). For daytime periods when the sky was clear, the atmosphere would become unstable and even high plume-rise scenarios would produce detectable soil concentrations in close.   It would be reasonable to assign an overall factor of ten uncertainty when assessing the soil-concentration tables. At a particular distance, deposition at some angles would be ten times higher, while ten times lower at other angles.

Table 2-8. PCi/g of Cesium-137 at 1.6 km for release of 300 Ci at SSFL uniformly in all directions. 30 Years after deposition. Nominal background levels in the LA region are 0.1 pCi/g.					
	Deposition velocity (m/s)				
Plume Rise (m)	**0.001**	**0.003**	**0.01**	**0.03**	**0.1**
5	1.27	2.85	3.45	0.57	7.71E-05
100	0.19	0.57	1.89	5.66	18.87
150	1.45E-02	4.34E-02	0.14	0.43	1.45
200	3.96E-04	1.19E-03	3.96E-03	1.19E-02	3.96E-02
250	3.89E-06	1.17E-05	3.89E-05	1.17E-04	3.89E-04

Table 2-9 shows the same data for a distance of 5.5 km.

Table 2-9. PCi/g of Cesium-137 at 5.5 km for release of 300 Ci at SSFL uniformly in all directions. 30 Years after deposition. Nominal background levels in the LA region are 0.1 pCi/g.					
	Deposition velocity (m/s)				
Plume Rise (m)	**0.001**	**0.003**	**0.01**	**0.03**	**0.1**
5	0.24	0.62	1.31	1.06	0.04
100	0.14	0.41	1.27	3.11	5.10
150	0.068	0.20	0.67	1.95	5.75
200	0.025	0.074	0.25	0.74	2.38
250	0.007	0.020	0.068	0.20	0.67

Table 2-10 shows the same data for a distance of 10 km.

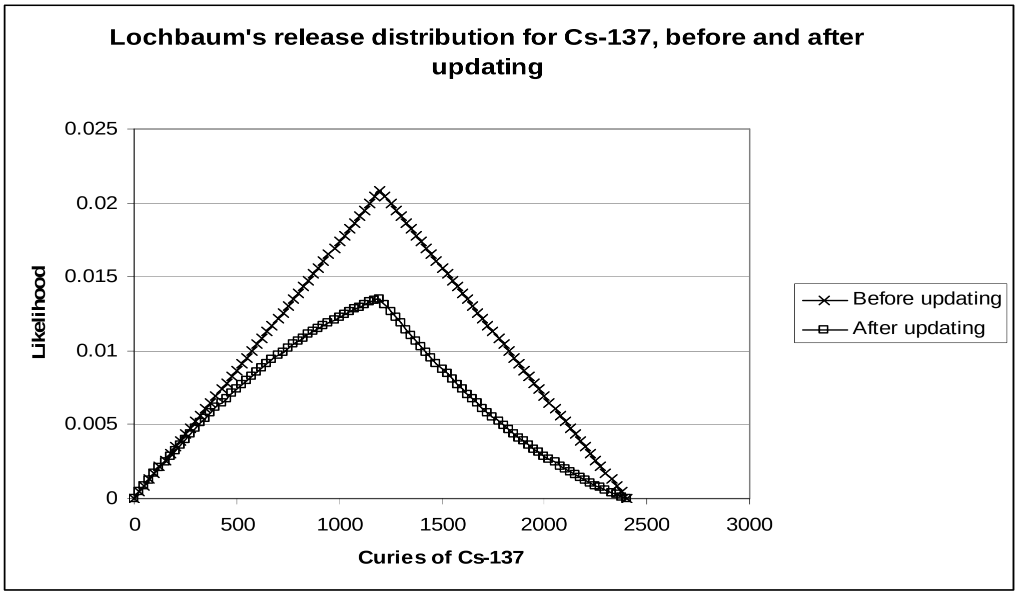
Table 2-10. PCi/g of Cesium-137 at 10 km for release of 300 Ci at SSFL uniformly in all directions. 30 Years after deposition. Nominal background levels in the LA region are 0.1 pCi/g.					
	Deposition velocity (m/s)				
Plume Rise (m)	**0.001**	**0.003**	**0.01**	**0.03**	**0.1**
5	0.087	0.25	0.76	1.72	2.61
100	0.066	0.19	0.60	1.44	2.54
150	0.047	0.14	0.44	1.12	2.31
200	0.029	0.086	0.28	0.76	1.84
250	0.016	0.047	0.15	0.44	1.22

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.

One consequence of a 150-200 plume rise, which is quite plausible given the energy produced in the reactor, is an increased probability of penetration through the inversion layer, with the radioactivity carried, for instance, up North beyond the San Fernando Valley, with an elevated return flow above the Eastern part of the San Fernando Valley reaching all the way back to Los Angeles, with some fumigation to the ground, possibly 60 km or more from SSFL. Population doses would be reduced, but they would be non-trivial. Thus, there exist scenarios consistent with 1000s of curies of Cesium being released, but their fingerprint may well be detectable today with overflights of helicopters equipped with modern gamma detectors (Bechtel 2005). The only overflight taken in the vicinity of SSFL was restricted to the facility boundary (EG&G 1979). 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.

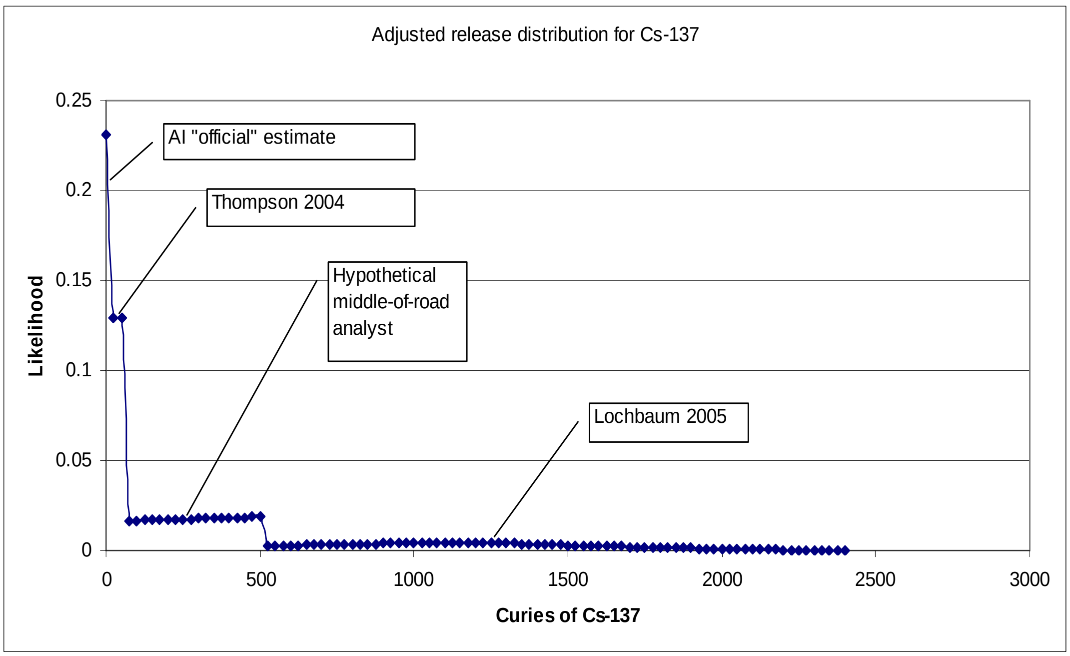
Based on the above considerations, it does not seem likely that 1000s of curies of Cesium-137 could have been released at ground level. This adds support for our decision in subsequent chapters to add a likelihood distribution to account for penetration of the inversion layer. Also, we have attempted to update the radiocesium release likelihoods at high values to account for the fact that an obvious soil signal has not yet been identified. Unfortunately, there are not sufficient soil measurements available to do this in a way that inspires confidence. Hence, the approach taken here is as much illustrative as it is informative. In particular, the Lochbaum source term has been updated so as to lower the likelihoods at high releases. See Figure 2-4. Specifically, in the Bayesian updating of the Cs-137 distribution for Lochbaum, the likelihood is reduced to 30% of its original value at the maximum, with a uniform scaling of the reduction at lower releases. This reduces Lochbaum's average release estimate from 1200 Ci to 1090 Ci. As can be seen in Figure 2-4, the weight his curve is given above ~ 300 Ci in the updated combined distribution is reduced.

Figure 2-4.



As for the ATSDR estimate of the release of radiocesium, which was based on AI reports, the likelihood weight is reduced to to 30% of the starting value, following the same logic used for I-131. A hypothetical middle-of-the-road analyst is added to fill in some of the gaps in the unadjusted distribution. The updating of the estimates of both ATSDR and Lochbaum has the net effect of reducing the average release of Cs-137 from 408- to 334-Ci. It also reduces the width of the distribution somewhat. The 408-Ci figure has 95% confidence limits of (0, 1925), whereas the 334-Ci figure has 95% confidence limits of (0, 1700). The resulting source-term curve, shown in Figure 2-5, is similar to the Iodine curve in that the distribution peaks close to zero and has a very long tail. This behavior will carry over to dose estimates, leaving no peak in the final curve on which to focus an epidemiologist's attention.

Figure 2-5



Adjusted release distribution for Cs-137
Likelihood
0.25
0.2
0.15
0.1
0.05
0
0
500
1000
1500
2000
2500
3000
Curies of Cs-137
AI "official" estimate
Thompson 2004
Hypothetical middle-of-road analyst
Lochbaum 2005

Ch. 3. Considerations in making dose estimates for the SRE.

For an epidemiological study, it is not always necessary to know the absolute magnitude of the release. Uncertainty in the overall scale factor is classified as "systematic error" (Thomas et al. 1993), which will not affect the statistical significance of the strength of any measured association, if the dose of interest is chosen to be a separate term in the epidemiological regressions. On the other hand, the magnitude of a release can always inform judgment about the reasonableness of any association that is found, and estimates of magnitude may "make or break" the funding of an epidemiological study. Release magnitude can also play a role in the statistical significance of an epidemiological analysis in cases where the analyst includes other dose components, say dose from fallout, in a non-linear function, such as log of total dose. A log transformation might be necessary for statistical reasons.

In any case, even if one were to choose a regression function where release magnitude is unimportant, or use a likelihood distribution for release magnitude, such as the ones presented in the previous chapter, there are still a number of unknowns for which additional likelihood distributions would need to be chosen to as part of geographic modeling:

1. The timing of release by hour and day is unknown and no doubt debated among SSFL stakeholders. To deal with this gap, likelihood distributions would need to be developed for release timing scenarios. Simulating hourly release scenarios is a non-trivial problem, but has been dealt with before (Beyea and DeCicco 1990). Because wind frequencies differ by time of day, different hourly release scenarios would produce impacts on inhalation dose that would differ depending on a person's location with respect to angle from the SRE. Therefore, this uncertainty is not systematic. It introduces random errors, which would tend to bias the association towards the null (Carroll et al. 1998; Thomas et al. 1993).

2. Site-specific meteorological data, such as wind direction, speed, and stability class, does not exist for July of 1959 and data for subsequent years has been withheld from independent analysts.

   At some sites, one could make simple assumptions about releases, e.g., assume a uniform release over time, and still obtain information useful for an epidemiological study. At SSFL, this will not be possible for independent analysts due to lack of access to detailed meteorological data. All onsite meteorological data, even that in the 1990s, has been withheld by the plant operator as confidential information,[15] implying that such data will only be available to epidemiologists deemed safe by the plant operator. Post-1959 data, even though not contemporaneous with the July 1959 release, would help in exposure assessment for 1959, following procedures used in studies at other sites (Nair et al. 2000). Contemporaneous data from surrounding airports, although useful, as we shall see in Appendix 2, is not adequate for tracking releases.

In a Power Point presentation archived on the Internet, aggregated site-specific SRE data for the 1990s has been presented in the form of wind roses (Chinkin et al. 2003). This data has been used for scoping calculations in Appendix 2. What is missing is the breakdown of the wind rose data by "atmospheric stability class" and time of day. If necessary, one could develop likelihood distributions for the missing data, forcing them to be consistent with the aggregated data. As with uncertainty in hourly release scenarios, random errors would be introduced, tending to bias any epidemiological association towards the null. Also, missing in the available meteorological data is the frequency of calms, which can be important for dosimetry at some sites.

3. The heat content of the released puffs is unknown, making plume rise uncertain. Uncertain plume rise means that the corresponding inhalation dose at ground level is uncertain, as well as the amount of deposition onto soil and food sources. To deal with this problem systematically, a prior likelihood distribution would need to be developed for plume rise. Once again, uncertainty in plume rise will introduce random errors in dose, with a tendency to bias the association towards the null. Note that plume rise may correlate with release magnitude.

4. As discussed in Appendix 2, there are a number of parameters associated with handling the complex terrain at SSFL, such as the fraction of radioactivity that ends up above the inversion layer, that are uncertain. Such parameters will have a wide uncertainty distribution that must be assigned likelihood values based on professional (subjective) judgment. Some uncertainties will represent systematic errors, which

[15] When information is withheld, it raises the suspicion that there is something to hide. What could be hidden in such information? It could be internally inconsistent, which would become apparent to an expert analyst. Handling of low wind speed events could be such as to underestimate total exposures. Presumably, withholding information serves the interests of the operator.

will not interfere with finding a statistically significant association (Thomas et al. 1993); however, some uncertainties will include random errors that will lead to additional bias of any epidemiological association towards the null.

5. Finally, even were all the above-mentioned uncertainties to be magically removed, there would still remain the standard uncertainties and variabilities that are inherent in geographic modeling exercises (Beyea and Hatch 1999). Some of them are systematic and therefore not an obstacle to an epidemiological study. Fortunately, even some of those that are not systematic do not lead to any bias towards the null (so-called Berkson errors)[16] (Carroll et al. 1998; Thomas et al. 1993).

Further exacerbating the difficulties of making dose estimates for SSFL is the lack of validation and calibration data for I-131 releases, which can be extremely important in building confidence in exposure estimates using geographic modeling (See for example, (Beyea et al. 2006)). Measurements of radiocesium contamination could have been made for use at SSFL, but so far have not been collected in a way that could be used for model validation and calibration.

Calibration can serve to narrow the uncertainty in model parameters. Without validation data for the exposure model, one never knows if a null result in an epidemiological study is due to the absence of an effect or a bad exposure model. At SSFL, there are no known contemporaneous measurements of radioiodine in milk or detailed groundshine measurements that would have informed modern exposure assessments and narrowed the uncertainties in thyroid and whole-body dose. Response in the United Kingdom to the 1957 Windscale accident in terms of monitoring demonstrates that radiation experts of the era knew the important pathways to humans; they knew what to measure, including radioiodine in milk; and they had the technology to make the measurements. (See Appendix 1.) Responsibility for the lack of validation data rests with the plant operators, who had a business incentive for avoiding unpleasant publicity and therefore an incentive to "look the other way."

On the other hand, knowledge that obvious measurements were not made (or, if made, suppressed) is information that can be used in a Bayesian updating of generic likelihood distributions. The same is true for knowledge that meteorological data is even today being withheld. The existence of data-withholding suggests that the true radioiodine and/or

[16] E.g., all members of a group being assigned the mean of the group. The conclusion about Berkson error applies to linear models of disease.

radiocesium release was not insignificant and that the true meteorological data is more conducive to higher doses than an analyst might assume based on experience with other sites.

Still, so many likelihood distributions would have to be made for crucial parameters, that it is quite possible that the spread in the distribution of an individual's dose estimate would turn out to be so high as to wipe out any chance of finding a statistically significant association with health data. The bottom line for dose calculations is that for epidemiological analysis, geographic modeling calculations are going to involve numerous assumptions, i.e., they may be limited to scoping calculations. The details of such calculations for the July 1959 accident at the SRE are discussed and presented in Appendix 2. The blame for any limitations in those calculations must be laid at the feet of the various owners of the SSFL.

In this report, both individual and population dose calculations are made. Population dose estimates for large areas around SSFL are considered for the purpose of calculating numbers of predicted disease events based on assumptions about the absolute population dose and assumed dose-response relationships. Although population dose estimates are based in part on variable assumptions about the height and speed of plumes of radioactive releases from the SRE – assumptions that affect estimated population doses based on geographic distributions of the population – the accuracy of the estimated numbers of cancers is a function of the correctness of the total population dose, not the geographic distribution of doses, assuming a linear model of dose response. In contrast, the accuracy of epidemiological measures of association between estimated doses and outcomes depends on ability to correctly estimate doses for individuals or specific geographic areas, which is a more difficult challenge. On the other hand, for an epidemiological study, the absolute magnitude of the dose is usually less important than its relative magnitude across populations.

Tables 3-1 to 3-3 present examples of the inhalation component of the thyroid dose using the Gaussian plume model (Viegele and Head 1978), (Hamby 2002). Results are shown for distances of 1.6, 5.5 and 10 km, respectively, for the most common atmospheric stability class, namely neutral stability, and a variety of assumed plume rises. The milk and food dose are not included. They tend to be less important, except at great distances. The numbers in the Tables were computed for a long-duration release, assuming a uniform wind rose, which means that everyone gets the same average dose at any wind angle. In reality, the numbers in the Tables could easily be ten or more times higher, or ten or more times lower in specific 22.5 degree sectors.

Table 3-1. Individual inhalation thyroid dose in rem at 1.6 km from 10,000 Ci of I-131 released below inversion layer. Gaussian Plume model, neutral stability. Uniform wind rose (same dose in all directions). Multiply by 1.5 to include I-133 contribution.

	Deposition velocity (m/s)				
Plume Rise (m)	**0.001**	**0.003**	**0.01**	**0.03**	**0.1**
5	6.18	4.63	1.68	0.093	3.76E-06
100	0.92	0.92	0.92	0.92	0.92
150	0.070	0.070	0.070	0.070	0.070
200	1.93E-03	1.93E-03	1.93E-03	1.93E-03	1.93E-03
250	1.89E-05	1.89E-05	1.89E-05	1.89E-05	1.89E-05

Table 3-2. Individual inhalation thyroid dose in rem at 5.5 km from 10,000 Ci of I-131 released below inversion layer. Gaussian Plume model, neutral stability. Uniform wind rose (same dose in all directions). Multiply by 1.5 to include I-133 contribution.

	Deposition velocity (m/s)				
Plume Rise (m)	**0.001**	**0.003**	**0.01**	**0.03**	**0.1**
5	0.97	0.84	0.51	0.12	0.00
100	0.61	0.59	0.54	0.41	0.16
150	0.32	0.32	0.31	0.29	0.23
200	0.13	0.13	0.13	0.13	0.12
250	0.042	0.042	0.042	0.042	0.041

Table 3-3. Individual inhalation thyroid dose in rem at 10 km from 10,000 Ci of I-131 released below inversion layer. Gaussian Plume model, neutral stability. Uniform wind rose (same dose in all directions). Multiply by 1.5 to include I-133 contribution.

	Deposition velocity (m/s)				
Plume Rise (m)	**0.001**	**0.003**	**0.01**	**0.03**	**0.1**
5	0.42	0.41	0.37	0.28	0.13
100	0.32	0.31	0.29	0.23	0.12
150	0.23	0.22	0.21	0.18	0.11
200	0.14	0.14	0.13	0.12	0.089
250	0.076	0.076	0.074	0.070	0.059

To convert deposition of radiocesium to dose, we have made use of coefficients derived from Chernobyl data (Bunzl et al. 1997). Bunzl et al. find that different equations must be used within 100 km of the release and at 1000 km.

According to these researchers, the solubility of radiocesium is considerably reduced < 100 km from the release, leading to a slow migration into the soil. Apparently, for longer travel distances, the particles attach themselves to natural, more soluble aerosols, leading to faster migration into the soil. Thus, the dose values measured by Bunzl et al. within 100 km of Chornobyl show a much slower penetration into the soil and a correspondingly slower reduction in the dose with time. The dose declines with a characteristic 16-year half life. The ground shielding factor is also much less than previously assumed, with an initial reduction of only 0.6. The net effect is a four-fold increase in the 30-year dose from a deposition of radiocesium compared to values previously used by the author (Beyea 1980) and others based on pre-Chornobyl fallout data. Bunzl et al. do find a faster penetration into the soil for distances of 1000 km from Chornobyl, consistent with the fallout data. The authors summarize past measurements and cite other researchers who reach the same conclusion.

Tables 3-4 to 3-6 show individual doses to the whole body accumulated over 30 years from groundshine computed with the new soil-penetration equation. These doses are delivered over time, with a 16-year halflife. Note that these doses should be added to the individual thyroid doses presented in earlier Tables, when assessing risk to the thyroid gland of exposed persons.

Table 3-4. Individual groundshine dose in rem accumulated over 30 years at 1.6 km from 300 Ci of Cs-137 released below inversion layer. Gaussian Plume model, neutral stability. Uniform wind rose (same dose in all directions). Includes accompanying Cs-134.

	Deposition velocity (m/s)				
Plume Rise (m)	**0.001**	**0.003**	**0.01**	**0.03**	**0.1**
5	0.50	1.11	1.35	0.22	3.01E-05
100	0.074	0.22	0.74	2.21	7.36
150	5.64E-03	0.017	0.056	0.169234	0.564
200	1.55E-04	4.64E-04	1.55E-03	4.64E-03	0.015
250	1.52E-06	4.55E-06	1.52E-05	4.55E-05	1.52E-04

Table 3-5. Individual groundshine dose in rem accumulated over 30 years at 5.5 km from 300 Ci of Cs-137 released below inversion layer. Gaussian Plume model, neutral stability. Uniform wind rose (same dose in all directions). Includes accompanying Cs-134.					
	Deposition velocity (m/s)				
Plume Rise (m)	**0.001**	**0.003**	**0.01**	**0.03**	**0.1**
5	0.078	0.20	0.41	0.30	8.00E-03
100	0.049	0.14	0.43	0.99	1.33
150	0.026	0.077	0.25	0.70	1.83
200	0.011	0.032	0.10	0.31	0.94
250	3.40E-03	0.010	0.034	0.10	0.33

Table 3-6. Individual groundshine dose in rem accumulated over 30 years at 10 km from 300 Ci of Cs-137 released below inversion layer. Gaussian Plume model, neutral stability. Uniform wind rose (same dose in all directions). Includes accompanying Cs-134.					
	Deposition velocity (m/s)				
Plume Rise (m)	**0.001**	**0.003**	**0.01**	**0.03**	**0.1**
5	0.034	0.099	0.30	0.67	1.02
100	0.026	0.076	0.23	0.56	0.99
150	0.018	0.054	0.17	0.44	0.90
200	0.011	0.034	0.11	0.30	0.72
250	6.14E-03	0.018	0.060	0.17	0.48

The sum of individual doses, the so-called population dose, is less model dependent than individual dose calculations. As stated earlier, population dose calculations are used to estimate total number of health effects. In Tables 3-7 and 3-8 below, the population doses derived from the scoping calculations for SSFL are summarized. These calculations make use of the source-term distributions discussed in Chapter 2. Units are 1000s of person-rem. Both thyroid population dose and whole-body population dose are presented in Table 3-7, whereas the doses by distance presented in Table 3-8 are restricted to whole-body ground shine doses.

For reference purposes, a table of population data is included. See Table 3-9 for population as a function of distance from SSFL for different wind sectors.

To generate the numbers in Tables 3-7 and 3-8, 20,000 simulations were run using different combinations of assumptions favored by different experts and different values of uncertain parameters. As indicated in the thyroid-column in Table 3-7, the thyroid population dose averaged over all 20,000 simulations was 65,000 person-rem (= the mean). In 5% of the simulations, the population doses was zero (mathematical prediction < 0.5). In half of the scenarios, the thyroid population dose was 20,000 person rem. In 2.5% of them, the thyroid population dose was 433,000. Summarizing the results: the average thyroid population dose was 65,000, with a 95%-confidence range of 0 to 433,000.

Table 3.7. Projected population dose in thousands of person-rem arising from the 1959 SSFL release computed at different likelihood percentiles. Scoping calculations. (Excess above whole-body background radiation). All distances. Excludes contribution from food, which is a relatively minor contribution at this site.

Percentiles	Thyroid	Whole-body
2.5%	0	0
5.0%	0	0
50%	20	14
Mean	65	75
95%	276	360
97.5%	433	520

Table 3-8. Whole-body population dose for different cutoff distances in 1000s of person rem arising from the 1959 SSFL release computed at different likelihood percentiles. Scoping calculations. (Excess above whole-body background radiation.) Includes dose from Cesium-134.

Percentiles	< 5 km	< 10 km	< 20 km	< 50 km	< 100 km[a)]
2.5%	0.00	0.00	0.00	0.00	0.00
5.0%	0.00	0.00	0.00	0.00	0.00
50%	0.11	2.0	3.1	6.0	14
Mean	0.93	14	21	35	76
95%	4.8	75	110	170	370
97.5%	7.6	110	160	260	530

a) Includes dose from return flow from North, some of which is delivered beyond 100 km.

Table 3-9. Cumulative 1960 Population distribution out to different distances. Approximate values involving extrapolation from 1970-2000 census tract population figures. All population in a census tract is assigned within or without a radial boundary based on location of centroid of tract. Chatsworth and San Fernando lie in Sector 4. Canogoa Park in Sector 6. Oxnard is in Sector 13.

	Distance from SSFL (km)					
Sector	5	7.5	10	20	50	100
1	0	6,540	10,283	10,283	10,844	11,130
2	0	3,743	3,743	3,743	7,672	9,966
3	1,129	2,222	2,222	2,222	36,080	90,942
4	0	0	1,054	42,560	145,940	151,852
5	0	2,702	8,405	112,606	507,177	831,365
6	0	4,941	26,861	139,380	1,301,202	3,334,308
7	0	8,145	30,512	58,371	1,123,621	3,339,142
8	0	0	2,072	6,019	11,430	129,045
9	0	0	0	0	4,437	4,437
10	0	0	606	1,023	2,308	2,308
11	0	331	398	6,746	7,275	7,275
12	0	0	0	20,085	56,172	56,172
13	0	0	572	14,573	143,382	167,865
14	0	4,454	9,748	12,412	35,594	143,970
15	0	14,036	16,381	16,381	23,994	24,249
16	4,600	10,370	11,293	11,293	12,634	14,324

Chapter 4. Projected health effects from the 1959 accident.

To go from doses to health effects, requires multiplication of individual or population doses by "dose effect coefficients." Recent studies have suggested that past ideas about these risk coefficients need updating. Although it is still early to assess the full ramifications of these new studies, a preliminary assessment of their impact on risk coefficients has been made in this chapter.

Health effects resulting from computed thyroid dose.

In this report, conversion of dose to expected number of thyroid cancers is carried out based on the risk coefficient from the expert elicitation of Little et al. This elicitation produced a distribution with a median of 6 fatalities per million person-rem and a 90% confidence range of <.001 to 71 (Little et al. 1997a). A 90%-confidence range has a lower limit at the 5%-likelihood level and an upper range at the 95%-likelihood level. The geometric standard deviation was 4.5[17]. To be consistent with Little et al., it is assumed that there are ten, non-fatal thyroid cancers for every fatal one (page C-136). Therefore the range given above has been multiplied by ten to obtain the coefficient for expected thyroid cancers (median: 60 [< .01, 710] per million person-rem). Note that the dose here is averaged over a standard population, including both children and adults. No Radiobiologic Effectiveness Factor (RBE) has been applied to account for any difference in effectiveness of radioiodine compared to external radiation. A distribution for the RBE of Iodine-131 has been estimated by Kocher et al. that is currently in use in compensation schemes for nuclear workers (NIOSH 2002), page 42, (Kocher et al. 2002). The mean reduction factor is 0.7. Given the uncertainty in the applicability of an RBE to beta rays, analysts must make up their own minds as to whether or not to include this factor in risk calculations. If so, it is necessary to incorporate the distribution into the computation of means and confidence intervals.

What risk coefficient can be estimated from the Chernobyl data? The estimated collective thyroid dose in Belarus following the Chernobyl accident was 553,000 person-GY (55 million person-rad) (O'Hare et al. 2000), Table 40. (No uncertainty range is given in the source document.[18]) About 15,000 excess thyroid cancers are projected to arise from this exposure within 50 years after the accident (Jacob et al. 2000). The estimated range is 5,000 to 45,000

[17] The median and 90% confidence interval is given as 0.059 ($<10^{-3}$, 0 .71). Using the Excel add-on, Crystal Ball, to fit a lognormal distribution with these parameters gives a GSD of 4.5. The corresponding mean is 0.18. Table 4.1 of Little et al. (Little et al. 1997a).

[18] The standard error from stochastic contributions is likely to be small, since the number of exposed persons (10 million) is so large. Systematic errors are likely to dominate.

thyroid cancers. The excess relative risk was 21 per Gy. The ratio of excess cancers to population dose suggests a risk coefficient of 270, which lies within the 90% confidence range set by Little et al., but is 4.5 times higher than the median value that appears in the Tables. It is also 50% higher than the mean value of the distribution used for this report.

A new Chernobyl case-control study (Kopecky et al. 2006) has found extremely high relative risk coefficients for their central estimate, albeit with wide confidence limits that do (barely) include earlier estimates. The authors state,

> "The estimated excess relative risk (ERR) associated with radiation exposure, 48.7/Gy, was significantly greater than 0 (P = 0.00013) but had an extremely wide 95% confidence interval (4.8 to 1151/Gy). Adjusting for dose uncertainty nearly tripled the ERR to 138/Gy, although this was likely an overestimate due to limitations in the modeling of dose uncertainties."

The authors argue that previous studies may have underestimated risk because they did not account for dose uncertainties.

The central estimate of Kopecky et al. is about twice the estimate of Jacob et al. and about five times the best central estimate obtained in the past, even without accounting for dose uncertainties. Acounting for dose uncertainty would argue for another factor of two increase and would appear to bring the estimate outside Little's 90%-confidence range.[19] Therefore, the study by Kopecky et al. should tend to increase any consensus risk coefficient for thyroid cancer. Of equal importance for the field of radiation studies is the authors' explicit recognition that the contribution of I-133 at Chernobyl was not an explanation for the high-risk coefficient. I-131 contributed 95% of the dose. In any case, the debate over which isotope caused the Chernobyl thyroid cancers is not an issue for SSFL releases, because I-133 was a major part of the inventory.

Another new case-control study of thyroid cancer at Chernobyl (Tronko et al. 2006b) did not find such high risks. In fact, their finding was very close to pre-Chernobyl estimates:

> "Thyroid cancer showed a strong, monotonic, and approximately linear relationship with individual thyroid dose estimate (P <.001), yielding an estimated excess relative risk of 5.25 per Gy (95% confidence interval [CI] = 1.70 to 27.5).

The findings of this study should serve to compensate, in part, for the findings of Kopecky et al, bringing back a consensus risk estimate towards pre-Chernobyl values.

Mild to moderate iodine deficiency existed in the population exposed at Chernobyl (Robbins et al. 2001). Compared to a U.S. population, this deficiency is likely to have increased

[19] Note that we have gone back and forth between excess and absolute risk quite casually in this study. In fact, this is a very difficult problem (Kellerer et al. 2001) that lies outside the scope of this study.

the uptake of radioiodine by the thyroid in the exposed Chernobyl population, increasing doses (Nedveckaite et al. 2004). Fortunately, the estimates of thyroid dose are tied to actual thyroid activity measurements (Drozdovitch et al. 1997), (Gavrilin et al. 1999), which means that the collective thyroid dose cited above accounts for the iodine deficiency in the population. More problematic is the role that iodine deficiency might play in the years following the accident .

> "Theoretically, the resulting increase in thyroid cellular activity might increase the risk of tumorigenesis but experimental or clinical evidence supporting this hypothesis is meager or absent." (Robbins et al. 2001)

Until recently, available data was restricted to animal experimentation. Even for animals, the protocols are not relevant to the Chernobyl situation. No experiments are known where I-131 treated animals were placed on a low iodine diet long after exposure to the radiation (Robbins et al. 2001).

In terms of the relationship between iodine status and background thyroid disease:

> "Overall, [the data] suggest that iodine deficiency increases the incidence of thyroid nodules and follicular cancer, while iodine sufficiency and excess are associated with more papillary cancer and more autoimmune thyroid disease." (Robbins et al. 2001)

A recent epidemiological study has provided indirect support for the first part of the above assertion. The authors find that the risk of radiation-induced thyroid cancer was three times higher in iodine deficient areas than elsewhere (Cardis et al. 2005a).

As a result, when transferring to a U.S. population the ratio of thyroid cancers to collective dose determined from Chernobyl data, there is an additional uncertainty that must be accounted for relating to iodine status. For this reason, and given the fact that the Chernobyl results, with one exception, lie within the 90% confidence interval used in this report, the raw Chernobyl risk coefficients have not been used for the central value.

<u>Risk coefficients for cancers from whole body doses.</u>

Until recently, it was standard to rely on results from epidemiological studies of the Atomic Bomb survivors to assess the risk of cancer resulting from radiation exposure at low doses. The National Research Council and the Institute of Medicine have put out every ten years or so a review of the scientific situation, always relying primarily on the A-bomb data. In the most recent report, the authors did accept a reduction of a factor of 1.5 at low doses and dose rates. However, after the bulk of the work was concluded, two new studies appeared that have shaken the logic of reliance on A-bomb data. Epidemiologists have to take these new studies into account when planning future studies.

Increases in cancer risk coefficients is nothing new. They have been steady since 1972.[20] The latest pressure to increase risk coefficients comes from two studies with average doses an order of magnitude less than received by the A-Bomb survivors, making them particularly relevant to the SSFL situation (Cardis et al. 2005b), (Krestinina et al. 2005). Both studies give similar values for mortality from low dose, protracted exposure, namely an excess relative risk of 1 per Sievert (100 rem). Large study populations are involved.

The study population in Cardis et al. consists of nuclear workers world wide. The average dose was 2-rem. The authors of this large, international study of radiation workers included major figures in the field of radiation studies. The authors state, "On the basis of these estimates, 1-2% of deaths from cancer among workers in this cohort may be attributable to radiation." Although it can be misleading to interpret epidemiological data in this way (Beyea and Greenland 1999), because it implies to non-experts a single-cause model of cancer, there is no doubt that a 1-2% increase in cancer mortality for a worker population is unusually high. When a comparable, age-matched selection from the A-bomb survivors was made, the authors obtained a ratio for ERRs = 0.87/0.32 = 2.72. (See Table 2 in Cardis et al.) This is a large increase in risk relative to the A-bomb results.

The study population considered by Krestinina et al. consists of a general population of males and females (Techa cohort) with an age distribution at exposure (~1950) similar to the Japanese in 1945, with the exception of a deficit in males killed during the Second World War (Kossenko et al. 1997). The average age of the population at exposure was 29 (Kossenko et al. 1997). The results for the Techa River cohort are as equally striking as the worker results, showing a strong linear effect down to a few rads (See Figure 4-1). The average dose was 3 rads. The authors, who once again include major figures in the field of radiation studies, state: "It is estimated that about 2.5% of the solid cancer deaths...are associated with the radiation exposure." As in the worker population, an increase in solid cancer deaths of 2.5% from an average dose of 3-rads is extraordinarily high compared to past estimates.

[20] For instance, there was a large increase in the risk coefficients estimated between the 1980 BEIR III report and the 1990 BEIR V report. See Table 4-4 of (National Research Council 1990), where the lifetime risk estimates increased by a factor of 4.6-19, depending on the risk model.

Figure 4-1. Dose response found in the Techa River cohort. Reprinted from (Krestinina et al. 2005).

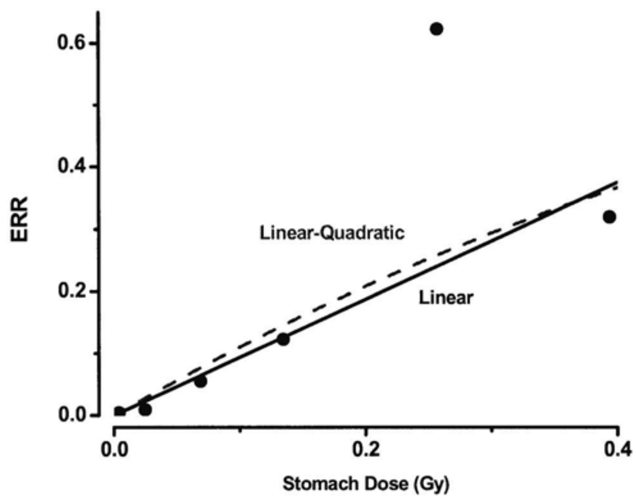


FIG. 1. ETRC solid cancer dose response.

Krestinina et al. give a value for absolute mortality risk at an attained age of 70 equal to 70.5 cases per 10,000 person-year per gray (page 606). This translates to a risk at age 60 of 70.5/2 = 35 using their fit to risk with age, which has a power law exponent of 4.5. Comparing this risk at age 60 to the comparable number in BEIR VII (Table 12-1, last row, p. 476. (NRC 2005)), it is seen that the Techa cohort produces an absolute risk estimate that is 2.7 times higher than the BEIR VII numbers, which are based on the Atomic Bomb data. (The data from the BEIR VII was taken from a Table in which the "dose and dose rate effectiveness factor" (DDREF) of 1.5 had not yet been included.)

A crude assessment of the excess relative risk found in the Techa cohort also supports an increase by a factor of about 2.77.[21] In contrast, a recent letter to the editor in Health Physics

[21] The mortality ERR/SV for the Techa cohort is 0.92. The absolute risk would be 0.92 * 0.23 /100 per rem = 0.002116, assuming a 23% lifetime risk of cacner. In contrast, BEIR VII has a lifetime mortality risk of

asserts, without any citation or analysis, that the risk from the Techa cohort is but 1.5 times the A-bomb risks (Anspaugh et al. 2006). Attempts to obtain an explanation have not yet been successful. Despite the lack of any derivation of this number, it must be taken seriously, since some of the authors have done work on this cohort. Furthermore, comparison of risks between different populations is a very tricky business (Kellerer et al. 2001), (Brand 2005), suggesting that considerable humility is appropriate. As a result, for this study, an average is taken between risk scale factors of 1.5 and 2.7 for the Techa cohort, which gives an increase over the A-bomb risks of of a factor of 2 (without consideration of any DDREF).

At a minimum, the results of these two new studies should put an end to the use of a DDREF, which has no epidemiological basis. The DDREF has been chosen based on assumptions that no longer seem credible. As a result, for this report, the lifetime attributable risk assigned by the BEIR VII committee of 0.00105 (0.0005, 0.0018) cancers per rem (0.0005, 0.0018) is increased by a factor of 1.5 to 0.00158 cancers per rem, with a comparably scaled set of 95%-confidence limits. For our main estimate of whole-body cancer risk for use in this report, we average the absolute numbers derivable from Cardis, Krestinina, and BEIR VII (without DDREF) to obtain 0.003 cancers per rem. We scale the 95%-confidence limits accordingly.[22] Generally, in the Tables that follow, for comparison purposes, we also include results using the BEIR VII number (without the DREF), which we list in parentheses.

Such high risk coefficients imply that background radiation itself must increase cancer mortality by 3-5%.[23] (It has long been known that background radon concentrations may well increase lung cancer rates by 10% or more (Lubin et al. 1995), (Darby et al. 2005).) Critics of studies like those by Cardis et al. and by Krestinina et al. argue that such big effects, if they were real, should show up in cancer statistics in places like Colorado, where background radiation is high, when compared to areas of the country where background radiation is lower. However, crude statistical analysis that does not adjust for covariates at an individual level is unlikely to be very reliable (Lubin 1998). Also, there is an issue of the confounding effect of hypoxia (Weinberg et al. 1987). Hypoxia also varies with altitude.

0.00051 for their preferred model, which includes a DDREF of 1.5. Ratio of the Techa risk to the BEIR VII preferred model = 2.116/0.51 = 4.15, Dividing out the 1.5 for the DDREF gives a ratio of 2.77.

[22] Due to limited time, the confidence limits were not increased to account for the variation in the estimates of the new studies. This omission should not change any conclusion in this report.

[23] Assuming 0.1 rem per year background, which ignores the "equivalent" dose to the lung from radon. It is more difficult to compare rates of lung cancer, because the interaction of smoking and radiation has been found to lie between a linear and relative model. Therefore, such interactions must be taken into account, before drawing conclusions about area-wide differences, or lack of differences, in lung cancer rates.

Because the average dose in these two new studies is so low and so close to background radiation dose, there is no way to escape the linear non-threshold model. Even were a hypothetical hormesis effect to lead to a minimum risk at background levels (5-rem lifetime dose), the risk has to rise again after another 2-3 rem dose, based on the studies by Cardis et al. and Krestinina et al.

Could the increased risk numbers be due to a systematic underestimate or underreporting of doses? Random errors in doses would tend, in most cases, to reduce the strength of associations (Carroll et al. 1998), (Thomas et al. 1993). On the other hand, if dose errors were not random, but were proportionately underestimated or proportionately underreported in the worker studies and the Techa River cohort, then the risk coefficients could be inflated. For this to happen in both studies would be a coincidence. And in the radiation worker study, the results for Hanford do not support the missing-dose hypothesis, even though we know the neutron doses were likely underreported at Hanford (CohenAssociates 2005). In fact, the cancer risk numbers at Hanford were lower than average, not higher (Cardis et al. 2005b). Finally, should the Techa River cohort dose estimates be too low that would mean that modern dose reconstruction techniques are underestimating doses, suggesting that other modern dose estimation techniques, such as those used in this report could well be too low. In that case, an upward adjustment of doses would be required, if the risk coefficients were kept the same. No change would result in the cancer estimate given in this chapter. Certainly, from a public health point of view, the arguments are strong for making use of the new risk coefficients, one way or another, whenever modern dose estimate computer programs and approaches are used.

Recent press reports around the anniversary of the Chernobyl accident seemed to suggest that effects of radiation doses were lower than expected. Not at all. The "new" estimates of 4,000 projected fatalities were merely a re-interpretation of a study from the 1990s. No longer were 5,000 projected cancers outside the most highly contaminated regions counted. Also, another 7,000 cancers projected to occur in Europe were not noted by the press (Cardis et al. 2006). A summary of all of these estimates can be found in (Cardis et al. 2006). Were the new risk coefficients discussed earlier applied to the population dose estimates, the projected numbers of fatalities from the Chernobyl releases would climb much higher.

The confusion over the Chernobyl numbers appears to be traceable to a typo in a highly publicized IAEA report (Forum 2005) that relied on a WHO report for its cancer numbers (WHO 2005). The WHO report stated that the "Expert Group" concluded that there may be up to 4 000 additional cancer deaths among the three *highest* exposed groups over their lifetime (emphasis added). This was translated in the IAEA report to, "The total number of people that could have died or could die in the future due to Chornobyl originated exposure over the lifetime of emergency workers and residents of *most* contaminated areas is estimated to be around 4 000." (Emphasis added.) In fact, in my view, the last clause should have referred to “residents of *the* most contaminated areas...”[24] See a recent article by Keith Baverstock that gives additional details about institutional failures to deal with these issues properly (Baverstock and Williams 2006).

Table 4-1 shows the projected number of cancers from the 1959 SSFL release computed at different likelihood percentiles. These numbers are excess cancers above the millions of background cancers that would have occurred without the accident. They were computed using the source-term distributions derived in Chapter 2 and the methods described in Appendix 2. 20,000 simulations were run using different combinations of assumptions favored by different experts and different values of uncertain parameters. As indicated in the last column of Table 4-1, the number of excess cancers averaged over all 20,000 simulations was 256 (= the mean). In 25% of the simulations, there were 8 or less excess cancers. In half of the scenarios, the number of cancers was 49 or more. In 2.5% of them, the number of predicted cancers was 1820 or more.

The numbers in the Table can be used to derive various confidence intervals. For instance, the 95%-confidence interval for all cancers is (0, 1800). The average value is 260. These numbers characterize the uncertainty in SSFL cancer consequences at our state of knowledge in the year 2006 of releases, transport, and radiation risks, based on the opinions of experts and data from the scientific literature combined in Bayesian fashion. The numbers given in the Tables should not be considered, “the truth.”

Note that no precision is implied by stating cancer values or confidence limits to 3 significant figures in the Table. The choice of representing a distribution in summary format is a matter of taste. The preference adopted in this report for sums of cancers is to use a mean and the 95%-confidence levels, which in this case, as stated above, would be 260 (0, 1800) for all cancers. However, for epidemiologists concerned about statistical power, it might be more sobering to use the median, namely, 50 (0, 1800). With such wide confidence intervals, neither the median nor the mean can be considered best for all purposes.

Table 4-2 breaks down the results by distance from the SSFL facility. Tables 4-1 and 4-2 could be (mistakenly) read to imply that the predicted excess cancers were caused solely by SSFL exposure and no further individuals would have been affected. In fact, released radioactivity

[24]Note that the IAEA stands by its original wording, not accepting it as a typo. Personal Communication, 2006, D. Kinley, IAEA public information, Vienna.

could have been a contributing factor, as opposed to the major cause, of additional cancers beyond the totals listed in the Tables (Beyea and Greenland 1999).

Summary of projected health effects.

Table 4-1. Projected number of cancers from the 1959 SSFL release computed at different likelihood percentiles. Scoping calculations based on 20,000 simulations. (Excess above millions of background cancers).

Likelihood Percentiles	Thyroid cancers [a)]	Non-thyroid cancers[b)]	All cancers[c)]
2.5%	0	0	0
5.0%	0	0	0
25%	0	2 (2)	8 (6)
50%	1	38 (23)	49 (33)
Mean	12	245 (130)	256 (140)
95%	45	1150 (630)	1200 (660)
97.5%	94	1790 (910)	1820 (950)

a) From both I-131 and I-133.
b) From 30 years of exposure to radiocesium groundshine. Computed using a risk coefficient averaged over recent epidemiological studies and the standard BEIR VII coefficient without a DDREF [average = 0.003 cancers per rem. 95% CI = (0.001, 0.0096)]. See text for derivation. Numbers in parentheses were computed using the BEIR VII coefficient for cancer incidence without a DDREF [0.0016 cancers per rem. 95% CI = (0.0007, 0.003)].
c) Note that the “All Cancers”-column is not the algebraic sum of the “Thyroid”- and “Non-thyroid”- columns

Table 4-2. Projected excess cancers from 30 years of whole-body radiocesium dose for different cutoff distances as a result of the 1959 SSFL release computed at different likelihood percentiles.[a] Scoping calculations based on 20,000 simulations. (Excess above millions of background cancers.)

	< 5 km	< 7.5 km	< 10 km	< 20 km	< 50 km	< 100 km[b]
1960 Pop-ulation[c]	5,729	57,484	124,152	457,698	3,429,762	8,318,350
Like-lihood Per-centiles						
2.5%	0	0	0	0	0	0
5.0%	0	0	0	0	0	0
50%	0	3 (2)	5 (3)	8 (5)	16 (10)	37 (23)
Mean	3 (2)	27 (15)	47 (25)	69 (37)	112 (60)	246 (131)
95%	15 (8)	133 (73)	230 (130)	330 (180)	540 (300)	1200 (635)
97.5%	25 (13)	220 (110)	380 (200)	525 (285)	840 (450)	1850 (940)

a) Numbers in parentheses were computed using the BEIR VII coefficient for cancer incidence without a DDREF. Numbers outside computed with the average value discussed in the text and in the footnotes of Table 4-1.
b) Includes return flow from the North, some of which will deposit beyond 100 km
c) Extrapolated values. The large number of digits listed do not reflect the true level of accuracy.

Ch. 5. Feasibility of epidemiological studies at SSFL.

There are a number of reasons why an epidemiological study around SSFL will be problematic.

1. Uncertainty of dose assignment. Uncertainty leads to exposure misclassification, which generally means a reduction in the ability to find an association between dose and disease (Carroll et al. 1998; Thomas et al. 1993). One form of uncertainty not discussed so far in this report involves movement of the exposed population during the exposure period . This is particularly important for groundshine from radiocesium, which is delivered at a slowly declining rate (16-year effective halflife). Although epidemiological studies that take individual residence histories can handle such movements in principle, missing data is inevitable, which complicates the analysis. The difficulties are even greater for ecologic studies that look at health statistics grouped by exposed areas.

2. Complicating pharmacokinetics. Perchlorate exposure from drinking water appears to have taken place in the area and may have been significant in magnitude at the time of any radioiodine releases. If so, risk assessment for radioiodine would be more complicated than normal, because perchlorate can reduce the uptake of iodine (Greer et al. 2002).

3. The SSFL site is conducive to relatively low offsite doses per unit I-131 released, which means a smaller number of cases are expected than would occur at a more typical site for the same release. There were times during the incidents at the reactor when released radioactivity would have carried above the inversion layer and thereby kept above people in the more crowded areas, only touching ground in low-population areas. In addition, limited vegetable harvesting took place during the most likely time for releases, and most of it took place some distance from the reactor, reducing contamination per square meter. Similarly, most production of milk occurred relatively far from the facility. These factors tended to reduce the total population dose, which was a fortunate circumstance for the health of the exposed population, but it makes future epidemiological analysis more difficult.

4. The total number of health effects determined from scoping calculations is spread out over a large population. Looking at a source term for radioiodine of a few thousand

Curies of I-131, which is typical of estimates that have been made, the total number of thyroid cancers estimated in this report has a 95%-confidence range of 0 to approximately 100, with a mean of 12. The median of the distribution was 1 cancer (i.e., in 50% of the 20,000 simulations, the number of predicted cancers was 1 or more). The projected cancers are spread out over such a large population (several million) that it could prove difficult to find them in the background of other thyroid cancers, including those from weapons fall out.

The possibility of finding an increased prevalence of low-birth-weight infants is a possibility based on a recent study of dental x-rays reported in JAMA (Hujoel et al. 2004). The authors found an odds ratio of 2.27 at a very low dose to pregnant women, namely 0.4 mGy (0.04 rads). For this study to be relevant to SSFL, it is necessary to make the (reasonable) assumption that the dose to the *thyroid* from the study subjects' dental x-rays caused the increase in risk of low-birth-weight infants. Short-term doses of 0.04-rads might easily occur around SSFL for a 20,000-Ci release of I-131, as found in the scoping calculations outlined in Appendix 2. Even the more moderate release estimates calculated by Thompson and the experts for plaintiffs in the Boeing litigation would lead to short-term doses of the order of 0.04 rads, when wind rose effects are taken into account. Since the prevalence of non-cancer thyroid disease is much higher than thyroid cancer, it might be possible to select cases with a higher probability of significant exposure and still retain sufficient numbers of study subjects. However, the study by Hujoel et al. has not yet been replicated; not surprisingly, it has been criticized (Brent 2005) and the authors have responded to the criticisms (Hujoel et al. 2005).

Another high prevalence disease that might be considered is (autoimmune) thyroiditis caused by I-131. A reanalysis of an earlier study at the Nevada Test site, including a reanalysis of the dosimetry, has found an association between fallout dose and thyroiditis in subjects exposed as children before the age of 8. The authors corrected for measurement error. These results were reported in a meeting abstract (Lyon et al. 2005) and their startistical significance noted in the revised dosimetry paper (Simon et al. 2006). The mean dose to the child was approximately 10 cGy or 10 rad.[25] Relative risk was 2.9 in the highest quintile, which had a dose of about 75 rads.[26] The association was

[25] The abstract contained a factor of ten error on dose (Hoffman 2005). Apparently, the authors meant cGy, not the mGy that appears in the abstract..

[26] The abstract contained a factor of ten error on dose (Hoffman 2005). Maximum dose was 1400 mGy (140 rads) , not 140 mGy (or 14 rads). According to Hoffman, the range of doses for the upper quantile

also significant in the second-highest quintile (20-40 rads) (Hoffman 2005). On the other hand, the Hanford Thyroid Disease Study (HTDS) found no statistically significant association between I-131 and autoimmune thyroiditis (Davis et al. 2002). The HTDS study did not correct for measurement error. It is also worth noting that the HTDS study looked at times since exposure longer than the corresponding times in the Lyon et al. study. Possibly, radiation speeds up the onset of autoimmune thyroiditis in a susceptible subset that is going to get thyroiditis eventually. At SRE, it might pay to look for excess thyroiditis by matching the time since exposure to the value in the study by Lyon et al. (up to 38 years).

Although HTDS did not find any association between fallout and autoimmune thyroiditis, it did report in the appendices a suggestive association for the more serious end state of thyroiditis, namely hypothyroidism, at least when alternate definitions of hypothyroidism were used. (Hypothyroidism occurs when about 80% of thyroid cells are killed, say by autoimmune thyroiditis.) This contradictory finding might be reconcilable with the failure to find an association with thyroiditis, if the prevalence of thyroiditis had saturated by 50 years after exposure, i.e., radiation simply sped up the onset of thyroiditis. Speeding up the time of onset of thyroiditis would have given more time for loss of thyroid cells and, hence, greater odds of reaching a hypothyroid state.

Even with the old dosimetry, the data from the Nevada Test Site on fetal exposures reveals a non-linearity in the association between fetal dose and excess thyroiditis (Lloyd et al. 1996). Consideration of fetal exposures seems appropriate, particularly since an epidemiological study of fetal outcomes could be combined with a study of low-birth-weight infants. Estimation of fetal doses from SRE releases, however, has not been considered for this report.[27]

Note that there are both experimental data and biologic reasoning that suggests a non-linear response is to be expected when measuring the dose response of radiation to autoimmune thyroiditis. First of all, the immune system is highly non-linear. As soon as self-tolerance is broken by radiation, a multiplicative effect would be expected to set in. Subsequent doses of radiation would be less influential. So, it is easy to explain a threshold with a subsequent flattening of the dose-response. On the other hand,

was 41 to 140 rads, which is taken for this report to imply a mean dose of 75 rads, which is the geometric mean of the two numbers.

[27] The NCI report (NCI 1997) gives information useful for computing fetal doses. See chapter 6, pages 6.18-6.21. Since the fetal thyroid only absorbs radioiodine after 10 weeks, with uptake peaking at 20 weeks, it would be possible to significantly narrow the birth date of the subjects to be considered.

averaging over individual variations would tend to linearize the dose response (Greenland 1995).

More difficult to explain is the actual downturn in response seen in the first major paper that provided evidence of a non-linear response, namely the 1994 paper by Nagataki et al. (Nagataki et al. 1994). The authors report a dose response for radiation and autoimmune thyroiditis that rises and falls. Although an update of this study (Imaizumi et al. 2006) no longer finds the effect statistically significant ten years later, the shape of the dose-response curve still shows a similar shape, when one plots the data. Another A-bomb study, (Yoshimoto et al. 1995), claims no effect based on what appears to be the assumption of a linear dose response, but the aggregated data in their Table II shows a doubled risk in the same dose region where Nagataki et al. found their peak response. It is not clear if the results would have been statistically significant based on a categorical analysis. A third A-bomb study, (Morimoto et al. 1987), did not look for non-linear responses and detailed aggregated data is not included in the dose range of interest based on the study by Nagataki et al.

A paper presented some years ago at a Chernobyl conference (Masyakin et al. 1997) shows a complex dose response for I-131 that is consistent with the Nagataki study, with a subsequent upturn at doses of 1-2 Gy (100-200 rad). However, this paper was never published in a peer-reviewed journal, but it is very similar to the dose response found for autoantibodies reported in the latest study from Chernobyl (Tronko et al. 2006a), which is clearly non-linear. As with other Chernobyl studies to date, Tronko et al. did not find an association with more advanced stages of autoimmune thyroiditis.

For example, the earlier Chernobyl study by Pacini and co-authors did not find evidence of clinical disease, but did find almost 20% of children in the exposed region to have thyroid autoantibodies, a precursor to thyroiditis (Pacini et al. 1998). The odds ratio was about 5. This 20% could encompass a large fraction of the subgroup of susceptible individuals. The average dose in the exposed children appears to have been around 75 rads, based on dose reconstructions in the same area (Gavrilin et al. 2004; Gavrilin et al. 1999). Thus, the NTS update is quite consistent with the study by Pacini et al. Gavrilin et al. indicate that 90% or more of the Chernobyl thyroid dose was due to I-131. On the other hand, because the relevant population at SRE was close to the facility, and because the I-133 inventory was relatively high in the core, I-133 would have made a significant contribution to persons at SRE, even though its half-life is only 21 hours. (Discussed in

Chapter 2.) The odds ratios of 5 found by Pacini et al. is greater than the 1.7 maximum value found in the case-control study of Tronko et al.

There is probably enough complexity in the autoimmune process to explain any dose response curve that has been seen – a situation that introduces ambiguity into any extrapolation to doses of interest at SRE. Presumably, a likelihood distribution would need to be chosen that would incorporate various dose response curves, both linear and non-linear. A linear extrapolation of an odds ratio of 2.9 at 75-rads to the odds ratio corresponding to 1-rad might suggest an odds ratio of 1.025[28] which would be difficult to detect in an epidemiological study. Extrapolating from the paper by Tronko et al., which has an odds ratio of 1.3 at 25-rads, would give an even smaller odds ratio (1.012).

What about doses from radiocesium? A similar low relative-risk situation exists for radiocesium, the other isotope that is generally of major significance in a reactor accident. Assuming a 600-Ci release of Cesium, which is the amount estimated to have been released at Windscale, the total number of delayed cancers scoped in this report is spread out over millions of people. Possibly, measurements of radiocesium in soil could identify locations where releases had focused cesium deposition, allowing a health study to focus its resources. Until then, an attemot to find a signal of SSFL radiocesium in health data would appear to be an expensive gamble.

Additional issues that complicate epidemiologcal research at SSFL, as pointed out in comments on a draft of this report by Steven Wing, include a) the possibility that the association between estimated doses from the 1959 SRE accident could be correlated (positively or negatively) with doses from other radiological or chemical releases that are risk factors for outcomes that might be chosen for epidemiological investigation, and b) the poor ability to document occurrence of diseases, such as non-malignant thyroid disease or chronic lymphocytic leukemia, that progress slowly, do not always lead to seeking medical care, and that may progress to be diagnosed as other diseases.

Given the many obstacles facing any epidemiological assessment that relies on exposure categorization, a null result because of low doses and measurement error is a real possibility. It may be impossible to tell if a negative finding is due to a small release or inadequate dose assessment. In any case, non-cancer health endpoints seem most promising, low-birth-weight infants and possibly autoimmune thyroiditis from fetal exposure. However, there is considerable controversy for both of these endpoints as to the correctness of the associations found at low doses.

[28] Assuming the prevalence was 15% at 75 rads, with a 5% prevalence at zero dose

Appendix 1. **Inferences that can be drawn from the actions (or lack of them) taken by Atomics International.**

To improve estimates of model parameter distributions, this chapter attempts to answer questions that are needed for Bayesian updating. For instance, what state of knowledge about reactor accidents, off-site monitoring, and health effects of radiation exposure can be presumed for management of Atomics International prior to the July 1959 accident at SRE? How do the actions of AI compare to what would be expected from a public health perspective? Is there evidence of a cover-up? Answers to these questions can help, for instance, in estimating the likelihood that the operator would have covered up releases of different magnitudes.

State of knowledge of AI management.

In discussing the state of knowledge of management of Atomics International, it is necessary to take note of the special background and expertise of its general manager, Chauncey Starr, who was also vice-president of the parent company, North American Aviation (NAA) (Anon 1958). Starr is now deceased. Before WWII, he was a research fellow in physics at Harvard University, working with Nobelist P.W. Bridgman in the field of high pressures (LLNL 2000). During the war, he was assigned to the Manhattan project, working initially at the Berkeley Radiation Lab. The researchers there were at the forefront of the use of radioisotopes, including radioiodine, in medicine. As early as 1941, researchers at Berkeley lab had identified radioisotopes getting into cows' milk (Erf and Pecher 1940; Pecher and Pecher 1941). For his later war years in the Manhattan project, Starr was part of a technical group at Oak Ridge (HOFEST 2005). These war-time experiences helped to make Starr a knowledgeable and well-connected nuclear scientist. He was familiar with human aspects of radiation exposure—experienced enough to be granted an appointment in 1967 at the UCLA medical school as a clinical professor of radiology (UCLAE 1966). At UCLA, Starr eventually became the dean of the engineering school, another indication of both his prominence and his capabilities (HOFEST 2005).

The significance of Starr's management performance at Atomics International goes beyond SRE. Starr was simultaneously the president of the American Nuclear Society (Anon 1958). He later became the president of the Electric Power Research Institute (HOFEST 2005) at a time when it was heavily involved in nuclear issues. The lessons he learned at SRE presumably contributed to the posture of these groups on public notification and cover-ups. Decisions made by Starr at SRE helped to set the tone for the entire US nuclear industry.

Knowledge of reactor accident consequences. By 1955, nuclear reactor designers had a sophisticated knowledge of the potential consequences of nuclear reactor accidents. The following quotes come from papers presented at a major 1955 United Nations' conference, which was attended by Chancey Starr and W.E. Parkins, another employee of North American Aviation, the parent company of Atomics International[29].

> "The possible consequences of a large scale release of radioactivity from a nuclear reactor in the event of accident has long been recognised as an important factor in the development of nuclear power" (Marley and Fry 1955).

> "It is still evident from Table I that radioactive poisons are more hazardous than chemical poisons by a factor of something like 10^6 to 10^9. This is such an enormous factor that radioactive poisons essentially must be considered a qualitative new kind of problem" (McCullough et al. 1955).

Since one of the authors of the above quote was Edward Teller, the father of the hydrogen bomb, these words cannot be attributed to "nervous Nellies." At the time, Teller was a member of the Atomic Energy Commission's, Advisory Committee on Reactor Safeguards (Weil 1955).

Discussion of hazards of nuclear power plants extended beyond technical meetings. George Weil writing in *Science* in 1955, said,

> "Reactors are not only expensive machines; they are also potentially hazardous machines, substantially more so, in fact, than any industrial machines with which we are currently familiar…. In the absence of realistic information based upon experience, those concerned with the design of nuclear power plants and those responsible for their operation must make the most pessimistic assumptions with regard to potential accidents and their consequences. In view of the potential seriousness of the off-site hazards, it is reasonable to place the burden of proof to do otherwise upon the sponsor of the plant" (Weil 1955).

Clearly, much was expected from the designers of nuclear power plants; publicity about an offsite release would have been very damaging to the nuclear industry, which was then in formation.

Nuclear scientists and nuclear engineers of the time also had an appreciation that consequences of radiation exposure went beyond acute events and extended to the induction of tumors in a subset of the exposed:

> "Also because our estimates were intended for statistical purposes (and because of lack of data) we have omitted consideration of late tumor production, which may appear in a small percentage of exposed individuals……. In principle of

[29] Both Starr and Parkins, another AI employee, presented papers about the SRE at the conference (Parkins 1955; Starr 1955). Also, at the meeting was Mark Mills, of Livermore Labs, a former employee of NAA, who presented a paper on the safety of nuclear reactors (McCullough et al. 1955). When Mills was an employee of NAA in 1951, he presented a paper on the hazards of research reactors (Lark-Horovitz 1952). NAA staff had obviously given considerable thought to reactor hazards over the years.

> course a tumor might be induced by a very small dose, but the chances are small" (Kuper and Cowan 1958)

Methods for making quantitative estimates of reactor consequences, including dispersion calculations were also available by 1955 (Marley and Fry 1955; Parker and Healy 1955), as they had been since the development of the atomic bomb in World War II. Monitoring of releases was also well understood by 1955, not just because of experience since 1944 with releases from the weapons production reactors and spent-fuel dissolvers, but also because of a release in July of 1954 at Argonne National Laboratory (Griffiths et al. 1955).

The dominant importance of radioiodine in reactor releases and broken fuel elements had been understood since the Manhattan project of World War II (Chambers 1943; Dreher 1944). Chauncey Starr would have known of its importance from his experience with the Manhattan project.

Radioiodine health effects. What knowledge would Starr have had about the health effects and physiology of radioiodine? The use of radioiodine in studying the thyroid was reported in 1938 (Hertz et al. 1938). Radioactive I-131 was being used in patients and "normal" controls by 1939 (Hamilton and Solely 1939). The ability of the thyroid to concentrate radioiodine by at least a 1000-fold was known at least by 1940 (Hertz et al. 1940).[30] Radioiodine uptake by fetuses was known by 1942, as cited in (Hamilton 1942). In 1942, Bachman and Dichter reported a dose-independent induction of hypothyroidism in some patients following the administration of radiation therapy:

> "The occurrence of postradiation hypothyroidism frequently appears to be independent of the roentgen-ray dose delivered to the hyperactive thyroid" (Bachman and Dichter 1942).

Today, such dose-independent, radiation-induced hypothyroidism would be ascribed to an immune response (Lundell and Holm 1980; Lundell et al. 1981), probably resulting from the end state of autoimmune thyroiditis. However, Starr in 1959 may not have known about this 1942-research, which does not seem to be heavily cited.

By the 1950s, evidence began building of the carcinogenic potential of I-131 in causing thyroid cancer. For instance, in 1950, Doniach found numerous excess benign tumors (adenoma) in rats injected with I-131 (Doniach 1950). Two years later, in 1952, Goldberg and Chaikoff found cancer in 7 out of 28 groups of rats injected with I-131 (Goldberg and Chaikoff 1952).

[30] In 1942, Hamilton states that the concentration factor can be 10,000 (Hamilton 1942).

These tumor studies were carried out at high doses, which ironically is known today to actually reduce the induction rate, because of thyroid cell killing.

Theshold for cancer. Although the studies in the early 1950s proved that radioiodine could cause cancer (in mice), there was a common view at the time that a dose threshold existed for cancer. This view was particularly common among nuclear engineers and physicists (Caron 2003), as well as among medical doctors using radiation for treatment and diagnosis (Nickson 1948). In the medical context, a health-related reason was given for neglecting low-dose effects:

> "The carcinogenic properties are not apt to cause difficulty for some years, if ever, since the life expectancy of the properly selected patient is short. Indeed we should be grateful if the life of an otherwise incurable patient could be prolonged sufficiently to occasion concern about the possibility of tumor induction" (Nickson 1948).

But, if there were a threshold, what was its numerical value? Knowledgeable medical experts, like Nickson, put the threshold at 25 rads, recognizing that the true level was very uncertain:[31]

> "The fact that no better assumptions can be made emphasizes the necessity for collecting usable data on animals and humans which ultimately will permit more reliable estimations of the amount of radiation that the thyroid gland may safely assimilate, e.g., the amount which is not carcinogenic" (Nickson 1948).

In addition, there was an early recognition that future research might change everybody's views about thresholds:

> "The majority of radiation effects are thought to be of the threshold type. It may be that, as more delicate indicators are found to measure effects, more of them will be seen to be of the nonthreshold type." (Cantril 1951)

The issue of a threshold was hotly debated in the science press beginning in 1957 (Brues 1958; Hollifeld 1958; Kimball 1958; Lewis 1957). A consensus quickly developed among standard setters that, regardless of what future studies might show, public safety required assuming a non-threshold model at the time (Anon 1960; Neuman 1958).[32] Starr must have been

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[31] "Insofar as is known, no report exists of chronic damage to the skin following exposure to 25 roentgens. If the time factor is ignored and the transposition from skin to thyroid gland is made, the tentative maximum permissible exposure to the gland would then be 25 roentgens, or 0.19 microcurie of I-131 per gram of thyroid.….It is appreciated that the bases for selecting the factors leading to the figure of 25 roentgens are at best shaky and at worst irrelevant." (Nickson 1948).

[32] Although the report cited (by an NCRP committee) is dated in 1960, committees like this only adopt positions that are well established, which means that the "prudent" position would have been well known to Chauncey Starr before the accident at SRE.

familiar with the debate, including the position that had developed in the National Commission on Radiological Protection that prudency required the assumption that there was no threshold (Anon 1960). Ultimately, Starr takes an appointment in the Medical School at UCLA, which suggests he must have had a long familiarity with the literature on the impacts of radiation on health. Certainly, Starr should have known that no specific threshold had been identified for cancers caused by radioiodine and that lower doses might simply shift the occurrence to later times (Nickson 1948). Even if he adopted the threshold theory, it would seem he would want to have measurements of any radioiodine releases at SRE to see what doses people might have received.

Continuing with the discussion of the state of scientific knowledge prior to 1959, it should be noted that the importance of radioiodine and the cow-milk pathway was certainly known by 1956 (Comar and Wasserman 1956). That radioisotopes in general entered the milk pathway was known a lot earlier (Erf and Pecher 1940; Pecher and Pecher 1941). In May of 1959, at congressional hearings on fallout, Caltech's E.B. Lewis presented estimates of thyroid cancers expected from I-131 fallout under the linear hypothesis (Eisenbud 1963; Lewis 1959a).[33] However, staff at AI may not have been aware of these calculations prior to the July accident at SRE.

***Windscale accident*.** Because the authorities at Windscale were largely open about what happened[34] and quickly published their findings in the scientific literature (much of which appeared by 1958 and early 1959), a great deal was learned about reactor accidents – knowledge that the operators at SRE appear to have ignored.

Eisenbud has the following comments on the behavior of the Windscale group, which made thousands of offsite measurements, as well as ensuring that restrictions on milk consumption were put into place:

> "If we have learned much from this experience, it is due to the exemplary manner in which the British scientists conducted their investigations and published their

[33] Note that Lapp in his review of the 1959 hearings does not mention testimony by Lewis (Lapp 1959), which suggests that Lewis did not get a prominent place on the hearing schedule.

[34] Although the British were open about most of what occurred at Windscale, parts of the report of the official inquiry into the accident were censored at the highest levels of government (Dickson 1988). Also, the authorities failed to account in health assessments for the release of polonium-210, which was being produced by irradiation of bismuth for use as a trigger for nuclear weapons (Crick and Linsley 1984; Urquhart 1983). It should be said, however, that Dunster does mention in 1958 the release of polonium, at least briefly: "Smaller quantities of other fission products such as caesium-137, strontium-89 and 90, ruthenium-103 and 106, zirconium-95, niobium-95 and cerium-144 together with polonium-210 were also released" (Dunster et al. 1958). Because the polonium was being produced during the deliberate irradiation of bismuth, it is not an isotope that is relevant to the SRE accident in 1959.

> reports. The accounts of this experience published by Dunster, the Medical Research Council, and others are to be admired for their thoroughness and promptness" (Eisenbud 1963).[35]

Numerous scientific papers resulted from the studies of the Windscale accident, many of which appeared in print by 1958 (Chamberlain and Dunster 1958; Dunster et al. 1958; Maycock and Vennart 1958; Stewart and Crooks 1958) dramatically increasing scientific knowledge of reactor accidents, including the importance of radioiodine. Moreover, actions were taken to protect the public health. For instance, restrictions were put on milk consumption for about 6 weeks to reduce radioiodine doses (Dunster et al. 1958).[36] As a byproduct of this action, an important public health lesson was learned: milk restrictions do indeed work to reduce doses (Crick and Linsley 1984). And, although milk restrictions were made applicable to the general population, the authorities were really only targeting children,[37] proving that certainly by 1957, knowledgeable nuclear scientists knew before Chernobyl which segment of the population was at most risk from radioiodine releases. What should have been of particular interest to staff at SRE would have been the limits set by British authorities on I-131 in milk (0.1 microcurie per liter) and the gamma levels on pasture grass that correlated with such a milk concentration (1000 nCi/m2 of Iodine-131)[38, 39] (Dunster et al. 1958). These rules of thumb would have been useful

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[35] It is interesting to note that the British Medical Research Council had concluded in a report issued in June of 1956 that the incidence of leukemia was proportional to dose (Osborne 1958). This contrasts with the position of a panel of the National Academy of Sciences in the United States, which concluded in a report also issued in June of 1956 that it was not possible to say whether or not there was a threshold (Osborne 1958). Possibly, the strong position taken by researchers in the UK helps to explain the response at Windscale. By 1958, concern had arisen that the National Academy Report was not as thorough as the BMRC's report. For instance, Congressman Chet Hollifeld, who had held hearings on fallout, wrote, "I would like to suggest at this point that the National Academy of Sciences revise and bring up to date its report on radiation. The data in this report are now more than 20 months old. There is competent opinion to the effect that British scientists did more detailed research on strontium-90 than the National Academy, and that their findings are less optimistic." (Hollifeld 1958).

[36] This is not meant to imply that the British restrictions were perfect. The target radioiodine dose level was 20 rads (Dunster et al. 1958), which we know today would significantly increase the incidence of thyroid cancer among children (Cardis et al. 2005a; Jacob et al. 2000; Ron et al. 1995; Shakhtarin et al. 2003). In addition, Wright has argued that in areas receiving high radiocesium deposition from Windscale, sheep had levels of radiocesium above current intervention levels as late as 1985 (Wright et al. 2003).

[37] "Medical evidence has indicated that cancer of the thyroid in children had sometimes followed radiation doses of 200 rads but there was no information about the possible incidence at lower doses. It was therefore decided to control the consumption of milk to avoid doses in excess of 20 rads to children. It was thought administratively impracticable to control milk for children while allowing its consumption by adults, so the calculated permissible level was based on data for children but applied to the whole population" (Dunster et al. 1958).

[38] Note that the relationship between gamma activity and milk concentration might have been different at SRE, but the Windscale rules of thumb would have been a place to start.

[39] Note that the total amount of radioiodine deposited in the SSFL area from all weapons tests has been estimated to be in the 500-1000 nCi/m^2 range (NCI 1997), Figure TS.1, p. Technical Summary page 4. Average concentrations would have been much lower due to radioactive decay.

for scientists and technicians at SRE, had they been interested in checking for offsite contamination.

Windscale was not the only accident prior to July of 1959 for which ground measurements had been reported and contour maps made public. Details of contamination following a 1954 accident at the National Reactor Testing Station in the State of Idaho were circulated widely to reactor specialists in 1955 at the United Nations conference that Chauncey Starr attended (Griffiths et al. 1955). On the other hand, the openness shown at NTRS was not comparable to that shown at Windscale, because the NTRS authors could claim that the contamination did not extend beyond the exclusion boundary. We do not know how open they would have been had their data extended out further to areas where members of the general public were present. In fact, a deliberate decision may have been made to truncate contour levels so no contour extended beyond the exclusion boundary.

What can be said about the SRE operator and staff in comparison with their counterparts at Windscale and the NRTS? The SRE operators knew that the reactor was behaving strangely. They knew there had been fuel damage, but they did not take, or else suppressed, relevant measurements. Did society learn anything from them about offsite implications of reactor accidents from SRE? No. Were the SRE scientists thorough? No, not to public knowledge. Any measurements they may have made must have been hidden.

Were the SRE scientists prompt? Hardly, given their secrecy about the entire event. Furthermore, no medical council or equivalent public health body was called in. An opportunity was thereby lost to help educate public health agencies about responding to nuclear incidents. No consideration was given to imposing restrictions on milk consumption or cow feeding practices. The action of the SRE operators was the opposite of “exemplary.” An opportunity was lost, regardless of the true magnitude of the release. If Atomics International actually found no offsite contamination, the measurements to support such a conclusion should have been reported. After all, data could have been collected and analyzed, if for no other practical purpose than to determine the limit of detection in the US in the midst of fallout deposition. Null evidence would by itself have been very valuable.

Had there been a large release kept secret at SRE, it would have been consistent with earlier behavior in the United States. For instance, at the Hanford weapons production facility, 9,000 curies of Iodine were secretly released in 1949 as part of the so-called, “Green Run” (Napier et al. 1994; Robkin 1992). (9,000 curies is about half of the amount that was released at Windscale.) No public notice was given and no milk restrictions were put into place. Perhaps, the difference in behavior between the US and the British can be ascribed to the timing:

knowledge of the sensitivity of children's thyroids to radiation was first reported in 1950 (Duffy and Fitzgerald 1950). However, the culture of secrecy was so strong in the United States, that even with such knowledge, no milk restrictions may have been put into place. Secrecy at SRE about an I-131 release would also have been consistent with the policy of the United States government to ignore the health consequences of fallout I-131. For instance, no restrictions on milk consumption were put into place during weapons tests, when copious amounts of I-131 were deposited on pasture across the United States (Hoffman et al. 2002).

Did the SRE operators have the technical capability to estimate and measure exposures? The answer is, "yes." Such knowledge was available since the WWII Manhattan project. Suttons book on micrometeorology was published in 1953 (Sutton 1953). A March 1960 report from UCLA on its training reactor tells in detail how to make exposure calculations given an assumed release (Maclain 1960). Atomics International was closely allied with UCLA. The SRE site was picked to be near a university (Chalker 1949). And although March of 1960 post-dates the July 1959 accident, it would be hard to argue that the knowledge necessary to calculate exposures was suddenly gained at UCLA subsequent to 1959. Thus, SRE operators should have known that even with conservative assumptions about meteorology, a 1000-Ci release was capable of causing 1000 nCi/m^2 of contamination at 30 km and, hence, a Windscale limit deposition at 30 km. Such calculations should have triggered offsite measurements.

The total amount of radioiodine deposited in the SSFL area from all weapons tests has been estimated to be in the 500-1000 nCi range/m^2 (NCI 1997), Figure TS.1, p. Technical Summary page 4. Average concentrations would have been much lower due to radioactive decay. Thus, with simple groundshine measurements, SRE technicians could have detected levels of radioactivity requiring restrictions on cow feeding routines and milk consumption according to the Windscale protocol. Actual measurements of fallout radioiodine in milk during the period in California ranged from 10 to 230 pCi per liter (Lewis 1959b), (Campbell et al. 1959). This is well below the levels (100,000 pCi/l) that would have triggered milk restrictions under the Windscale standard.

Moral responsibility. The main purpose of this chapter is to assess the information that could be used in a Bayesian analysis of parameters-- parameters that might be used in a dose reconstruction underlying an epidemiological study. The actions of the operators at SRE must be judged from a moral perspective in the context of the times. Possibly, Starr's judgment was affected by the wartime practice of covering up offsite exposures and the medical community's tabling of concern about long-term effects of radioiodine exposures in patients because of the immediate benefits (Nickson 1948).

Moreover, the SRE accidents occurred in the heat of the cold war, when government policy appears to have been to cover up health impacts of fallout (Hoffman et al. 2002), presumably to prevent a weakening of public nerve in the war of nerves with the Soviet Union. Examining whether or not a cover-up by the federal government concerning weapons fallout might justify a cover-up by contractors at SSFL who might be liable for damages one day, is beyond the scope of this report.

Now public health officials in the United States and California are not blameless for ignoring Windscale. Windscale should have taught them what colleagues in UK realized:

> "Because the production of radioactive material is now many thousand times greater than it was in the last decade, the public-health authorities cannot afford the complacency of leaving all control to the producers of fission energy and to the atomic physicists" (Madge 1957).

On the other hand, even had they been notified of the SRE incident, public health officials would have faced a dilemma. Should they risk frightening the public, which itself can lead to health effects, or should they impose restrictions on milk as a preemptive measure to prevent some thyroid disease? In any case, it is hard to imagine that public health officials would not have insisted on extensive testing, had they been notified of the incident and the potential hazards of a release of radioiodine.

Bayesian updating. To perform a parameter update using Bayes theorem based on historical information, it is necessary to estimate the likelihood that the operator would have covered up various magnitudes of releases. Presumably, the larger the release, the greater the likelihood of a cover-up. However, it is possible that the operators would have hidden even the smallest release, thereby negating the quantitative value of evidence of a cover-up. For instance, in the press release, SRE management denies any release, even release of noble gases. So the question arises, "Were they any more likely to cover up a 1-Ci release than a 1000-Ci release"? If the chance of a cover-up is already nearly a 100% for a 1-Ci release, then the historical information will not be useful for updating the source term.

However, the operators did not hide in official reports the fact that some radioactivity escaped, suggesting that there is a difference between their public statements and their written reports. Furthermore, based on the experience at the National Reactor Testing Station in Idaho in 1954 mentioned earlier (Griffiths et al. 1955), discussion of releases confined to the site were apparently perceived differently within the reactor community than offsite releases. Apparently, on-site releases were not so shocking as to require complete secrecy. Thus, it seems likely that, had the SRE operators found contamination only on-site, they would have been more likely to reveal it in official reports. To make a judgment that contamination did not extend offsite, of course, would have required a set of measurements—measurements that are not in the public record. As a result, there is a good chance that SRE staff made measurements showing off-site contamination, but a decision was made by management not to discuss the results outside the company. Based on this reasoning, the likelihood of a release of 1-CI of I-131 has been discounted by those, such as ATSDR, who relied solely on AI reports. As discussed in Chapter 2, the relative likelihood assigned to the ATSDR estimate was reduced by about a factor of three.

Appendix 2. Methodology for estimating doses and health effects from a hypothetical release from the SSFL in July of 1959.

In this appendix, the methodology for estimating consequences of a hypothetical release during the July 1959 event at the SRE are detailed. Given the complexity of the site, only scoping calculations are possible. The "source term" chosen for analysis is identical to the 20,000 curies of I-131 and the 600 curies of radiocesium that were released during an accident that occurred a few years before the SRE event at Windscale in the UK. Doses and consequences for other release magnitudes, such as those appearing in the source-term distributions described in Chapter 2, are obtained by scaling the results in this appendix.

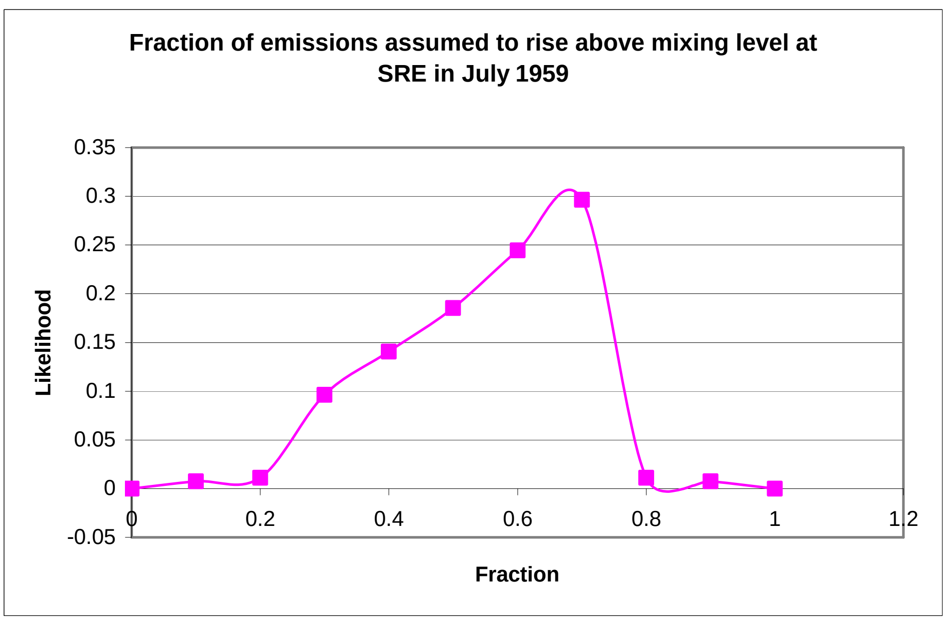
The 1957 Windscale accident was well publicized in the science press and its consequences analyzed in scientific and engineering articles (Madge 1957), (Dunster et al. 1958), (Chamberlain and Dunster 1958), (Stewart and Crooks 1958). The managers of the SRE would have been familiar with it. At Windscale, the operators initially ignored signs of problems, only to learn later that there had been a release. A Windscale-magnitude release has been chosen for this appendix, because it represents the scale of an accident that should have been considered by the SRE managers both for planning purposes before the accident and for deciding whether or not to notify public health authorities at the first signs of trouble. Note that in using the scoping calculations in this appendix to estimate doses and health effects from actual source-terms tied to engineering analysis of the July 1959 SRE accident, it would be necessary to multiply the I-131 source term by (approximately) a factor of 1.5 to effectively account for the I-133 that would have been released simultaneously, as discussed in Chapter 2.

Although there were other releases from SSFL besides the July 1957 accident, there is less information available about them. They have not been considered in this report. Given the complex terrain and proximity to ocean breezes, it is a challenge to estimate consequences of any release from the SSFL site. Figures A2-3 and A2-4, located at the end of the report, illustrate some of the complexity of the terrain. SSFL sits atop a mountainous outcrop, with lowlands and ocean to the South and valleys to the North, which run from East to West.

To a first approximation, estimates of health effects resulting from other assumptions about releases can be obtained from the Tables in this appendix by scaling, isotope by isotope, from the ratio of curies released. To expedite such scaling, consequences from radioiodine have been separated from consequences for radiocesium. While it is true that different assumptions about the accident could lead to different assumptions about the temperature of the air in which the radioactivity was carried to the outside air, and therefore the extent to which the contaminated air rose, there does not exist at this point any relevant information for *any* accident scenario that could be used to refine dose calculations by accounting for plume rise.

Based on information available today, for instance, it is necessary to make *ad hoc* assumptions about the fraction of material that ends up above the atmospheric inversion layer, which would have kept the radioactivity "aloft" from reaching ground level for many tens of kilometers, therefore sparing the most highly populated areas of Los Angeles from doses they might otherwise have received. True, the presence of complex terrain can "can lead to efficient vertical transport of airborne pollutants through stable layers of air such as nocturnal ground-based inversions" (Reible et al. 1981).[40] And, vertical uplift can produce a subsidence on the return flow (Lu and Turco 1995). Nevertheless, some fraction of released radioactivity will stay aloft, for which a number must be chosen, if consequences are to be assessed.

Figure A2-1a



In this appendix, it is assumed that approximately 50% of the released radioactivity on average is sent aloft, only touching down (fumigation) beyond 60 km (Wakimoto and McElroy 1986). The elevated component of the release only makes a small contribution to the population

[40] Such mechanisms for bringing SSFL radioactivity in contact with people were not discussed in assessment documents produced by the SSFL owner. For example, (Rockwell 1976).

dose, except in cases where the deposition velocity is very high. Elevation of radioactivity released from the SSFL can be expected, for example, as sunlight warms up the mountains, leading to mountain breezes converging at the top of the Santa Monica Mountains (Poulous and Pielke 1994), (McElroy and Smith 1991), (Ulrickson and Mass 1990b). Figure A2-1a shows the full likelihood distribution assumed for this parameter. The mean of the distribution is 0.55. To represent this distribution in Monte Carlo calculations used to combine uncertainty, it is approximated by a triangular distribution starting at 0.2, peaking at 0.7, and stopping at 0.8.

It is also assumed in this appendix that the release does not vary with time of day. Variations about this assumption are implicitly included in the distribution shown in Figure A2-1 for the fraction of emissions that rise above the mixing layer, because nighttime releases tend to descend from the mountain, whereas daytime releases tend to rise. It is further assumed that the release occurs on days that are typical of the period from July 12th to July 26th 1959, which means typical Los Angeles summertime weather. Were the bulk of the release to have occurred during the few days when weather conditions were atypical, a separate meteorological analysis might be necessary. Typical weather for the period means onshore winds pushing into the Oxnard Plain and connecting valleys, with flow from the south into the San Fernando Valley through the Glendale pass (Lu and Turco 1995), (Chalker 1949). As a result of these flows in opposite directions, air movements from Oxnard through to the San Fernando Valley, when they occur can be expected to converge against flows from the South, causing uplift in the middle of the San Fernando Valley (Lu and Turco 1995), (McElroy 1986). Thus, locations in the eastern part of the San Fernando Valley would have received lower exposure than they would have in the absence of convergence. [41]Wind data at Oxnard during the accident period (NCDC 1959b) indicates a period of calm occurred (zero average wind speed) for 20% of the time (5 hours per night) during the accident period. The most likely explanation for this extended calm period, given its occurrence at night, is the strengthening of drainage flows down the Valleys that enter the Oxnard Plain. When the drainage flows are strong enough to compensate for the onshore flow on average, a calm results (Boucouvala and Bornstein 2003). Drainage flows are examples of divergence, the opposite of convergence. To supply the drainage flows, air must descend from aloft. It is assumed in this appendix that during the nighttime period when calms existed, drainage flows carry all released radioactivity from SSFL down from the Santa Monica Hills,[42] with half going

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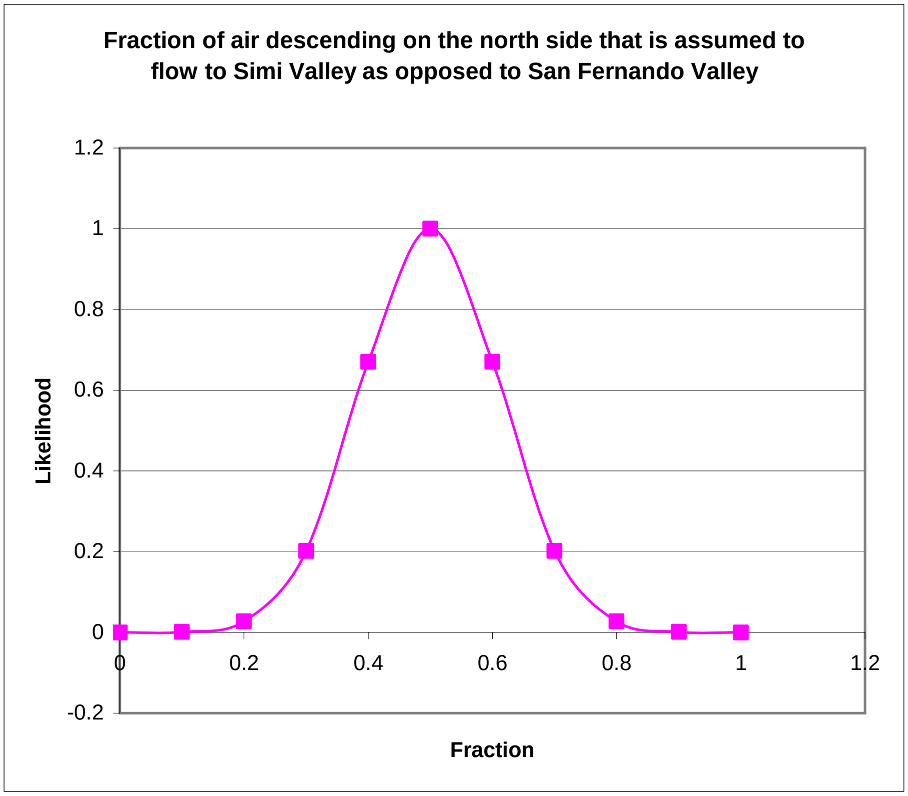
[41] Uplift in the Oxnard plain has not been considered, because no evidence of its occurrence was found in the literature.

[42] "… tracers released immediately above the drainage flows near the ridge top did reveal considerable mixing between the transition layer flows and the underlying surface drainage flows." (Guidksen et al. 1984).

into the valleys north of the Santa Monica Hills and half descending to the south of the mountains.

Drainage flows existed for longer periods than 5 hours per day, but it is not clear that the divergence flow bringing air down onto the Santa Monica Mountains had fully developed at other times.

Figure A2-1b.



As for the approximately 50% of SSFL emissions assumed during periods of calm to descend into the valleys on the northern side of the Santa Monica Mountains, half of that fraction is assumed to move towards Oxnard and half into the San Fernando Valley, where some fraction of it would deposit in and around dairies and pasture in the Western part of the Valley. The likelihood distribution assumed for this parameter is shown in Figure A2-1b. At some penetration distance into the San Fernando valley, the flow carrying SSFL radioactivity would rise aloft upon converging with air entering the valley from the south, which was continuously arriving during the July-1959 accident period, as indicated by contemporaneous wind data from the Burbank airport (NCDC 1959a).[43] Some fraction of the flow carrying SSFL radioactivity might recirculate, a circumstance which would also serve to reduce doses beyond the convergence point.

Drainage flows have the potential to "recycle" pollution (Strimaitis et al. 1991), (Hanna et al. 1991), and depending on their strength can strongly affect how far downwind each "tongue" of pollution can reach before looping back for the return flow (which may drift to locations other than the starting point). Drainage flows have the potential to complicate dose estimates by geographic region. Consideration of such complications cannot be made at this time due to lack of data. In this appendix, effects of drainage flows are accounted for when their impacts can be deduced from general meteorological principles, whose application to the Los Angeles Basin have been deduced from tracer or tetroon[44] measurements and discussed in the scientific literature (Hanna et al. 1991), (Bastable et al. 1990), (Wakimoto and McElroy 1986), (Guidksen et al. 1984), (Reible et al. 1981), (Angell et al. 1966). In particular, drainage flows are assumed to bring radioactivity to vegetables in Oxnard and to bring radioactivity to both vegetables and dairies in the western San Fernando Valley. The flow to San Fernando Valley dairies makes a major contribution to the thyroid population dose from milk, accounting for 60% of it. However, the dose from milk is overshadowed for most of the simulations presented in this appendix by the inhalation dose, for which drainage flows are ignored.

Inhalation dose, as well as deposits of radioactivity on dairies outside of the San Fernando Valley, have been computed under the naïve assumption that radioactivity released under the inversion layer travels in a straight line, with frequency of directions set by onsite data recorded from 1994-1997, based on published wind rose charts located in a PowerPoint presentation by a UCLA group (Chinkin et al. 2003). Access to the full meteorological data set, including a breakdown of wind frequencies by atmospheric stability class, was not available, but the information in Chinkin et al. was sufficient to establish the basic wind frequency data as a function of wind speed. A request for the full meteorological data set was turned down by the UCLA group, because after making inquiries they were told that Boeing claimed the data was confidential (Thompson 2004a). Since the full meteorological data set had been given to ATSDR,[45] Gordon Thompson filed a Freedom of Information request with ATSDR. However, ATSDR responded that they could find no documents pertaining to the request (CDC/ATSDR 2004). Thompson also made a FOIA-request to DOE. DOE responded similarly (NNSA 2003).

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[43] This flow from the South is difficult to simulate with numerical models. (Ulrickson and Mass 1990a).

[44] Tetrahedral balloon.

[45] See page 22 of ATSDR's 3 December 1999 Draft Preliminary Site Evaluation for SSFL. The 4th paragraph on this page discusses SSFL meteorological data that were supplied to ATSDR by Rocketdyne.

The straight-line assumption is rather heroic for the Los Angeles Basin, given the many air layers with alternate flow directions that can exist in the Basin (Boucouvala and Bornstein 2003), (Lu and Turco 1995), (Bastable et al. 1990), (McElroy 1986) and which can split plumes (Reible et al. 1981), (Hinds 1970). Furthermore reliance on surface winds is generally inadequate in complex terrain,[46] but there is no alternative at this time, if scoping calculations are to be made. *Caveat Emptor*. Even if hourly data were available, which would have allowed use of a puff model simulation, the model would still have been naïve. On the other hand, the computation of population dose, as opposed to computation of individualized doses, is likely to be reasonably robust to model error.

Because the SSFL terrain and coastal location make modeling so complex, results in this appendix are presented for a range of model parameters.

Table A2-1a Scoping Scenario A (Key assumptions and parameters.)		
Meteorologic model	Wedge model, with results computed for a range of deposition velocities and wedge heights	Deposition velocity is very uncertain (USNRC 1995), even at inland sites with flat terrain.
Wind flow	Naïve straight-line assumption, with sector percentages based on hourly data for 1994-1997, apparently with calm periods, if any, excluded	Using annual data extracted from graphs presented in a report of the UCLA study (Chinkin et al. 2003). Note that wind rose data may be different for the July 12-26 period.
Release of radioactivity	20,000 Ci of I-131; 600 Ci of Cesium 137 (and corresponding amounts of Cesium-133)	Equal to the measured release for the 1957 Windscale Accident, which should have been the planning numbers used by Atomics International before, during, and immediately after the 1959 accident.
Percent of release above mixing layer	50%	Lofted radioactivity has an impact on dose only for very high deposition scenarios.
Percent of release below mixing layer	50%	Sensitivity of dose to this parameter can be estimated by varying total release.
Population data	1960 census tracts, extrapolated from 1970-2000 data.	Leads to a modest underestimate for cesium population dose, due to population increases after 1960.
Building penetration factor	0.8	Range: 0.6 to 1, uniform distribution. Incorporates time spent outdoors.
Fumigation distance	60 km	Distance before radioactivity released above the inversion layer is allowed to contribute to dose. Wedge height is 1000 m for fumigated material.

[46] "Predictions of pollutant transport based on surface wind measurements cannot normally be expected to provide good results in complex terrain." (Reible et al. 1981).

Table A2-1b. Cow-pathway parameters taken from NCI fallout report (NCI 1997).[a]

Pasture grass consumed by cows in Southern California	1 kg/day	Summer rates. NCI report, Appendix 5.
Feed consumed by cow	17 kg/day	NCI report, Table 4.2, page 4.11.
Feed to milk transfer of I-131	0.004 nCi/(liter/day)	Geometric mean. NCI report, Page 4.21.
Cow drinking water contribution	0.007 (nCi/(liter/day)/(nCi/m^2)	Median. NCI Report, page 4.40 table 4.9.
Cow breathing rate	130 m^3/day	Average. NCI Report, page 4.37.
Milk: delay between production and distribution	2 days	For milk produced in LA County. Add 1-day for milk in other counties. NCI report, Page 5.7.
Ratio of 1959 to 1955 pasture acres in LA County	0.263	Needed to update from 1955 data in NCI report to time of accident.
Ratio of iodine on stored hay	0.04	NCI page: 4.38.
Cow intake to milk transfer	Same as "Feed To Milk Transfer"	NCI page 4.37.
Consumption of stored hay	16 kg/day	Difference between total feed and pasture grass consumed. Thus, relative contribution of stored to pasture grass here = 0.04*16/1 = 0.64

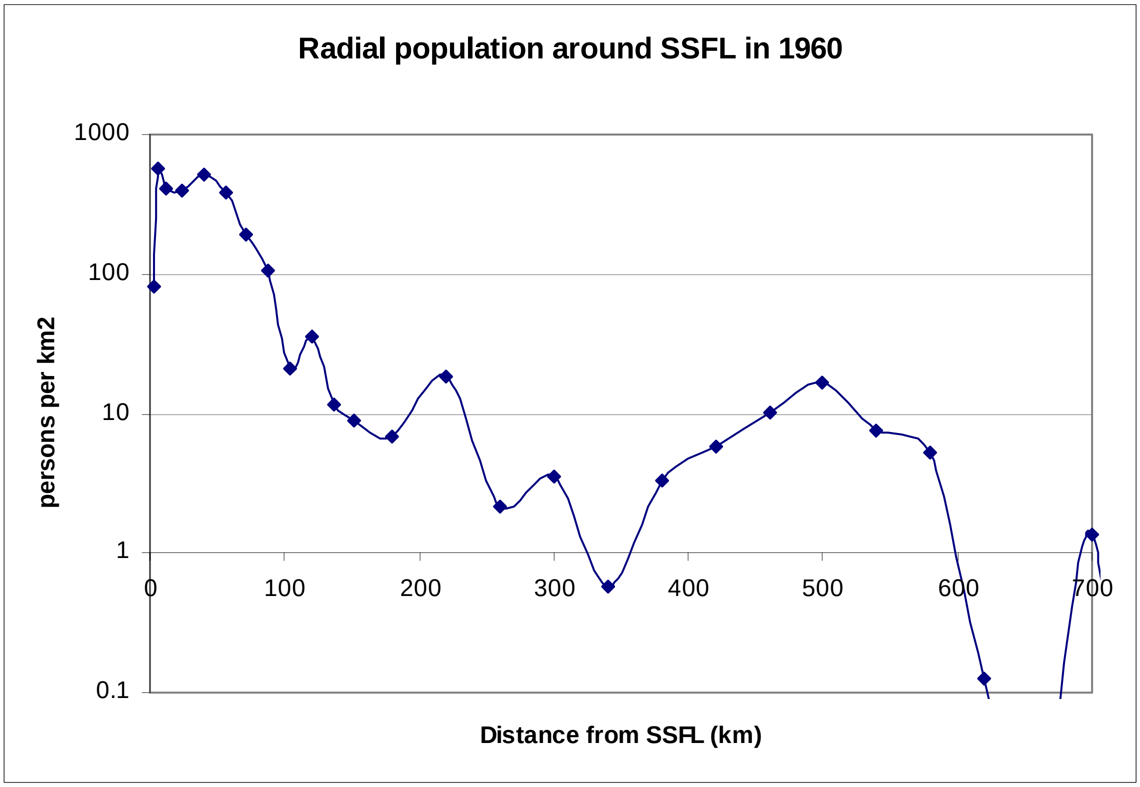
a) Since it turns out that the food pathways do not make a major contribution to dose, uncertainties in the parameters are not listed.

Table A2-1c. Other modeling parameters [a]		
Parameter	Value	Comment
Adult breathing rate	15 m^3/day	
Dose conversion factor (DCF)	2.636 * 1000000 rem/Curie	This is a weighted average over the population age distribution, based on NCI report, Table 6.7, p 6.18
Dry yield of crops	0.3 kg/m^2	Median. GSD = 1.8. NCI report, page 4.3.
Vegetation interception fraction	1.8 m^2/kg	NCI report, Figure 4.4 page 4.4.
Culinary removal	0.2	Faction of deposit remaining after food preparation. NCI report, page 7.8.
Effective half life of I-131 on vegetation	4.5 days	NCI report, page 7.2.
Vegetables: delay between production and distribution	1 day	NCI report, page 7.8.
I-131 dose-to-cancer-incidence conversion coefficient	60 per million person-rem	Averaged over age distribution of population. GSD = 4.5. From (Little et al. 1997a). Adjusted from deaths to incidence using a 10-fold ratio (page C-136)
Distance from SSFL of lettuce crops grown in San Fernando Valley	15 km	Based on inspection of 1960 land-use maps provided by (Bergquist 2005) created as part of (DWR 1964).
Cesium dose-to-cancer-incidence conversion coefficient	1) 0.00158 cases per rem 2) 0.003 cases per rem	Both values increased an additional 9 % to account for excess children under 15 in 1959 due to baby boom . 1) (NRC 2005). Factor of 1.5 DDREF removed.. 2) Average over 3 studies: (Cardis et al. 2005b), (Krestinina et al. 2005), (NRC 2005). See text.
Average cumulative thyroid fallout dose in LA area	0.1 rads	NCI report, figure ES.1
Latitude and Longitude of SSFL	34.22916667; -118.7083333 degrees	
Cesium dose conversion factor for 30 years of exposure, post-deposition	~ 1 rem per microcurie/m^2	Based on (Bunzl et al. 1997). See Chapter 3. Includes contribution of accompanying Cs-134. Assumes a 0.9 shielding factor, i.e., wood buildings.
a) Since it turns out that the food pathways do not make a major contribution to dose, uncertainties in the food pathway parameters are not listed.		

Population around SSFL in 1959.

Population data by census tract for 1970, 1980, 1990, and 2000 were obtained from a commercial company (Geolytics 2003). The Geolytics data is standardized to year-2000 tract boundaries, facilitating interpolation. Estimates of the number of persons residing in 1959 within year-2000 tract boundaries were obtained using logarithmic interpolation within each census tract. Data were extracted from the Geolytics database out to 1600 km, with population figures for a tract located geographically at the latitude and longitude of the tract centroid. Thus, dose estimates for persons within a census tract were computed at the tract centroid. Population whose tract centroid fell within a wind sector boundary were considered to reside in that wind sector. According to the Bureau of the Census, "census tracts generally have between 1,500 and 8,000 people" (http://www.census.gov/geo/www/cob/tr metadata.html). Population in Mexico was not included in the estimates of total population dose. A radial average of the population density is shown in Figure 6.2. The density is quite low beyond 100 km.

Figure 6.2



Choice of meteorological model.

A variety of approaches have been taken to estimate exposures following a release of radioactivity for use in epidemiological studies (Beyea and Hatch 1999). The current author has in the past used models that account for terrain (Beyea and DeCicco 1990), but never at a site as complex as the Los Angeles Basin. In fact, no epidemiological study that could be located in the literature has considered releases from the top of a complex terrain feature. A few meteorological studies have released tracers above terrain valleys, and these are very helpful in the present endeavor, but not in a quantitative way that is suitable for an epidemiological study.

One study of releases of chemicals from SSFL (Cohen 2005), (UCLA 2006) has made use of the computer program, CALPUFF, which has the potential to handle a number of features of complex terrain, but for CALPUFF to handle the SSFL site properly it would be necessary to input very detailed wind field information throughout the terrain. Such information could in principle be generated with a program like MM4 or MM5,[47] but that has probably not been done for this site. Furthermore, MM4 and MM5 have not been validated, to the author's knowledge based on computer searches, on as fine a scale as would be needed, and not at foggy sites with complex terrain. Nor does it appear that the algorithms in CALPUFF that are relevant for the SSFL site have been validated,[48] although the program's authors are very familiar with the meteorology of the Los Angeles Basin. The program's handling of coastal boundary layers appears to be state of the art, but necessarily rudimentary, especially when there is simultaneous interaction with complex terrain. Whether or not this feature was turned on in the UCLA work by Cohen et al. is not clear from their reports. In fact, no indication of the CALPUFF options used has been made public at this point, not that anyone has asked.

Given the near impossibility at this time of knowing with much confidence how the released activity moved during the July event,[49,50] even if one had the site-specific meteorology

[47] "The PSU/NCAR mesoscale model (known as MM5) is a limited-area, nonhydrostatic, terrain-following sigma-coordinate model designed to simulate or predict mesoscale atmospheric circulation. The model is supported by several pre- and post-processing programs, which are referred to collectively as the MM5 modeling system." (http://www.mmm.ucar.edu/mm5/)

[48] The need for validation in the complex terrain of the Los Angeles Basin has long been recognized. "Our simulations and analyses have demonstrated the importance of explicitly resolving the dynamics of the boundary layer, the coupling between the mixed layer and free troposphere, and the interactions of air flow with terrain in complex coastal settings." (Lu and Turco 1996).

[49]"…there appears to be no way to accurately estimate, a priori, [in complex terrain] the amount of tracer transported by either the drainage or synoptic flow." (Reible et al. 1981). Hinds also discusses the difficulties in estimating pollution entrained in drainage flows (Hinds 1970).

for the same time period, it is appropriate to use a simple model whose parameters can be varied over a wide range to facilitate sensitivity studies.

The "wedge model" is simple, transparent, easy to understand, and appropriate for scoping calculations (Alvarez et al. 2003), (Beyea et al. 2004). It has been used for most dose calculations in this report, including the 20,000 simulations. The only exception had been when considering elevated releases, as was done in Chapter 2, for computing ground deposition of radiocesium and in Chapter 3, when computing individual exposures to radioiodine from elevated releases. One advantage of the wedge model is that the size of the plume, wind speed, and deposition velocity all combine into one dominant parameter. Most important, there is little chance of getting unduly impressed that computer output has somehow transcended the data limitations.

The wedge model assumes radioactivity is uniformly distributed to a wedge height, within a fixed angle. It is similar to a Gaussian plume model using the "top hat" approximation, when vertical dispersion has reached a boundary level above. Given the strong vertical turbulence that develops in complex terrain (Angell et al. 1966), the use of the wedge model is particularly appropriate at this site for scoping purposes. By considering a very wide range of model parameters, both wedge height and deposition velocity, the hope is to bracket the results of an actual release.

Medians, means, and confidence intervals.

Because doses and health effects cannot be estimated without uncertainty, most of the numbers calculated in this appendix are really random variables with their own distributions, often lognormal. When random variables such as dose estimates and risk conversion coefficients are combined, it is desirable to fold together the two distributions, which is problematic, if the range of one of the distributions is not known. Such is the situation at SSFL, where the uncertainty in the dispersion model is not known. Because the resulting medians of the combined distribution do not require full knowledge of all parameter distributions, it is more rigorous for the calculations in this appendix to work with medians as input into Monte Carlo simulations. However, in so doing the reader should bear in mind that averages (means) of the distribution will generally be higher, and sometimes much higher than the medians of the distribution. Such

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[50] "The interaction of the marine layer with significant terrain features and the effects of slope flow, elevated-layer generation, and inland convection combine to produce an exceedingly complex boundary layer structure for potential modeling applications." (McElroy and Smith 1991).

behavior is reflected in the percentile Tables shown in the main text, which take into account the various uncertainties in running the 20,000 simulations.

Furthermore, it is always a challenge to describe distributions of interest without drowning the reader in data. Sometimes medians, sometimes means, are appropriate for describing the center of the distribution. For case/control epidemiological studies, with individualized exposure estimates, it may be most appropriate to use medians. In contrast, it may be most appropriate to use means for ecologic epidemiological studies, because one is usually looking at exposure over a group, say an average over a zip code or census tract. Better yet, it might be better to carry out regressions with both indicators to see if it makes any difference as to which is used.

In any case, for intermediate calculations, such as population doses, it is usually more useful to report medians. For the final calculation of interest, reporting means may be most informative, once all uncertainties have been calculated. Because uncertainties are not incorporated in the Tables produced for this appendix, median values are presented. Descriptors of the full distribution can be found in the Tables presented in Chapters 2-4 based on results from the 20,000 simulations.

Thyroid doses to individuals.

Doses presented in the following Tables have been averaged over the age distribution of the 1959-population. The doses include contributions from both inhalation and milk. The inhalation contribution assumes a naïve, straight-line wind model. Dose from food products depends, in part, on the distribution network bringing food to grocery stores and the grocery store(s) utilized by a population group. Relevant data on food distribution in the area has not been located for the period. Therefore, the contribution from milk in the Tables is computed as an average over the entire 1959 Los Angeles population of 6 million persons (Census_Bureau 1992). The milk dose average ranges from 0.008 to 0.1 rem depending on the model parameters assumed. The average does not vary with distance, so the changes in distance shown in the following Tables only reflect changes in the contribution from inhalation. Therefore, the fall off of dose with distance shown in the Tables is quite muted for parameter choices that lead to inhalation dose reduced relative to milk dose.

The closest distance to SSFL for which doses has been presented in the Tables using the Wedge Model in this appendix is 5 km. (Note that the distance to the center of Simi Valley is 6 km.) The reason is: In the absence of information about the heat release rate during the accident and the building ventilation rate, it is difficult to estimate plume rise. Without an estimate of plume rise, it is not possible to estimate the reduction in dose that occurs close to the reactor, before the plume expands. Note, however, that ground contamination levels at 1.6 km were considered in Chapter 2 using the Gaussian plume model for purposes of assessing possible impacts of soil measurements on refining release estimates. Inhalation thyroid doses were also considered at 1.6 km in Chapter 3. In both of those cases, various plume rise values were assumed for exploratory purposes.

Thyroid doses averaged over the population's age distribution are presented in Tables A2-2a through A2-2e. Each Table assumes a different deposition velocity. Consideration of a wide range of deposition velocities is necessary, because this parameter is very uncertain (USNRC 1995). The doses in the Tables are much lower than would be obtained for a worst-case scenario in which all the emitted radioactivity was contained in a nighttime plume moving in one direction with low velocity. Here, the emissions are assumed to occur over a range of conditions and times, with wind varying in direction and magnitude.

Because of preferential wind directions, the magnitude of individual dose estimates do vary by wind sector, the magnitude of which is relevant to epidemiological assessments. Table A2-3, which is presented after the average-dose Tables, shows how the inhalation dose varies with wind sector for a set of model parameters.

For deposition velocity = 0.001 m/s:

Table A2-2a. Thyroid dose in rem from inhalation and milk ingestion after a release of 20,000 Ci of Iodine-131 at SSFL, under the assumptions in Scoping Scenario A[a)] for a deposition velocity of **0.001 m/s** and various wedge heights. Dose is averaged over the population's age distribution.

	Distance from SSFL (km)					
Wedge Height (m)	**5.0**	**7.0**	**9.0**	**12.5**	**17.5**	**22.5**
50	1.6	1.3	0.94	0.64	0.49	0.28
100	0.87	0.69	0.52	0.36	0.29	0.17
200	0.45	0.36	0.28	0.20	0.16	0.099
500	0.18	0.15	0.12	0.084	0.070	0.044
1000	0.094	0.077	0.059	0.043	0.036	0.023

a) Including the assumption of a naïve, straight-line wind model.

For comparison purposes note that the estimated thyroid dose in the Los Angeles area accumulated from all weapons tests was 0.1 rem (NCI 1997), Fig ES.1. One year's dose to the thyroid from background radiation is also about 0.1 rem.

For deposition velocity = 0.003 m/s:

Table A2-2b. Thyroid dose in rem from inhalation and milk ingestion after a release of 20,000 Ci of Iodine-131 at SSFL, under the assumptions in Scoping Scenario A[a)] for a deposition velocity of **0.003 m/s** and various wedge heights. Dose is averaged over the population's age distribution.						
	Distance from SSFL (km)					
Wedge Height (m)	**5.0**	**7.0**	**9.0**	**12.5**	**17.5**	**22.5**
50	1.4	1.0	0.72	0.46	0.33	0.20
100	0.82	0.65	0.48	0.33	0.26	0.16
200	0.46	0.37	0.28	0.21	0.17	0.11
500	0.20	0.17	0.13	0.099	0.085	0.060
1000	0.11	0.088	0.070	0.055	0.048	0.035

For deposition velocity = 0.01 m/s:

Table A2-2c. Thyroid dose in rem from inhalation and milk ingestion after a release of 20,000 Ci of Iodine-131 at SSFL, under the assumptions in Scoping Scenario A[a)] for a deposition velocity of **0.01 m/s** and various wedge heights. Dose is averaged over the population's age distribution.						
	Distance from SSFL (km)					
Wedge Height (m)	**5.0**	**7.0**	**9.0**	**12.5**	**17.5**	**22.5**
50	0.77	0.50	0.30	0.18	0.13	0.099
100	0.65	0.48	0.34	0.23	0.18	0.13
200	0.44	0.35	0.27	0.20	0.16	0.13
500	0.22	0.19	0.15	0.12	0.11	0.088
1000	0.13	0.11	0.096	0.080	0.073	0.061

For deposition velocity = 0.03 m/s:

Table A2-2d. Thyroid dose in rem from inhalation and milk ingestion after a release of 20,000 Ci of Iodine-131 at SSFL, under the assumptions in Scoping Scenario A[a)] for a deposition velocity of **0.03 m/s** and various wedge heights. Dose is averaged over the population's age distribution.						
	Distance from SSFL (km)					
Wedge Height (m)	**5.0**	**7.0**	**9.0**	**12.5**	**17.5**	**22.5**
50	0.19	0.10	0.053	0.035	0.030	0.028
100	0.29	0.19	0.12	0.084	0.070	0.064
200	0.32	0.24	0.18	0.14	0.12	0.11
500	0.23	0.20	0.17	0.14	0.13	0.11
1000	0.16	0.14	0.12	0.11	0.10	0.091

For deposition velocity = 0.1 m/s:

Table A2-2e. Thyroid dose in rem from inhalation and milk ingestion after a release of 20,000 Ci of Iodine-131 at SSFL, under the assumptions in Scoping Scenario A[a)] for a deposition velocity of **0.1 m/s** and various wedge heights. Dose is averaged over the population's age distribution.						
	Distance from SSFL (km)					
Wedge Height (m)	**5.0**	**7.0**	**9.0**	**12.5**	**17.5**	**22.5**
50	0.016	0.010	0.0080	0.0077	0.0076	0.0076
100	0.040	0.023	0.015	0.013	0.012	0.012
200	0.079	0.051	0.034	0.028	0.026	0.025
500	0.15	0.12	0.10	0.091	0.086	0.083
1000	0.16	0.14	0.13	0.12	0.11	0.11

Doses vary by wind sector. Table A2-3 presents an example of scaling factors by wind sector for inhalation dose, assuming the naïve, straight-line wind model. The scaling factors do not apply to milk doses, so are not strictly matched to Tables A2-2a through A2-2e. Were the milk dose included the variation in scale factors would be muted. Note that "Sector 1" in the Table points due north, referring to winds blowing to the North. Sector 9 points due south. Sector 5 points due east, referring to winds blowing to the east. Sector 13 points to the west. Blanks in the Table occur when there were no centroids of census tracts in the zone.

Although Sectors 6-8 and 14-15 generally have the highest scale factors and hence the highest doses at the same radius from the plant, there are variations when different model parameters are chosen (data not shown), making the identification of zones of highest exposure somewhat model dependent. Still, to get a simple likelihood distribution for combining uncertainties, all the data in Table A2-3 were fit to a series of probability distributions, using the Excel add-in, Crystal Ball (Decisioneering 2003). The best fit that preserved a mean of ~ unity was a normal distribution truncated on both sides (Mean = 1. Standard Deviation = 0.76. Min = 0.17. Max = 2.76).

Table A2-3. Scale factors for inhalation dose by wind sector.[a] Wedge Height = 200 m. Deposition velocity = 0.003 m/s.						
	Distance from SSFL (km)					
	5.0	**7.0**	**9.0**	**12.5**	**17.5**	**22.5**
Average	0.403093	0.315544	0.22826	0.151586	0.115429	0.058907
Sector 1	0.29	0.29	0.31			0.30
Sector 2	0.17	0.19				0.20
Sector 3		0.18				0.20
Sector 4			0.26	0.31	0.25	0.35
Sector 5	0.90	0.86	0.89	0.94	0.81	1.2
Sector 6		1.6	1.6	1.6	1.4	2.0
Sector 7		2.1	2.2	2.5	2.2	2.7
Sector 8			1.9	2.0	1.9	2.6
Sector 9						1.1
Sector 10			0.60	0.69		0.77
Sector 11		0.37	0.36	0.41	0.37	0.43
Sector 12				0.17	0.18	0.23
Sector 13			0.38	0.40	0.41	0.54
Sector 14	1.5	1.6	1.5		1.5	
Sector 15	2.1	2.1				
Sector 16	1.0	0.85				1.4
a) The highest/lowest scale factor in a column indicates the highest/lowest dose at the radius listed in the heading of the column.						

Implications for the feasibility of future epidemiological studies.

In general, the use of the wedge model underestimates the falloff of dose with distance, since it does not include an increase in vertical dispersion with distance. As a result, the wedge model is not the best indicator for planning an epidemiological study. On the other hand, it is quite possible that measures of distance from the SSFL plant or use of naïve models that do not account fully for terrain are not the most valuable dose surrogates to consider. Trying to use predictions based on a poor database could be misleading in that a negative dose response result would not be useful, given the uncertainty, and might give a misleading impression.

In the absence of an adequate individual exposure model, it may be more useful to select study subjects who resided in area where SSFL pollution may have "recycled" (Hanna et al. 1991) and/or select study subjects who obtained milk from backyard cows or goats that largely fed on pasture, or who know their milk came from dairies close to the facility.

For instance, persons residing in the San Fernando Valley, where milk from the Peterson Dairy (9 km from SSFL[51]) was likely to have been common in stores, probably received average doses from radioiodine in food that would have been much higher than the average for all of Los Angeles County. It is also worth noting that the average individual inhalation doses presented in the above Tables, A2-2a through A2-2e, probably do not apply to Simi Valley and Chatsworth. Radioactivity, like other pollutants, can get trapped in the Valleys, moving back and forth between them (Strimaitis et al. 1991), (Hanna et al. 1991).[52] As a result, average velocities used in the calculations may be too high, which means an underestimate of dose.

Separating persons in the western and eastern portions of the Santa Fernando Valley is an interesting possibility to consider for an epidemiological study, because convergence flows appear to act as a barrier to radioactivity reaching the Eastern part of the valley (Lu and Turco 1995).

A way to go beyond or supplement modeling efforts is to look for exposure signatures of the release that still remain. The first step would be to fly helicopters around the plant at distances from 5 to 20 km equipped with Germanium gamma detectors set to pick out Cs-137. Another possibility would be to measure long-lived radiocesium deposited in undisturbed locations, such as attics (Cizdziel et al. 1999), (Ilacqua et al. 2003), crawl spaces, abandoned buildings, and rarely used warehouses. [Quantification of this idea is beyond the scope of this report.] Other possible selection criteria might include people who lived on farms or people who used swimming pools. Swimiming pools are of interest because it is difficult to swim without ingesting some water. However, the number of swimming pools present in the valleys close to SSFL in 1959 appear to be small. The prevalence on the south side of the Santa Monica Mountains has not been researched for this report.

Finally, it would be theoretically possible to release tracers, balloons, or even fog droplets from the SSFL site to increase knowledge of local pollution transport. However, this would

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[51] The address of the Peterson Dairy, 9409 Farrolone Avenue in Chatsworth, was obtained from http://digital-library.csun.edu/agriculture.html A picture of the Dairy circa 1966 is shown on the web site. For this report, the pasture acreage identified for the San Fernando Valley in 1959 is assumed to have been located at the Peterson Dairy Farm.

[52] This phenomenon is revealed in surface wind maps presented in the 1949 survey of the area in preparation for siting the reactor (Chalker 1949).

require a massive effort and, presumably, the plant operator's cooperation, which does not seem likely.

Thyroid population dose.

Sources of milk considered in this report include dairies in Dairy Valley, San Fernando Valley, and Chino (outside of Los Angeles County). Dairies near Puente Junction (wind flux sector 6) have been lumped with those in Diary Valley (wind flux sector 7), because the distance to SSFL is the same for both and because the wind distribution for sectors 6 and 7 are similar. The dairies in the San Fernando Valley contribute by far the bulk of the milk population dose, even though the pasture there represents only 22% of the total pasture considered. Dairies in the Santa Ana area were excluded because of a lack of information on their exact location. Additional effort to track down the locations was not necessary, because the total pasture reported for this area was small compared to the County total.

Some dairy operations occurred in the Oxnard Plain (Yee 2005), but no pasture acreage or other indicator of herd size has as yet been identified for the Oxnard area.

The population dose from milk was computed by estimating an average radioiodine concentration in the total amount of milk produced for human consumption in Los Angeles County and the neighboring Chino region (97 km from SSFL). All estimates of radioiodine in milk and conversions to dose have been computed using the methodology laid out in the NCI's fallout report (NCI 1997). When appropriate, information given in the NCI report specific to California Counties in the 1950s was utilized. From calculations of the median dose produced per median liter of milk consumed, using the NCI methodology (NCI 1997), it was possible to sum up the total population dose, without knowing precisely who drank milk from which dairy.

As a starting point, the total amount of milk produced for human consumption in Los Angeles County for 1954 was extracted from the NCI report (NCI 1997). The production in Los Angeles County accounted for approximately half of the milk consumption in the County, according to the report. Any SSFL radioactivity ending up in the out-of-county milk (other than at Chino) has been ignored, since the concentrations are likely to be small.

Land use surveys taken by the Los Angeles Water Department were used in this study to allocate the relative contribution of dairies in different parts of Los Angeles County (DWR 1964). Allocation was made according to the percentage of pasture in each dairy area.[53] However, data

[53] Since most of the feed comes from stored material, not pasture grass, this approach has limitations and will introduce large uncertainties into the true allocation. However, no other allocation scheme was identified.

from the LA Water department showed a marked decline in pasture between 1955 and 1960, indicative of a shift outside the county. Declines in the LA County region were assumed to reflect a shift to the Chino area, based on Peterson (Peterson 2000) and noted in other historical sources. Pasture acreage estimates for 1959 were obtained by logarithmic extrapolation between 1955 and 1960 data. See Table A2-4.

Table A2-4 Acres in pasture in Los Angeles County around the time of the SRE accident.

	Year		
Los Angeles County water district[a]	1955	1960	1959 (log-extrapolated)
Coastal plain	11300	1500[b)]	2246
San Fernando	5100	900	1273
San Gabriel	3400	1400	1672
Santa Ana	1400	300	408
Malibu	100	0	0

a) As defined by the DWR report (DWR 1964).
b) Assumed to reflect movement of Dairies to Chino-Ontario area.

Table A2-5 shows the estimated median population dose from ingestion of milk

Table A2-5. Contribution to the median thyroid population dose (person-rem) from drinking milk after a release of 20,000 Ci of Iodine-131 at SSFL, under the assumptions in Scoping Scenario A for a variety of modeling parameters.

	Deposition velocity (m/s)				
Wedge Height (m)	**0.001**	**0.003**	**0.01**	**0.03**	**0.1**
50	63,700	98,200	87,000	27,200	7,600
100	42,700	74,700	109,900	61,600	11,700
200	26,400	53,500	95,900	105,100	24,500
500	12,900	31,300	66,700	104,600	81,800
1000	7,400	19,700	48,300	82,400	104,500

a) These estimates are comparable to medians. Were uncertainties in modeling parameters included, the likelihood distribution would be right skewed and the mean estimates would be much higher, given the huge uncertainty at this facility due to the complex terrain and failure on the part of the plant operator to collect data that would have assisted in making dose estimation.

Doses from consumption of leafy vegetables were computed in a similar fashion to doses from milk using the NCI fallout methodology. The average amount of radioiodine per kilogram of leafy vegetables was estimated, along with the dose from eating 1 kg. An estimate was also made of the total kg of leafy vegetables produced in Los Angeles and Ventura counties, which was then used to compute the total population dose, again, without knowing exactly who ate what vegetables from what farm. Production of leafy vegetables was only considered in the Oxnard Plain and in the San Fernando Valley. In 1959 scattered farming remained in San Fernando Valley, including some lettuce (Robinson 1961). Acreage obtained for Ventura County was assumed to be located in the Oxnard Plain, since most of the Arable land in Ventura lies in the Coastal Plain (Edwards et al. 1970), p. 142. Production of leafy vegetables in any other locations was assumed to make a negligible contribution to the population dose and therefore was ignored. Table A2-2 shows the results for population dose from leafy vegetables. The contribution is considerably smaller than the milk contribution. For one reason, a relatively small number of acres of leafy vegetables was harvested in the two counties in July of 1959.

Table A2-6. Contribution to the median thyroid population dose (person-rem) from eating leafy vegetables after a release of 20,000 Ci of Iodine-131 at SSFL, under the assumptions in Scoping Scenario A for a variety of modeling parameters.

	Deposition velocity (m/s)				
Wedge Height (m)	**0.001**	**0.003**	**0.01**	**0.03**	**0.1**
50	19,200	10,900	400	0	0
100	15,900	18,100	4,800	0	0
200	10,400	18,500	13,200	1,500	0
500	4,900	11,800	19,200	10,900	400
1000	2,500	6,900	15,900	18,100	4,800

a) These estimates are comparable to medians. Were uncertainties in modeling parameters included, the likelihood distribution would be right skewed and the mean estimates would be much higher, given the huge uncertainty at this facility due to the complex terrain and failure on the part of the plant operator to collect data that would have assisted in making dose estimation.

b) Ventura County 1959 harvested acres of leafy vegetables, of which only the summer crop, if any, is relevant to the July event at SRE. Acreage source: (McDonnell 2005)
Cabbage (550 acres Spring crop, **400 Late Summer crop**, 960 Winter crop)
Celery (1150 Spring crop, 1100 Winter crop)
Lettuce (1800 Spring crop, **150 Summer crop**, 1230 Fall crop.)
Broccoli (1550 Spring crop, 2160 Fall crop)

c) Los Angeles County 1959 harvested acres of leafy vegetables, of which only the summer crop, if any, is utilized. Source: (McDonnell 2005)
Cabbage (460 acres Winter crop, 450 Spring crop, 500 Fall crop)
Celery (630 Winter crop, 900 Spring crop, 100 Fall crop)
Broccoli (0 acres)
Lettuce (150 Spring crop, **200 Summer crop**, 100 Fall crop
Assumed to be grown in the San Fernando Valley. Source: "…the San Fernando Valley is the most likely place [lettuce] would have been grown during the summer. The Antelope Valley (where most of the agriculture is today) is high desert, so if lettuce was grown there, it probably would have been grown in the winter (similar to the Imperial Valley today." (Kisko 2005).

Table A2-7 presents the inhalation population dose to the thyroid based on the assumptions discussed above as summarized earlier in Table A2-1. Results are presented for a range of two modeling parameters, namely, the height of the wedge and the deposition velocity of radioiodine to vegetative surfaces. High deposition reduces the amount of material available for inhalation further down wind, so the inhalation dose is generally greatest for small deposition velocities. On the other hand, the smaller the wedge height assumed, the greater the concentration, when the release magnitude is kept the same. Therefore, as shown in the Table, the population dose tends to decline with larger wedge height. The fraction of material emitted above the inversion layer makes a significant contribution only for the two highest deposition velocity values for which data is presented in the Table.[54] For deposition velocities of 0.03 and 0.1, material emitted below the inversion layer has largely been depleted before it reaches the high population zones.

The numbers in the Table represent *median* population doses. *Means* would be higher, as is shown in the percentile Tables given in Chapters 2-4.

Table A2-7. Inhalation thyroid population dose in person-rem from a release of 20,000 Ci of Iodine-131 at SSFL under the assumptions in Scoping Scenario A, including an assumed straight-line wind model.

	Deposition velocity (m/s)				
Wedge Height (m)	**0.001**	**0.003**	**0.01**	**0.03**	**0.1**
50	1,406,000	554,000	144,000	45,000	20,300
100	1,021,000	545,000	176,000	58,000	22,000
200	650,000	445,000	193,000	71,000	25,300
500	326,000	273,000	173,000	81,000	30,600
1000	193,000	175,000	135,000	80,000	33,800

a) These estimates are comparable to medians. Were uncertainties in modeling parameters included, the likelihood distribution would be right skewed and the mean estimates would be much higher, given the huge uncertainty at this facility due to the complex terrain and failure on the part of the plant operator to collect data that would have assisted in making dose estimation.

Thyroid doses from other pathways are significantly smaller at this site than the dose from inhalation. This unusual situation arises because 80% of the cows supplying the population of Los Angeles with milk are further away from the SSFL site than the high density population. For instance, Dairy Valley is approximately 70 km from SSFL and Chino is 100 km away. Also, Los Angeles dairies, like others in Southern California, relied heavily on stored feed, with one of the lowest pasture rates in the country according to the NCI study. This factor alone reduces milk doses by a factor of 3-5 compared to other sites in the US for the same deposition to pasture.

Table A2-8 shows the combination of inhalation and food consumption (milk and leafy vegetables)

[54] Radioactivity emitted above the inversion layer is assumed to be in a 1000 m wedge by the time it touches down.

Table A2-8. Total thyroid population dose in person-rem from a release of 20,000 Ci of Iodine-131 at SSFL under the assumptions in Scoping Scenario A, including an assumed straight-line wind model.

	Deposition velocity (m/s)				
Wedge Height (m)	**0.001**	**0.003**	**0.01**	**0.03**	**0.1**
50	1,488,900	663,100	231,400	72,200	27,900
100	1,079,600	637,800	290,700	119,600	33,700
200	686,800	517,000	302,100	177,600	49,800
500	343,800	316,100	258,900	196,500	112,800
1000	202,900	201,600	199,200	180,500	143,100

a) These estimates are comparable to medians. Were uncertainties in modeling parameters included, the likelihood distribution would be right skewed and the mean estimates would be much higher, given the huge uncertainty at this facility due to the complex terrain and failure on the part of the plant operator to collect data that would have assisted in making dose estimation.

For comparison, purposes, the estimated thyroid population dose from all weapons fallout tests combined is 620,000 person-rem in Los Angeles and Ventura Counties.[55]

Using the numbers from Little et al, the results from converting the total thyroid population dose to estimated excess thyroid cancers are shown as *medians* in Table A2-9. Mean values would be higher. Note that when converting these numbers to a prevalence rate, it is necessary to remember that it is the population of children that is of interest, which was about 1/3rd of the total at the time of the accident.

[55] The median dose from fallout in this region of the US was 0.1 rad per person. Figure ES-1 of (NCI 1997) The Population of Los Angeles in 1960 was 6 million persons; population of Ventura County was 200,000 (Census_Bureau 1992).

Table A2-9. Median excess thyroid cancers expected from milk intake and inhalation of Iodine-131 following a release of 20,000 Ci from SSFL under the assumptions in Scoping Scenario A, as a function of various modeling parameters.[a,b,] The mean number of excess cancers, as opposed to the median, would be higher. One-out-of-ten thyroid cancers assumed to be fatal.[c] The background number of thyroid cancers in the same population would be in the thousands.					
	Deposition velocity (m/s)				
Wedge Height (m)	**0.001**	**0.003**	**0.01**	**0.03**	**0.1**
50	89	40	14	4	2
100	65	38	17	7	2
200	41	31	18	11	3
500	21	19	16	12	7
1000	12	12	12	11	9

a) These estimates are comparable to medians. Were uncertainties in modeling parameters included, the likelihood distribution would be right skewed and the mean estimates would be much higher, given the huge uncertainty at this facility due to the complex terrain and failure on the part of the plant operator to collect data that would have assisted in making dose estimation. The mean would be a factor of 3 higher accounting for uncertainty in the risk conversion coefficient alone. There would be a 5% likelihood that the excess cancers were 12 times higher than given in the Table based on uncertainty in risk conversion coefficient.
b) No RBE factor is incorporated.
c) According to Little (Little et al. 1997b). Other estimates are lower.

As a point of comparison, note that, using the same risk coefficient that was assumed in Table A2-7, the number of excess cancers expected in Los Angeles and Ventura counties would be 36 from the 0.1 average rem of radioiodine exposure accumulated from the sum of all nuclear weapons tests (NCI 1997), Table ES.1. The numbers expected from natural background radiation in the same population cohort might be a factor of ten higher, i.e. 360.[56] The total of 360 excess thyroid cancers expected from natural background radiation might account for 4-32% of all thyroid cancers in the same cohort, based on the A-bomb epidemiologic data (Little 2002).

At Windscale, the total population dose was estimated to be 2.6 million person-rem (Crick and Linsley 1984). This is about a factor of five to ten higher than the middle values presented for SSFL in the previous Tables, assuming the same release. Crick and Linsley used a thyroid cancer risk coefficient of 100 per million person rem to the thyroid, which lies between the median (60) and mean (180) used in this appendix. Thus, they predicted some 250 excess thyroid cancers to result from the 20,000 Curies released. These authors also cite the version of the UNSCEAR report current at the time of their writing, which indicated that the risk coefficient might be as high as 330 per million person thyroid rem. This latter number is within the 90%

[56] Ten years of childhood exposure of the 1959 cohort at 0.1 rem per year.

confidence range presented by Little, which had a 95%-likelihood value of 700. The current UNSCEAR report (UNSCEAR 2000), however, uses the Little et al. median number .

Recall that we are assuming in this appendix that the same number of curies are released at SRE as at Windscale, so the differences in projected doses reflect differences in the sites, not in the choice of release magnitude. So then, why is the population dose estimated at SSFL less than the value estimated in the UK?

Part of the explanation has to do with the assumption made here that half the released radioactivity was carried aloft. Also, at Windscale, 70% of the dose was estimated to come from milk, whereas at SSFL the percentage varies between 4 and 40% in the scoping calculations, depending on the choice of model parameters. These two factors alone can explain the discrepancy.

Furthermore, 90% of the population dose at Windscale was calculated to have been received beyond 50 km (Crick and Linsley 1984), whereas at SSFL, most of the population dose occurs within 50 km. The UK has a population density of 243 inhabitants per km^2 (http://copernicus.subdomain.de/List_of_countries_by_population_density), which is significantly larger that the average population in the US, which is 31 per km^2. Conceivably, this difference may also play a role, although SSFL has Los Angeles very near by.

Note that, because there are field measurements to rely on, the estimate of population dose at Windscale is much more accurate than any comparable estimate that can be made at SSFL, And, there is little argument about the Windscale thyroid population dose numbers. There are official estimates (Crick and Linsley 1984) and estimates made by nuclear skeptics, including one for which the current author arranged funding and reviewed the analysis (Taylor 1981). Taylor estimated a population dose of 3-million person-thyroid-rem, which is quite consistent with the 2.6 million figure estimated a few years later by Crick and Linsley. In contrast to Windscale, the population dose at SSFL is very uncertain because of the absence of measurements that could have compensated for the difficulty in exposure modeling in the LA Basin.

Accounting for uncertainty. For the Tables presented in the main text, the various likelihood distributions discussed in this report were combined in a Monte Carlo analysis, using the Excel add-in, Crystal Ball (Decisioneering 2003). 20,000 simulations were run.

Individual doses from deposited radiocesium.

See Chapter 3 for results. Average individual doses were computed by dividing population doses by relevant population totals. Although this sounds backwards, it was appropriate given the fact that our population figures obtained from Census data were aggregated at the Census Tract level from the beginning.

Population dose from deposited radiocesium.

Population doses from 30-year exposure to radiocesium groundshine are presented in Table A2-11 for the component that does not penetrate the mixing layer. The corresponding numbers when 50% of the release does penetrate the mixing layer are shown in Table A2-12. The added population dose occurs beyond 60 km from the SSFL site and is significant only for high effective deposition velocities. Note that the numbers in these Tables were computed using the Wedge Model and the dose conversion coefficients described in Chapter 3. To convert the numbers to projected cancers using the average coefficient discussed in Chapter 4, multiply by 3.4. To convert the numbers to projected cancers using the BEIR VII number without the factor of 1.5 DDREF, multiply by 1.7. Both numbers incorporate a 9% increase to account for baby-boom children.

In the Monte Carlo calculations used to generate the percentile figures presented in the main text, each of these entries in the following Tables was given equal probability.

Table A2-11. Median whole-body population dose (1000s of person-rem) from a release of 600 Ci of cesium-137 at SSFL under the assumptions in Scoping Scenario A. Only shown is the population dose from the 300 Ci assumed to remain beneath the mixing layer.

	Deposition velocity (m/s)				
Wedge Height (m)	**0.001**	**0.003**	**0.01**	**0.03**	**0.1**
50	112	125	87	36	4
100	81	124	113	68	17
200	51	101	127	99	44
500	24	58	113	125	87
1000	13	34	81	124	113

a) These estimates are comparable to medians. Were uncertainties in modeling parameters included, the likelihood distribution would be right skewed and the mean estimates would be higher, given the uncertainty at this facility due to the complex terrain and failure on the part of the plant operator to collect data that would have assisted in making dose estimation.

Table A2-12. Median whole-body population dose (1000s of person-rem) from a release of 600 Ci of cesium-137 at SSFL under the assumptions in Scoping Scenario A. Includes the 300 Ci assumed to penetrate the mixing layer.					
	Deposition velocity (m/s)				
Wedge Height (m)	**0.001**	**0.003**	**0.01**	**0.03**	**0.1**
50	116	136	119	114	174
100	85	135	146	146	188
200	55	112	160	177	214
500	28	69	145	204	257
1000	17	45	114	203	283
a) These estimates are comparable to medians. Were uncertainties in modeling parameters included, the likelihood distribution would be right skewed and the mean estimates would be higher, given the uncertainty at this facility due to the complex terrain and failure on the part of the plant operator to collect data that would have assisted in making dose estimation.					

In the Tables below, we show whole-body population doses accumulated out to different distances from SSFL.

Table A2-13. **< 5 km**. Median whole-body population dose (1000s of person-rem) from a release of 600 Ci of cesium-137 at SSFL under the assumptions in Scoping Scenario A.					
	Deposition velocity (m/s)				
Wedge Height (m)	**0.001**	**0.003**	**0.01**	**0.03**	**0.1**
50	0.7	1.9	3.9	3.7	0.8
100	0.4	1.1	2.7	4.2	2.5
200	0.2	0.6	1.6	3.4	4.0
500	0.1	0.2	0.7	1.9	3.9
1000	0.0	0.1	0.4	1.1	2.7
a) These estimates are comparable to medians. Were uncertainties in modeling parameters included, the likelihood distribution would be right skewed and the mean estimates would be higher, given the uncertainty at this facility due to the complex terrain and failure on the part of the plant operator to collect data that would have assisted in making dose estimation.					

Table A2-14. < **7.5 km**. Median whole-body population dose (person-rem) from a release of 600 Ci of cesium6137 at SSFL under the assumptions in Scoping Scenario A.					
	Deposition velocity (m/s)				
Wedge Height (m)	**0.001**	**0.003**	**0.01**	**0.03**	**0.1**
50	8.7	21	35	23	3.4
100	4.6	12	28	34	13
200	2.3	6.7	18	33	26
500	1.0	2.8	9	21	35
1000	0.5	1.4	4.6	12	28
a) These estimates are comparable to medians. Were uncertainties in modeling parameters included, the likelihood distribution would be right skewed and the mean estimates would be higher, given the uncertainty at this facility due to the complex terrain and failure on the part of the plant operator to collect data that would have assisted in making dose estimation.					

Table A2-15. < **10 km**. Median whole-body population dose (1000s of person-rem) from a release of 600 Ci of cesium-137 at SSFL under the assumptions in Scoping Scenario A.					
	Deposition velocity (m/s)				
Wedge Height (m)	**0.001**	**0.003**	**0.01**	**0.03**	**0.1**
50	17	39	57	31	4
100	9.1	24	51	51	16
200	4.7	13	35	57	37
500	1.9	5.6	17	39	57
1000	1.0	2.9	9.1	24	51
a) These estimates are comparable to medians. Were uncertainties in modeling parameters included, the likelihood distribution would be right skewed and the mean estimates would be higher, given the uncertainty at this facility due to the complex terrain and failure on the part of the plant operator to collect data that would have assisted in making dose estimation.					

Table A2-16. < **20 km.** Median whole-body population dose (1000s of person-rem) from a release of 600 Ci of cesium-137 at SSFL under the assumptions in Scoping Scenario A.					
	Deposition velocity (m/s)				
Wedge Height (m)	**0.001**	**0.003**	**0.01**	**0.03**	**0.1**
50	32	65	75	35	4
100	18	43	76	63	17
200	9.3	25	59	78	42
500	3.8	11	32	65	75
1000	1.9	5.7	18	43	76
a) These estimates are comparable to medians. Were uncertainties in modeling parameters included, the likelihood distribution would be right skewed and the mean estimates would be higher, given the uncertainty at this facility due to the complex terrain and failure on the part of the plant operator to collect data that would have assisted in making dose estimation.					

Table A2-17. **< 50 km**. Median whole-body population dose (1000s of person-rem) from a release of 600 Ci of cesium-137 at SSFL under the assumptions in Scoping Scenario A.					
	Deposition velocity (m/s)				
Wedge Height (m)	**0.001**	**0.003**	**0.01**	**0.03**	**0.1**
50	80	110	85	36	4
100	52	96	106	68	17
200	30	68	108	96	44
500	13	35	80	110	85
1000	6.6	18.8	52	96	106
a) These estimates are comparable to medians. Were uncertainties in modeling parameters included, the likelihood distribution would be right skewed and the mean estimates would be higher, given the uncertainty at this facility due to the complex terrain and failure on the part of the plant operator to collect data that would have assisted in making dose estimation.					

Table A2-18. **< 100 km**. Median whole-body population dose (1000s of person-rem) from a release of 600 Ci of cesium6137 at SSFL under the assumptions in Scoping Scenario A. Includes the population dose from the return flow from the North, some of which may extend beyond 100 km.					
	Deposition velocity (m/s)				
Wedge Height (m)	**0.001**	**0.003**	**0.01**	**0.03**	**0.1**
50	115	136	119	114	174
100	82	134	146	146	188
200	52	109	160	177	214
500	26	66	143	204	257
1000	15	42	111	202	283
a) These estimates are comparable to medians. Were uncertainties in modeling parameters included, the likelihood distribution would be right skewed and the mean estimates would be higher, given the uncertainty at this facility due to the complex terrain and failure on the part of the plant operator to collect data that would have assisted in making dose estimation.					

Appendix 3: Scope of work set out for this report:

1) Produce exposure estimates for populations surrounding the Santa Susana Field Laboratory (SSFL) for airborne releases from SSFL suitable for use in epidemiological studies and exposure/consequence assessment. Such estimates shall be made using standard dispersion modeling, once contractor receives estimates of historical emissions, and shall permit estimation of an upper bound estimate of exposures and/or 95/99% confidence limits.

2) Suggest alternate exposure surrogates that could be used in lieu of, or in conjunction with, modeled exposures.

Appendix 4.

January 17, 2001

Santa Susana Field Laboratory Workgroup Members:

The Ventura County APCD has been asked to describe the meteorological data that is available from its air monitoring sites in Simi Valley.

The APCD has two air monitoring sites in Simi Valley. One on the east side and one on the west side. The air monitoring site on the east side of the valley is located at Simi Valley High School (aka Simi Valley) and the other is on the west side of the valley at Simi Valley Landfill (aka Simi Upper Air) near Madera Road and the 118 Freeway.

The Simi Valley site has meteorological data available from 1991 until present. The Simi Upper Air site has data from mid-1995 until present. All of the data is reported as hourly averages. All of the data is collected in standard metric units. Depending upon the period of time needed the data may be available in digital or hard-copy format.

The Simi Valley site collects: Wind Speed Average, Wind Speed Vector, Wind Direction Average, Wind Direction Vector, Sigma Theta, Temperature, Relative Humidity, Total Solar Radiation, and Visibility.

The Simi Upper Air site collects: Wind Speed Average, Wind Speed Vector, Wind Direction Average, Wind Direction Vector, Sigma Theta, Temperature, Relative Humidity, Total Solar Radiation, Ultra Violet Radiation, Barometric Pressure, and Rainfall. In addition, the Upper Air Profiler collects atmospheric profiles (Wind Speed, Wind Direction, and Temperature vs. Height) averaged over 15-minutes, in 60 meter intervals from the surface up to 2 kilometers.

Meteorological data sets for 1991, 1992 and 1993 that is suitable for use with the EPA ISCST dispersion model is available for download on the APCD's website at http://www.vcapcd.org/air_toxics.htm - metdata at the bottom of the page.

I was also asked to determine what meteorological data is or has been collected at the Santa Susana Field Laboratory itself. Boeing declined to provide me with any information about meteorological data collected at its site.

Sincerely,

Karl E. Krause, Manager
Engineering Division

Citations.

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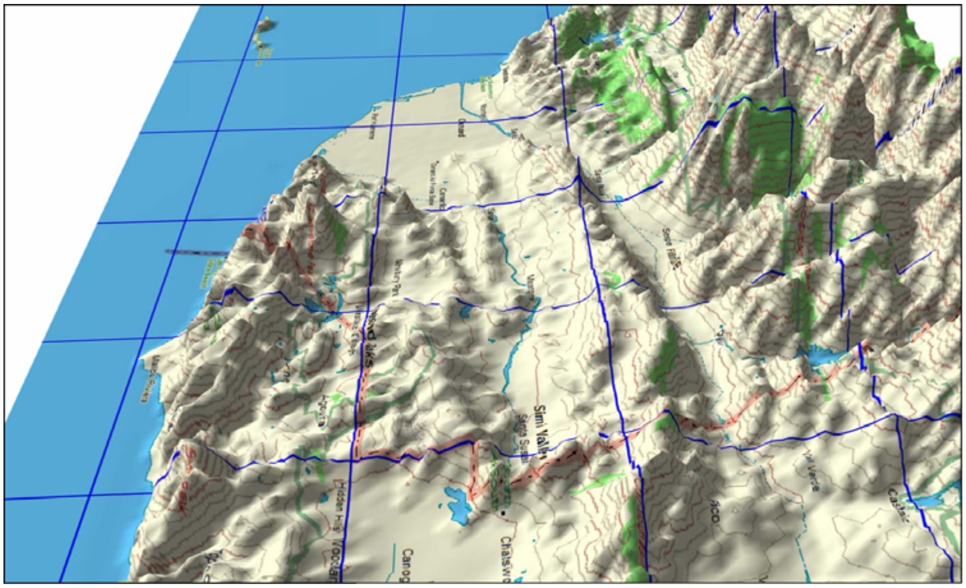
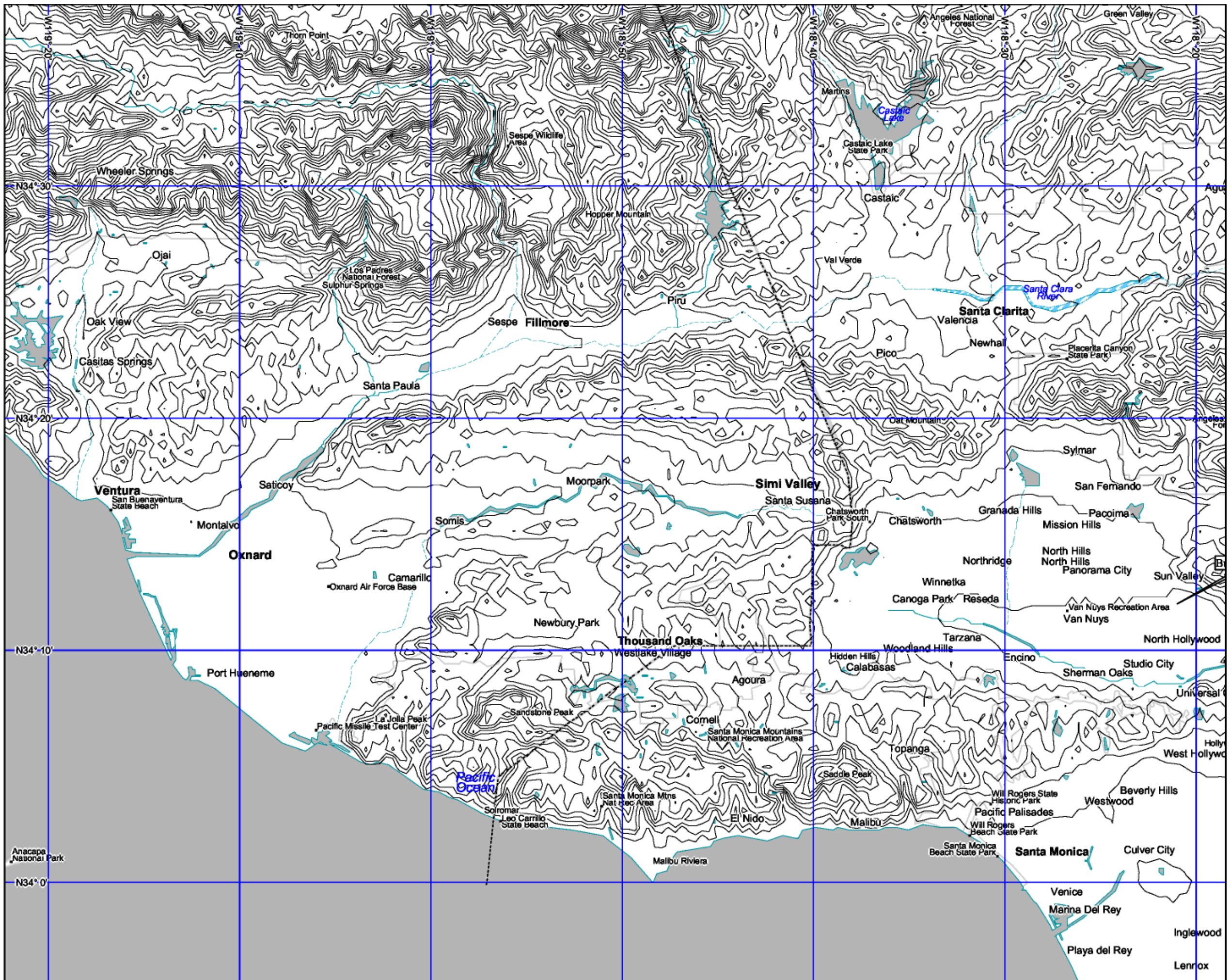
Figure A2-3. View of the Oxnard Plain from Chatsworth in the San Fernando Valley, showing the connection to the North of the Santa Monica Mountains. Exaggerated vertical height.





3-D TopoQuads Copyright © 1999 DeLorme Yarmouth, ME 04096 3 mi Scale: 1 : 400,000 Detail: 9-0 Datum: WGS84

Figure A2-4 (above). 2-D contour map, showing the ridge-shaped nature of the Santa Monica Mountains, with the low-lands behind.