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University of California

LOS ALAMOS SCIENTIFIC LABORATORY

Post Office Box 1663 Los Alamos, New Mexico 87545

In reply refer to:
Mail stop: ITO

27 March 1981

Drs. Charles Kelber, Mel Silberberg
US Nuclear Regulatory Commission
Washington, D.C. 20545

Dear Charlie and Mel,

I wish to congratulate the two of you, your staff and contractors for what I believe is a tremendous piece of work in creating the document "Technical Bases for Estimating Fission Product Behavior during LWR Accidents," NUREG-0772. You have managed to put together what I believe is the first comprehensive review of the chemistry of iodine and cesium in relation to LWR accidents and the first systematic study of fission product transport and mitigation phenomena. In addition, the analyses of "worst case" accidents has been started in a manner vastly superior to the treatment in, for example, WASH-1400. The field of reactor safety and reactor accident analysis will never be the same again.

My comments on NUREG-0772 will be brief and I do not pretend to be complete.

1. At several times during the meeting Mel commented that a cutoff date was necessary in order to produce a document, but that a revised NUREG-0772 could be promised in some months. I urge that this be done and that a date for a revised version be set. Many experts (e.g. from Los Alamos) cannot possibly contribute before the April 1 deadline.

2. Dr. Richard Vogel, representing EPRI, volunteered the full cooperation of EPRI and other experts from the industry. I urge that this offer be accepted and that input be broadened to include contributions from other National laboratories and from experts from other countries.

3. It was stated several times that the study was meant to be realistic, a best-estimate. Yet, in several places in the text and during the presentations, this was not the case. I urge that the study be scrutinized to assure that best-estimate assumptions (with plausible variations) are always made.

4. Along with others, as expressed in the meeting on March 17 and 18, I urge that the various assumptions buried in the MARCH, TRAP, MELT, CORRAL, etc. codes be written as clearly as possible.

5. At various places in the text it is not clear whether the writer is discussing the output of one of the computer codes, or discussing a "gedanken experiment." These should be identified and clarified.

6. In some places, assumptions are made that may or may not be generally true, but which then lead to what seem to be predetermined results. These should be identified, stated clearly, alternative assumptions made (if nearly equally plausible) and results determined. As an example, I offer the postulate of particulates that escape from molten fuel and on which cesium iodide is then postulated to condense. These particulates are then expected to ride through the system like a noble gas. All this can be argued as unlikely and alternatives can be postulated. The consequences could be very much different and should be stated. One can question the existence of particles of the right size at the right time, timing of escape of C_sI, temperatures of the fuel, effect of steam, temperatures of local surfaces, etc.

Such clarification would, in addition, sharpen the areas in which experiments and theoretical studies are needed.

You may correctly assume that I disagree with the second conclusion in the abstract.

7. Along with others, I urge that the several codes (MARCH, TRAP, MELT, etc.) be examined by organizations who were not involved in their creation or development.

8. As time passes and the most important areas for investigation emerge, the NRC should consider creation of competitive computer programs to assure realistic treatment.

9. All of us will be able to identify areas for further investigation, either theoretical or experimental. I think it would be especially worthwhile for the authors of the various sections state their opinions. They have investigated the subject of their section more than anyone else.

10. Some of the postulated amounts of aerosols emitted by a molten core are extraordinary. I urge review of this area.

11. I ask that the experimental validation of the several computer programs be described.

In conclusion, I repeat my earlier congratulations. It is a good document and I look forward to the published document.

Sincerely,

Bill Stratton

W. R. Stratton

WRS:dp

xc: ITO Files

Particular Questions Regarding Chapter 5.3

1. The existence of HOI and how it affects iodine partition coefficients is not well established. What are the probable answers to these questions?

Answer: There is little doubt that HOI can exist in aqueous solutions when conditions favor HOI. Aqueous systems at equilibrium in an LWR accident will have insignificant quantities of HOI. However, there may be pre-equilibrium conditions such that the HOI concentration could be as much as 50% of the total iodine concentration. The primary concern then is with regard to the HOI partition coefficient. The partition coefficient, PC, (the concentration in the aqueous phase divided by the concentration in the gas phase) certainly favors the liquid phase. In Chapter 5.3, the magnitude of the partition coefficient was conservatively set at two times that for the molecular I_2 species [PC(HOI) = 2(83) = 166]. However, the HOI coefficient probably is much greater and could easily be as great as 10^3. A partition coefficient of 10^3 would imply that the concentration of HOI in a gas phase above an aqueous solution in an LWR accident is insignificant even at times before the aqueous solution equilibrates. Even though this has not been absolutely proven, some recent excellent work using very sensitive analytical procedures could not detect HOI above aqueous solutions at conditions favoring HOI [R. J. Lemire, D. J. Paquette, D. F. Torgerson, D. J. Wren, and J. W. Fletcher, Whiteshell Nuclear Research Establishment, AECL-6812 (1981), also private communication with D. F. Torgerson].

2. How does the difference in the kinetics of reactions 5.1 and 5.4 affect the total iodine partition coefficient?

$$I_2 + H_2O = HOI + 2H^+ + I^- \qquad 5.1$$

$$3HOI = IO_3^- + 3H^+ + 2I^- \qquad 5.4$$

Answer: Reaction 5.1 approaches equilibrium at a faster rate than that for 5.4. Therefore, the amount of HOI at short times may be considerably greater than the equilibrium concentration of HOI, and at those times the concentration of HOI in the gas phase would be proportionately higher. The extreme case for this situation would be when reaction 5.1 essentially reaches equilibrium before reaction 5.4 begins. This extreme situation is discussed in the text and the corresponding total iodine partition coefficients are given in figure 5.5 with an assumption that the HOI partition coefficient is two times that of I_2 (see question 1 above). If the HOI partition coefficient is ten times that of I_2, as suggested above, the total iodine partition coefficients as given in figure 5.5 should level out at about 1660 rather than near 330. On the other hand, the coefficients for solutions at equilibrium, figure 5.4, would hardly change because the equilibrium solutions contain insignificant amounts of HOI.

3. The distribution of a given amount of iodine into the various iodine species in an aqueous solution will depend on the redox potential of the solution. What will be the effects of the redox potential being controlled by chemicals other than iodine?

Answer: Probably the greatest uncertainty is the effect of hydrogen and oxygen on the aqueous iodine chemistry. The real effects can be addressed as introduced in Appendix C.5. There the oxidation of iodide, I^-, to molecular iodine, I_2, and on to iodate, IO_3^-, was shown. Similar information can be generated for the effects of hydrogen or other chemicals by proper utilization of redox potentials given in reference 5.16. However, the kinetics of such reactions are not well known and a time frame could not be estimated.

4. An iodide, I^-, source dissolved into LWR water will remain as the nonvolatile iodide species unless the oxidation potential is such that oxidation occurs. An iodine, I_2, source in LWR accident quantities will react with water, and at equilibrium essentially all of that source will have been converted to nonvolatile iodide and

iodate species. What will be the effects of LWR accident radiation on those aqueous systems? Will the oxidation potential be changed by radiation such that volatile iodine species will be formed?

Answer: Chapter 5.3 and Appendix C.8 clearly state that the immediate effect of radiation on an LWR accident aqueous system will be the well-known effects of radiation on water. The question then becomes, what will be the effects of water radiolysis products on the iodide and iodate species? The water radiolysis products and the relative amounts are given in equation C.8.1. Water in an LWR accident will have many impurities which will significantly scavenge the water radiolysis products before they can interact with the iodine species. However, the extent of scavenging could be only approximated and the iodine species interacting with the water radiolysis products deserve consideration. The oxidizing radical, $\cdot OH$, would react rapidly with appreciable concentrations of iodide to form I atoms and hence molecular I_2. However, the water radiolysis products include an equivalent or greater number of reducing agents (e^-, $H\cdot$, H_2O_2, H^-) that could reduce iodine species to iodide, I^-. At the same time any atomic or molecular iodine would also tend to react with water as discussed in Chapter 5.3.5. From these considerations the I_2 molecule is the least stable iodine species in an LWR aqueous system in a radiation field.

5. Organic iodide will form by the reaction of molecular iodine with organics such as methane and lubricants. What is a reasonable rate of organic iodide buildup after an LWR accident?

Answer: At this time there is no decision on the amount of organic iodide that could be expected in an LWR accident, and an estimate of a rate of formation is therefore unrealistic. However, Chapter 5.3, Section 5.3.9, and Appendix C.9 suggest that 0.03% of the iodine that exists as atomic or molecular iodine, I_2, would be converted to organic iodide. We have no basis for estimating the time required for the small amount of molecular I_2 to be converted to organic iodide under LWR accident conditions. Good and applicable experiments are needed.

Reviewers' Comments and Authors' Responses
Chapter 5.3 and Appendices C.5—C.9

J. T. Bell

Reviewer - J. B. Ainscough

Comment 1: More consideration could be given to the difference in primary circuit water and water released into the CB.

Response 1: Yes, more attention could be given to specific differences in those waters. However, the conditions for both waters are within the ranges considered in the text except when the primary circuit water is >100°C. Extrapolation of data to temperatures beyond 100°C would not be wise.

Comment 2: Vapor-surface and solution-surface chemistry are largely neglected, etc. The chemistry of iodine compounds on steel surfaces needs detailed consideration and experimental investigation.

Response 2: This is a very good point. Molecular iodine, I_2, in an aqueous solution in contact with structural alloys is reduced to iodide by the iodine-metal reactions, especially at temperatures near or greater than 50°C. A comment will be added in the text, but information is not readily available for a detailed discussion.

Comment 3: Why is the I_3^- species not considered?

Response 3: A statement will be added to the text that solutions with 10^{-5} M or less total iodine should have insignificant amounts of I_3^-.

Comment 4: Kinetic data on reaction 5.4 are lacking, so it may be preferable to accept Eggleton's model where the iodate formation is omitted.

Response 4: Such kinetic data are needed as suggested. However, the model without iodate formation should not be accepted because the kinetics at higher temperatures, >50°C, are such that iodate and its additional iodide formation are very significant. This chapter gave data for the case of final equilibrium of iodine species in water and for the formation of HOI and I^- alone. These two cases are the extreme conditions and certainly bracket all realistic systems.

Comment 5: The hydrolysis of methyl iodide is barely worth including in any model.

Response 5: Methyl iodide does react with water to convert a volatile iodine species to a nonvolatile species, and this reaction is especially fast near 100°C. This is good information and should stay in Chapter 5.3. In fact, this reaction may be part of the reason that methyl iodide concentration in TMI has been at a steady state.

Reviewer - D. F. Torgerson

Comment 1: pH is a function of temperature and could drop to 5.5 at 25°C.

Response 1: Correct. The pH range of 7 to 11 will be changed to 5 to 11.

Comment 2: HIO_3 can be more abundant than I_2.

Response 2: Perhaps there is some indication of this. However, since HIO_3 in aqueous solution is a strong acid, the authors would need much good data to believe that HIO_3 could exist in the pH 5 to 11 range.

Comment 3: pH values should be indicated to be at the indicated temperatures.

Response 3: Will do.

Comment 4: Avoid taking a definite stand on HOI at this time.

Response 4: Agree and will soften the statement.

Comment 5: In Appendix C.6 the comment on no rate studies is not correct.

Response 5: Will correct the statement. See comment by Brewer.

Reviewer - R. K. Hilliard

Comment 1: The data in this report should be compared with Eggleton's.

Response 1: Eggleton's paper and data are referenced and may be compared. The coefficients in figure 5.4 are similar to those of Eggleton except that Eggleton did not include HOI in the gas phase. We assumed that HOI could be half as voaltile as I_2 and included HOI in the gas phase.

Comment 2: Calls attention to work done at temperatures up to 120°C in the CSE program.

Response 2: These references are good for transport or removal studies. The authors felt that those sources did make some observations but did not define iodine chemistry.

Reviewer - A. W. Castleman

Comments on the terms static, nonequilibrium, and unimolecular reaction are appreciated and will be clarified.

Comment 1: The last sentence of first paragraph on page 5.26 appears misleading if no_ incorrect.

Response 1: That statement refers to a system that has only one iodine species and that one species is iodide. If a system has iodide changed to some other species, it would not be encompassed by this statement.

Comment 2: Regarding Appendix C.6 and entropies.

Response 2: The entropy change determined by Turner was a negative value, -8.9 e.u. The ΔH value was positive, 14170 cal/mole, and these values give a positive ΔF at 25°C of 16840 cal/mole.

Comment 3: Equation C.7.1 includes several intermediate steps or reactions.

Response 3: A good point, and will be noted in the text.

Comment 4: Unimolecular reaction should refer only to the aqueous phase.

Response 4: Will be noted in the text.

Reviewer - R. H. Buckholz (GE)

Comment 1: The partition coefficients in figure 5.5 are not in agreement with data reported in Docket RM-50-2, Draft Regulatory Guides, and in a paper by Lin that is in press.

Response 1: The lines in figure 5.5 represent hypothetical cases where the iodine system is not at equilibrium and where the HOI partition coefficient is assumed to be conservatively low (two times that for the I_2 species). These hypothetical cases represent the extreme lower limits if reaction 5.1 approaches equilibrium before reaction 5.4 begins. This is unlikely and experimental data should be compared to that in figure 5.5 only to show that the experimental partition coefficients are greater. Observed coefficients greater than those in figure 5.5 would imply two points: (1) reaction 5.4 begins before reaction 5.1 approaches equilibrium; and (2) the HOI partition coefficient is greater than two times that for I_2.

The calculated coefficients in figure 5.4 represent equilibrium systems and should be compared to experimental data. In fact, values in figure 5.4 are several orders of magnitude greater than those in figure 5.5 — just as the reviewers indicated the coefficients should be.

Comment 2: The reviewer suggested consistency in the amounts of iodine that may be expected to convert into methyl iodide.

Response 2: The authors of Chapter 5.3 suggested that 0.03% of the molecular iodine, I_2, in a LWR accident could be expected to convert into methyl iodide, and they suggest that the authors of Chapter 1 change the 0.1% conversion factor for total iodine to 0.03% of the molecular iodine. The Chapter 5.3 authors agree with the reviewer that the conversion factor for total iodine would be even smaller because most of the iodine will exist as ionic species, but the authors are not prepared to suggest the 0.005% of total iodine conversion factor.

Comment 3: the reviewers commented on the conditions that reaction C.5.11 could occur.

Response 3: The authors generally agree with the reviewer that the reaction is promoted by acid. However, this does not mean that I^- would not react with O_2 in base solution. For example,

$$2I^- + 1/2O_2 + 2H^+ = I_2 + H_2O, \ K = 2.6 \times 10^{23} \ ,$$

$$\text{at pH} = 7, \ (H^+)^2 = 10^{-14} \text{ and } \frac{I_2}{(I^-)^2 (O_2)^{1/2}} = 2.6 \times 10^9 \ .$$

Comment 4: The reviewer pointed out that I_2 and HI species react with reactor construction materials.

Repsonse 4: The reviewer is correct that the reaction of molecular iodine, I_2, with construction materials can reduce I_2 to iodide, I^-. A comment to this effect will be added to the text.

Reviewer - Richard C. Vogel - EPRI

Comment 1: Vogel noticed the suggestion that CH_3I is a less serious problem than was once anticipated. He suggested that this prediction be more reflected in the report or abstract.

Response 1: See Response 2 to reviewer Buckholz.

Reviewer - R. M. Wallace - Dupont, SRL

Comment 1: The reviewer questioned the implication that iodine would exist as iodide and iodate in a radiation field.

Response 1: We have no evidence that volatilization from aqueous iodine solution increases under gamma radiation. In fact, Lin's works (References 5.44 and 5.45) indicate that radiation effects produce the nonvolatile species.

Reveiwer - Lennart DeVell - Studsvik

Comment 1: In the summary, too much emphasis is put on the formation of iodate. We know the reaction is relatively slow even at 100°C.

Response 1: This is the only review to state this. The authors do not agree. The formation of iodate may be always relatively slow compared to the formation of HOI or whatever the first reaction produces. However, the relative rates lose significance when the slower rate becomes significant, and the rate of formation of iodate is very significant at 100°C.

Reviewer - Leo Brewer - Berkeley

Comment 1: There are straightforward procedures for extrapolating partial molal heat capacities -- but the conclusions of the report do not depend upon the sharpening of the accuracy of the thermodynamic calculations.

Response 1: The authors used the straightforward procedure of Helgeson to extrapolate the equilibrium constant for reaction 5.1 to 100°C and to calculate the data in table 5.2. However, the constant for the intermediate reaction 5.3 at 100°C was calculated by extrapolation of the Arrhenius plot of data over the 10—60°C range, and better thermodynamics for that intermediate reaction could give "sharper" hypothetical concentrations of I_2, HOI, and I^- in table 5.4, figure 5.3, and figure 5.5. The authors agree that "sharpening the accuracy of the thermodynamic calculations" would not affect the conclusions.

Comment 2: The major defect in the presentation is illustrated by the statement on page C.15 that there are no known rate studies of the conversion of iodine to iodate. ...This has been a very extensively studied reaction...etc.

Response 2: The statement starting on page C.15 was "There are no known rate studies of the direct reaction C.6.3."

$$3I_2 + 3H_2O = 5I^- + IO_3^- + 6H^+ \quad \text{C.6.3}$$

Dr. Brewer has pointed out that the rate of iodate formation has been studied extensively by following the rate of disappearance of I_2 and I_3^-, and of the intermediate species HOI, IO_2^-, and I_2O_2, and that a rate expression for the formation of iodate can be derived from the studies of the intermediate reactions and intermediate species.

The statement has been deleted from the text with immediate discussion of Eggleton's work. Then a statement was added on page C.16 "Other studies on the disproportionation of IO^-, the hypoiodate ion (reference 5.62) generally support the conclusions of Eggleton."

Reviewer - A. K. Postma

The comments of Postma on Section 5.3.9 of Chapter 5 have been reproduced directly and authors' responses have been injected.

Most of my comments are directed to Section 5.3.9 "A Less Conservative Assessment of Organic Iodide Formation" (WASH-1233). My overall conclusion is that the data base on organic iodide formation is not sufficient to support an accurate, realistic prediction of organic iodide formation for a specified accident. Given this limited information base, realistic predictions of organic iodide formation are bound to be quite subjective. More experimental studies will be required if one is to come up with a reliable, accurate estimate of organic iodide formation. However, the upper limit prediction arrived at in WASH-1233 is supported by the discussion in NUREG-0772 in that the 3.2% conversion is cited as being "highly conservative."

Response

The authors completely agree that the data base on organic iodide formation is insufficient to yield accurate conclusions. Some wording changes are suggested to make this point clear.

a. *Adequacy of Data Base for Accurately Predicting Conversion to Organic Iodides*

The present data base on organic iodide formation is not sufficient to allow one to make a reliable, accurate prediction of fractional conversion to organic iodides for a specified accident condit.on. All of the mechanisms and parameters which control organic iodide formation are not known. Also, the values of some parameters expected to be important, such as the concentration of airborne organic compounds, are not well known for the accident case. These uncertainties would be expected to allow different workers to arrive at different projections of organic iodide formation. It is specifically for this reason that an upper bound estimate was provided in WASH-1233. The difference between the prediction provided in NUREG-0772 and the earlier upper bound predicted in WASH-1233 reflects two factors:

- *best estimate versus upper bound estimate*
- *inadequacies of data base for accurate prediction of organic iodide formation.*

Response

This comment is covered in Section 5.3.9, especially paragraph 2. To emphasize this point further, add to page 5.27, paragraph 1 (just before "Conclusions"): "With the information available, it is not possible to derive a firm predictic organic iodide formation, and the large disparity between this .mate and the prior one is indicative of the extent of uncertaint ."

b. Degree of Conservatism in WASH-1233

On page C.22 of the subject report it is stated that the upper limit estimates made in WASH-1233 were "...deliberately chosen to be highly conservative..." This statement does not accurately reflect the objective of WASH-1233. Actually, the objective was to identify the lowest organic iodide fraction which could be defended as demonstrably conservative. The number arrived at, 3.2%, was a factor of ~3 lower than the 10% fraction which had been used prior to the publication of WASH-1233.

The many areas of conservatism discussed in WASH-1233 were highlighted in that report to provide a convincing argument for lowering the assumed organic fraction. The author of Section 5.3.9 appears to have interpreted such discussions in WASH-1233 as signifying that a "highly conservative" estimate was desired, whereas the goal actually was to identify the lowest estimate which could be defended as being an upper bound.

Response

The motivation and objectives of WASH-1233 are beyond the scope of the report. The point is, it was a conservative treatment, whatever the reason. On page C.22, paragraph 1, change the next-to-last sentence to read: "These conclusions were based on interpretations that were highly conservative, and the conservatisms were clearly stated."

c. Elemental Iodine Form in Experiments

On page C.22, it is stated that "Essentially all the data in WASH-1233 was for experiments and tests in which molecular iodine (I_2) was the dominant or exclusive chemical form of iodine introduced into the experiment." This statement does not fairly reflect the fact that a number of the tests used fission product iodine evolved from heated UO_2 fuel, clad in zirconium, and released into gas streams composed of varying amounts of H_2O, H_2, and Ar. Examples are the tests of Hilliard(1) (tests IA32 and IB42), the tests of Freeby(2) (CDE runs 3 and 4), and the measurements of Perkins(3) (the PRTR incident). Organic iodide conversions which resulted from the iodine released from UO_2 fuel were found to be consistent with results obtained in experiments where I_2 crystals were vaporized. The agreement in behavior between the simulated iodine (I_2) and that released from fuel supports the validity of the tests which used elemental iodine. An inspection of Maypack data obtained by Hilliard(1) for tests IA32 suggests that more than 70% of the iodine was in particulate form.

In summary, the data set used to identify a conservative organic iodide fraction in WASH-1233 included tests in which fission product iodine was released from UO_2 fuel elements, and appeared in the containment atmosphere partly in particulate form. Therefore, the data base is not limited to iodine airborne solely as the elemental vapor as is suggested in the subject report.

Response

Most tests did use I_2, but a few used irradiated fuel, as pointed out. On page C.22, paragraph 2, change the second sentence from "Essentially all ..." to "Most of ..."

d. *Extrapolation to Accident Conditions*

The author of Section 5.3.9 has taken a different approach in applying the existing data base to accidents than was presented in WASH-1233. While the major difference stems from the goal of obtaining a realistic estimate rather than a conservative one, the predicted effect of radiolysis is appreciably different. Several approaches are used in the subject report and all of them lead to radiolytic conversions lower than approximately 0.02%. It does not appear to the reviewer to be possible to provide more than a subjective judgement of which approach is the more valid in providing a best-estimate prediction. One area of agreement is that the approach used in WASH-1233 yields a conservative estimate of organic iodide formation by radiolysis.

Response

Agree. No comment required.

e. *Hydrolysis of CH_3I*

On page C.23 of the report, it is stated that "Hydrolysis is reproted to be rapid at the elevated temperatures that exist in the containment building for a time following certain accident scenarios." While this statement may be technically correct, it should be recognized that organic iodides react very slowly with water at temperatures likely to be encountered in the containment atmosphere. This is shown in CSE test A-10[4] *where the removal half-time was 12 hours for methyl iodide when scrubbed by caustic sprays at 120°C. Thus, hydrolysis will not usually be a significant factor in reducing the airborne methyl iodide concentration.*

Response

The authors disagee with respect to hydrolysis rates reported in the literature. Rates are fast at temperatures around and above 100°C, and such temperatures are projected for many of the accidents of concern. This is an area that requires further study since some observations are in conflict with established literature data.

f. *Comparison with Measurements*

It would have been helpful if the author of Section 5.3.9 had compared his predictions with experimental measurements. For example, in the PRTR incident,[3] *approximately 10% of the iodine initially airborne in the containment atmosphere was converted to organic iodides. The best estimate method described in Section 5.3.9 appears to predict that less* *than 1% should have been converted to organic iodides. Admittedly, one cannot generalize on the basis of one data point, but neither is it very satisfying for the prediction to miss the best experiment by an order of magnitude or more.*

Response

This is beyond the scope of this study. One problem is that actual accidents (TMI, PRTR) usually are not sampled or analyzed until some time after the event, and there is no firm evidence as to how much iodine was airborne the first few minutes or hours. Also, since very low iodine concentrations were observed during these accidents, the fraction converted to organic iodide would be larger. The projected fraction of organic iodide (whether 3 or 0.03%) is for the case of a large release, yielding an airborne concentration around 10^2 mg/m^3; with a smaller release the fraction organic will be larger. Although the fraction is larger, the amount is much smaller.

g. Implications with Respect to I_2 in TMI-2

According to measurements reported by Pelletier,[5] approximately 0.003% of the core inventory of iodine became airborne in the containment atmosphere of TMI-2 as organic iodides. If one uses the best estimate assumption that 0.03% of I_2 is converted to organic forms, then one can back-calculate the amount of I_2 initially airborne. The I_2 airborne is predicted to be 0.003/0.03 or 0.1 of the core inventory. This estimate of 10% of the core inventory of iodine airborne as I_2 is orders of magnitude higher than would be expected for the TMI-2 scenario.

One must conclude that either (1) there was a large quantity of I_2 airborne in the TMI containment atmosphere or (2) that the fractional conversion of airborne iodine to organic iodides was much higher than the 0.03% figure arrived at in Section 5.3.9. The second of these two possibilities appears to be the more likely.

Response

This is related to (f). The 0.003% figure for TMI was measured long after the event. The first sample, a couple of days after the accident, showed 0.007% airborne iodine, but I understand that no species identification was done for months. During this long time, other reactions not included in the analysis (such as liquid phase or surface reactions) could yield small amounts of organic iodide, such as were observed. In addition, inorganic species would continue to be removed, leaving an increased fraction of organic iodide.

It should be remembered that a large fraction of the core inventory of iodine was airborne (or at least gas phase) during the accident; it was in the primary circuit rather than the containment building. However, this could still yield organic iodide since the mechanism for formation is unknown.

Thus, there are other possibilities that could be added to the two suggested. The important point is that such mechanisms apparently did not yield substantial amounts of organic iodide.

CHAPTER 1

Comments of Devillers

1. A table summarizing the differences with WASH-1400 results would be informative.

2. The report did not look at a long enough timescale to see the effect of chemical form.

Response

1. In this report the various stages of fission product release and transport were treated separately. By comparing the results obtained in this report with WASH-1400 assumptions, it is possible to evaluate in a semi-quantitative manner the extent of conservatism, if any, in the WASH-1400 analyses. Since there can be significant interactions between phases, however, it will be necessary to perform a consistent integrated analysis of each of the WASH-1400 sequences to obtain updated release estimates. This was beyond the capability of the analytical tools available to the authors of this report. This type of analysis has high priority in planned follow-on efforts.

2. The report did examine a range of characteristic timescales for LWRs of American design. There are some aspects of the analyses performed which artifically have made the results appear less sensitive to the chemical form of iodine than would actually exist. The parametric treatment of decontamination factors for pools and icebeds, for example, could not recognize the differences in the physical mechanisms associated with chemical form.

CHAPTER 1

Comments of Levine

1. The conclusions about the conservatism in WASH-1400 risk dominant sequences are premature.

Response

1. Agree. The conclusion is being rewritten with better recognition of the magnitude of uncertainties in the analyses.

CHAPTER 2

Comments of Malinauskas

1. Editorial comments.

2. Radioactivity cleanup system is not the principal source of radioactive material released to the environment during normal operation. Activation products are major radioactive species in coolant.

3. The comments "early fatalities would at worst be expected to occur... within a few miles of the plant". "Iodine-131...are potential major contributors to the dose...from the passing cloud...in severe accidents", indicate bias.

4. Engineered safety features were not effective in retaining chemically reactive fission products at TMI.

Response

1. Revisions have been made to address a number of editorial comments.

2. Disagree. Since activation products do not present the magnitude of hazard as fission products in an accident, it was not considered necessary to discuss them in Chapter 2.

3. Disagree. It is helpful to the reader to understand the significance of different radionuclides to health effects according to the risk analyses that have been performed to date. Clarification of the source of these results will be provided, however, with qualifications regarding uncertainties in release magnitudes.

4. Disagree. Partial performance of the ECC system was effective in providing water which made up for loss through the relief valve, keeping the pressurizer full and blocking the release of iodine and cesium to the containment atmosphere. The containment building was very effective in retaining fission products that did become airborne.

CHAPTER 2

Comments of Vogel

1. Chapter 2 needs rewriting.

Response

1. Partially agree. Chapters 2 and 3 are intended to provide introductory material to a non-technical reader in order that he can achieve some context for results presented in Chapter 1. A number of changes are being made in Chapter 2 to clarify the discussion. Adding a discussion of fission in plutonium isotopes is not necessary to meet the objectives of this chapter. Yes; genetic effects can be carried forward into future generations. A footnote has been added to Table 2.1 describing the basis of the inventories.

CHAPTER 3

Comments of Anderson

1. AB and S_2C are not necessarily risk dominant sequences.
2. Were multi-compartment MARCH runs used to support CORRAL-2 analyses?
3. NPSH specifications are such that the ice condenser spray system would operate after containment failure

Response

1. Further discussion on the bases for selecting sequences has been added to Chapter 3 which should provide adequate qualifications.
2. The MARCH 1.1 code available through the National Energy Software Center provides multi-compartment data for use in CORRAL-2. The interfaces are straight forward.
3. The assumed failure of the pumps was related to uncertainty in the mode of containment failure and interaction with the spray system rather than NPSH requirements.

CHAPTER 3

Comments of Buchholz

1. Conclusions are based on incomplete information on accident sequences and plant design characteristics.

2. Assumptions about dominant accident sequences affect the results of the report.

3. Assumptions made about BWR sequences are not in agreement with recent BWR PRA studies.

4. BWR transport pathways in the containment were not adequately considered.

5. There is a misunderstanding in the report about the compartmentation of the BWR plants

6. Modeling and assumptions used for dominant severe accident sequences do not adequately describe BWR transport pathway.

Response

1. Conclusions are being modified to better account for modeling uncertainties.

2. The report attempted to examine a spectrum of accident sequences with particular emphasis on those that were predicted to dominate the risk in WASH-1400 analyses. In Chapter 3, discussion is being added regarding uncertainties associated with thermal, hydraulic, and structural behavior of the plant in accident sequences and the potential impact of assumptions about plant behavior on fission product release estimates.

3. Accident sequence behavior is design dependent and in some instances subject to significant uncertainty. Some qualifying statements have been made to the discussion of sequences in Chapter 3.

4. No changes are required for the sequences analyzed. More discussion is provided in Chapters 3 and 7.

Buchholz (Continued)

5. In Appendix A, discussion of accident sequences was divided into two sections: A.2.1. Conditions in the Reactor Coolant System and Drywell. A.2.2. Containment Conditions. The intent was to consider the flowpath and conditions in the reactor coolant system and drywell generically in A.2.1 and then in A.2.2 to consider the flowpath and conditions in the suppression pool and the vapor space of the wetwell for the three different principal design variations in the containment. This apparently led to confusion as to why on pg. A-13 the discussion stops in the drywell. The discussion will be made clearer in the text.

6. The transport pathway in the analysis is as described in the comment. As discussed previously, the consideration of the flowpath on pg. A-13 ended with the drywell because the topic of Section A.2.1 is "Conditions in the Reactor Coolant System and Drywell". The following Section A.2.2 describes conditions in the transport pathway through the suppression pool to the overlying vapor space. This will be clarified in the report.

CHAPTER 3

Comments of Campbell

1. There should be a table like Table 2.1 that gives the amounts of fission products in grams as well as curies.

2. Could a common classification of accidents be used in the Regulatory Report and Technical Bases Report.

Response

1. This will be provided in Chapter 4.

2. Disagree. The classifications in the two reports serve different purposes. In the Regulatory Report, the groupings relate to off-site release characteristics. In the Technical Bases Report, the classifications relate to in-plant behavior.

CHAPTER 3

Comments of Devell

1. Additional knowledge tends to decrease release figures and probabilities for risk dominant sequences.

Response

1. Partially agree. Uncertainties in release fractions are skewed toward lower values. We expect additional research to lead to reductions in release estimates. It should not be assumed, however, that this will necessarily be true. No changes are planned in text.

CHAPTER 3

Comments of Devillers

1. It is unlikely that containment failure would follow immediately after core meltdown. A case of containment failure delayed many hours should have been examined.

Response

1. The authors recognized the possibility of the type of sequence described by the commenter. This is why the TMLB' sequence without containment failure was used as the basis for intercomparing a number of computer codes. The approximate consequences of the accident can be inferred from the airborne fraction at the time of containment failure.

CHAPTER 3

Comments of Hilliard

1. In the report, conservative assumptions have probably been made about thermal-hydraulic conditions in the primary system.

2. An attempt should be made to identify conditions in which an oxidizing atmosphere could exist at the time of release.

3. More detail should be provided regarding containment failure modes.

Response

1. Mostly disagree. The attempt was to be as realistic as possible. There are some variations of sequences, such as partial ECC operation where there could be water injected into the pathway. There is significant uncertainty about the presence of water in the pathway in other sequences such as TMLB'.

2. In Appendix A, an evaluation was made of accident phases for a variety of meltdown sequences to explore this possibility. Although there were some possible conditions of this type identified, they appeared to be comparatively unlikely.

3. In Chapter 3, greater discussion is being provided regarding the importance of containment failure modes and the related uncertainties.

CHAPTER 3

Comments of Levine

1. The difficulties in defining flowpaths and failure modes are not stated.

Response

1. In Chapter 3, discussion is being added regarding uncertainties in predicting failure modes and release pathways.

CHAPTER 3

Comments of Malinauskas

1. Editorial comments.

Response

1. Revisions have been made to address a number of editorial comments.

CHAPTER 3

Comments of Tong

1. Are the statements regarding risk-dominant accident sequences plant specific or generic? Different sequences would be expected to dominate the risk of different plant designs.

Response

1. The statements are based upon the reanalysis of the WASH-1400 plant sequences. This will be clarified in Chapter 3. A common characteristic of sequences predicted to dominate the risk in past studies, however, is that they involve failure of the containment near the time of core melting and reduced effectiveness of containment safety features in trapping fission products.

CHAPTER 3

Comments of Vogel

1. Because of the combination of unlikely events, the probability of containment failure in TMLB' should not be considered high.

Response

1. Disagree. The arguments provided relate to the probability of occurence of the TMLB' sequence not to the likelihood of containment failure given TMLB'.

CHAPTER 4

Comments of Vogel

1. Uninhibited fuel oxidation and Ru released in steam explosions was not addressed.

2. Uncertainties of ± 100°F shown in Tables B-2 and B-3 are not correct.

3. Existing models do not predict the failure of the grid plate or core barrel.

Response

1. This was a matter of priority. As discussed in Appendix A, the oxidation release term was not investigated because it is the opinion of the authors that experimental evidence since WASH-1400 has downgraded the importance of steam explosions. The fuel oxidation release terms must be examined further in the future.

2. Agreed. The notation was intended to show the interval width not the uncertainty. It is being changed in order not to be misleading.

3. The sentence is being modified to remove an implication that the codes model the support plate failure models mechanistically.

CHAPTER 6

Comments of ACRS

1. "Inasmuch as the use of computer code models play a major role in assessing the risks associated with various accidnets, it is important that work be continued on improving such codes.

2. This should include developing a better understanding of the soundness of the basic assumptions used in their preparation and in the identification of the range of uncertainties in the projections they produce.

3. Independent review and evaluation of these computer models would also be warranted."

Response

1. The author agrees with this assessment, and wishes to point out that work is currently being performed to improve both the TRAP-MELT and CORRAL codes. It is hoped that work can be performed which will expand the capabilities of the MARCH code in the future.

2. The major assumptions in the TRAP analyses are presented in Section 6.2 of the report. Through the discussions of the various sequences examined, and the presentation of the information in Appendix D, it was hoped that the interested reader would be able to make at least a rough assessment of the uncertainties in the TRAP predictions given in Chapter 6. A more detailed examination of the uncertainties in the TRAP code predictions can be found in a sensitivity study of the code which was recently completed. This level of detail is not really suitable for inclusion in the present report.

3. This is currently beginning, in an informal manner, through distribution of the code to parties interested in using it for analysis of LWR accidents. Attempts will be made to maintain a dialogue with the users to assess the code results, indications of problems within the code, and desirable alterations of the code. This process may become more formalized with the distribution of the reference version of the code to the National Energy Software Center.

CHAPTER 6

Comments of Ainscough

1. "The input assumptions are critical in assessing results from the TRAP code..." (p. 4, ¶1)

2. "... more detail of the influence of the input parameters on the characteristics of the release into the containment would be useful..." (p. 4, ¶1)

3. "... clarification of the relationship between Chapter 6 and Chapter 4..." (p. 4, ¶1)

4. "The TRAP runs usually stop at relatively short times after the start of an accident... (does this relate) to assumptions in the model or the source term." (p. 4, ¶2)

5. Regarding page 6-10, ¶3. Whether such high concentrations would arise during core heat up and melt down is highly questionable and the results of small scale experiments suggest that these values are too high." (p. 4, ¶3)

6. "... iodine in the various accident sequences has to be considered in more detail and with more clarity... the proportion of CsI, molecular iodine and organic iodides must be assessed." (p. 4, ¶4)

7. "There is little mention of other fission product elements, for example, tellurium." (p. 4, ¶4)

Responses

1. These are now listed in Section 6.2.

2. Such detail can be found in the sensitivity study of the TRAP code and is indeed useful for examining the influences of various input parameters. The goal of the present report, however, was to provide, insofar as possible, best estimate values of these parameters and examine effects of changing these estimates only to indicate the uncertainties contained in the TRAP predictions.

3. This has been attempted in the source term discussion in Chapter 6.

4. The time frame covered by TRAP analyses is duscussed in the first two paragraphs of Section 6.3. The time at which the TRAP runs stop is dictated by the MARCH code predictions of pressure vessel meltthrough.

5. These concentrations represent worst case scenarios. They may not, however, be unlikely under certain conditions. For the core melt sequences with very low flow rates (TMLB', TC), these high mass concentrations really appear to be possible. This is obviously a point which will receive further attention.

Ainscough (continued)

6. At the time these analyses in Chapter 6 were performed, it was not known what iodine species would be predominant. The analyses in Chapter 5 make it fairly apparent that CsI is the preferred iodine form for the sequences analyzed in this report. Since the TRAP code does not include chemical changes, the analyses were performed assuming the iodine to be present throughout the primary system as either CsI or I_2, with no interchange between the two forms.

7. The focus of the report has been on iodine. This is certainly not to say that other fission products are unimportant, but beyond the scope of the present work.

CHAPTER 6

Comments of Anderson

1. "... the major uncertainties in (thermal hydraulic conditions, extent and timing of any liquid water present, and chemical nature of fission products) need to be emphasized more clearly..." (p. 2, Section 6)

2. "Page 6-5 - the third and fourth complete paragraphs are very confusing." (p. 3, Section 6)

Responses

1. Section 6.2 has been strengthened in this regard.

2. These paragraphs have been reworked for clarification.

CHAPTER 6

Comments of Campbell

1. "... a detailed list (should) be presented giving the assumptions built into the programs." (p. 2, ¶3)

2. "Vaporization of control rod material seems to be ignored... Also, tin vaporization from cladding not be properly included..." (p. 2, ¶2)

3. "... why not put in the TMI accident... Since this is a good experimental value, and not a model, it should be treated in this report." (p. 2, ¶4)

4. "... statements like factor of 2 on Fahrenheit scale (p. 6-12) should be clarified." (p. 2, ¶5)

Responses

1. This list is now presented in Section 6.2.

2. Control rod material, tin from the cladding, and other materials are included in the aerosol mass source term, as discussed in Section 6.2, ¶3 and ¶4.

3. This issue is addressed in the comments regarding Chapter 1 in the report.

4. The statement has been changed for clarification, but a detailed listing of conditions used in the sensitivity study is not added to the chapter since they are availiable elsewhere.

CHAPTER 6

Comments of Castleman

1. "... when co-released from a source, some iodine can be adsorbed on surfaces... a few percent... likely present along with fine aerosol particles." (p. 3, 1st ¶, last 2 sentences)

Response

1. This is very likely to be the case. Inclusion of the adsorption process in the TRAP code requires use of empirical data. This mechanism is very much dependent on the surface involved and the temperature at the surface. Until experimental data are available regarding adsorption of I_2 (and potentially other fission products) on aerosol surfaces of interest in the primary system, this process cannot be properly considered in TRAP. It is worth noting that the condensation on particles of C_sI, which appears to be the predominant form of iodine in the primary system, is considered correctly in the TRAP code.

CHAPTER 6

Comments of Devell

1. "The knowledge (regarding retention effects) is still limited to a certain extent." (Item 3)

Response

1. Agreed.

CHAPTER 6

Comments of Devillers

1. "Cesium hydroxide, CsOH, has not been studied explicitly." (Item 8, ¶2)

2. "... there is no qualified model for coremelt conditions (for steam flow rates and system temperatures." (Item 8, ¶2)

3. "... particle source term kinetics and particle agglomeration inside the primarily system should receive special attention." (Item 8, ¶3)

4. "... a large fraction of cesium and iodine releases would occur at temperatures at which the particle source term is very low..." (Item 8, ¶3)

5. "The retention of tellurium should also be estimated..." (Item 8, ¶4)

6. Particulate matter retention in dry system seems low, given the high aerosol concentrations which would exist. (Item 8, ¶5)

7. No consequences are drawn from the high release fraction for tellurium given in Chapter 4. (Item 9, ¶1)

Response

1. CsOH is among the myriad species not studied explicitly in this report. The focus of this report has been on fission product iodine and CsOH has been considered only insofar as it represents a competitive fate for the Cs which may combine with the iodine to form CsI.

2. This is correct, and bears repeating, as it is a significant source of uncertainty in the results of this chapter, and others.

3. The author interprets "particle source term kinetics" to refer to nucleation of particles. The reasons for not treating this process mechanistically are now presented in Section 6.2. The lack of particle agglomeration in the primary system is now discussed more fully in Section 6.3.2.[illegible], but it is correct that it should receive special attention.

4. This is certainly true for any isolated portion of the melting core. It must be kept in mind though, that different portions of the core experience different time-temperature profiles, and as a result, more species are emitted simultaneously than would perhaps be expected.

Devillers (Continued)

5. True, especially in light of its importance as an iodine precursor.

6. This point is somewhat cleared up in the discussion regarding coagulation at high mass concentrations (Section 6.3.2.2.1). The retention values cited in the conclusions have been corrected to reflect the influence of the high concentrations in the primary system.

7. To reiterate, iodine is the principal focus of this report, although it is unsatisfying to give incomplete treatment of iodine precursors. This should receive attention in the near future.

CHAPTER 6

Comments of Hilliard

1. "The estimates of FP transport were performed with computer codes that are largely unvalidated. This is especially true for transport in the primary system." (Item 3)

2. "A more exhaustive review of the literature on FP release and transport... should be performed. This should include a review of experiments and past accidents." (Item 3)

   "No past accident is typical of the severe core damage accidents emphasized in this report."

3. "Values of code input parameters (should be) given." (Item 5)

4. "The assumptions used and an estimate of the impact of their uncertainty should be provided." (Item 5)

5. "The calculations made in the report (for the primary system) are least well substantiated by experimental evidence." (p. 4, ¶1)

6. "The conditions assumed for the TRAP-MARCH code predictions should be clearly stated." (p. 4, ¶1)

7. "The effect of range of possible deviations, especially thermal hydraulic, should be shown." (p. 4, ¶1)

8. "The statement is made that after the core has left the pressure vessel there would be no further retention of FPs in the primary system. I question this..." (p. 4, ¶2)

9. "The statement is made that elemental iodine is not expected to interact significantly with particles." Experimental evidence from the CSE program is given which ostensibly contradicts this.

Response

1. This issue is discussed in the comments regarding Chapter 1 in the report.

2. Such a review could prove helpful, especially if experimental conditions relevant to primary system conditions during a core melt sequence could be identified and used to test TRAP predictions. The author is very doubtful regarding the usefulness of what can be gleaned from past reactor accidents. This issue is discussed in the comments on Chapter 1 in the report.

Hilliard (Continued)

3. Detailed TRAP input data are presented in Appendix D for two sequences analyzed. This information is presented to permit the reader to assess the level of input required for TRAP and the potential sources of uncertainty associated with it. See Response A at the end of these comments and responses.

4. The assumptions used are now presented more clearly in Section 6.2. It has been attempted in the discussion throughout the Chapter to illustrate where the uncertainties due to the assumptions made have an impact.

5. This is certainly true. The reason for this can be found in the extremely adverse conditions which characterize a primary system during a core melt sequence. These conditions are a serious deterrent to most experimenters. It would be very worthwhile to attempt to experimentally verify portions of the TRAP code, at conditions which are relevant to accident situations.

6. Between Section 6.2 and Appendix D, I believe this information is available.

7. The report has attempted to address this point by analysis of two different sequences, simulated using, for each, two sets of thermal-hydraulic conditions. It was pointed out in the report (Section 6.3.2.2.1 and 6.3.2.2.2) that this was done to indicate the range of uncertainty in trap predictions due to the uncertainties in the input thermal hydraulic data. To exercise TRAP through a complete range of thermal hydraulic conditions for each sequence considered is more appropriately set forth in a sensitivity study of the TRAP code, rather than in an analysis of fission product transport.

8. There is, of course, the possibility of some flow into the pressure vessel after the molten core has fallen into the containment beneath it. It is impossible to asses the extent of flow into the failed pressure vessel after vessel failure, but it would be quite difficult to support the contention that this would be an important fission product retention mechanism.

9. There are some experimental indications that a few percent of the iodine in some systems may be adsorbed on particle surfaces. This phenomenon is very much dependent on the surfaces and temperatures involved and as experimental data relevant to primary systems become available, they will be included in the TRAP code. Nevertheless, in light of the finding of this report that molecular iodine is not the dominant form of iodine in the accidents considered, such interaction is not expected to be an important mechanism for fission product retention in the primary system.

CHAPTER 6

Comments of Levenson

1. "... the existing computer models and codes treat chemistry, aerosol physics and similar phenomena either inadequately or not at all." (p.1, ¶2)

2. "... the bulk of the available consequence data is a result of accidents and large experiments and that data does not confirm the calculated consequences." (p.1, ¶2)

Response

1. The existing computer models do have deficiencies which contribute to uncertainties in the resulting predictions. These have been noted in the Chapters which employ numerical simulations for analysis of accidents. The codes employed in Chapter 6 represent the best available means of examining fission product behavior in the primary system after a LWR accident. It is not clear from this comment whether the reviewer is referring to specific shortcomings of the TRAP-MELT code, or merely expressing an opinion arrived at after scanning the draft report.

2. This issue is addressed in the responses to comments on Chapter 1 in an Appendix to the report.

CHAPTER 6

Comments of Levine

1. "... the study of the transport of aerosols in the primary containment is in its infancy..." (Item 2ii, ¶2)

2. "... the assumptions ... are not clearly stated..." (Item 3)

3. "... nor is it always clear how well the various codes used have been validated by comparison with experiment. This is particularly true of the TRAP_MELT code." (Item 3)

4. "... the assumptions and simplifications that go into defining flow paths are not clear..." (Item 3)

5. "One of the key elements of this sort of analysis is the definition of the flow path into the containment... flow path diagrams such as Figure D7 and D8 conceal a host of assumptions and simplifications." (p. 4, Item 2)

6. "... aerosol behavior, particularly in the primary system, is a major source of uncertainty." (p. 4, Item 3)

Responses

1. This is perhaps true, although a bit overstated. The analysis presented in Chapter 6 of this report must be considered a significant advancement over the state of the art at the time of WASH-1400, which gave credit for no attenuation of fission products in the primary system.

2. They are now listed in Section 6.2.

3. This item is discussed in the comments concerning Chapter 1 in the report.

4. It is hoped that Section 6.2 now clarifies them.

5. The intent of the flow path diagrams in Appendix D was to illustrate the assumptions and simplifications employed in performing the TRAP analyses.

6. Aerosol behavior, under a given set of fully specified environmental conditions, is not uncertain. The major difficulty in this chapter is the specification of the conditions to which the aerosol is subject, and it is agreed that there are numerous uncertainties in this area.

CHAPTER 6

Comments of Malinauskas

1. "I see nowhere any provision for the formation of aerosol or the calculation of primary particle size distribution." (p. 10, ¶1)

2. "Documentation, particularly with regard to input data is poor." (p. 10, ¶2)

3. "... I seriously question the wisdom of citing any results of TRAP-MELT runs." (p. 10, ¶3)

4. "A more detailed description of the code, and a listing of all the underlying assumptions are in order. Also, references should be cited for of the mass transfer equations employed." (p. 10, ¶4)

5. "... there are no experimental tests of the overall processes, and particularly of the thermal hydraulic condtions assumed (this is especially true of the conditions within the primary system)." (p.11, ¶5)

Responses

1. This is discussed in the response to Postma's comment #1, and is also discussed in Section 6.2 of the report now. It is good to keep in mind that within fairly wide bounds, the initial particle size distribution is relatively unimportant since at the high concentrations in the core region the aerosol will rapidly approach a distribution which is insensitive to the initial distribution. Given the complicated aerosol dynamics in the core region it is difficult, in fact, to define what is meant by primary particle size distribution.

2. This item is discussed in response A at the end of the comments on Chapter 6.

3. The only reasonable alternative to citing results of the TRAP-MELT analyses of the sequences considered in the report is adoption of the WASH-1400 approach, which allowed for no fission product retention in the primary system. This would add nothing to our understanding of fission product release and transport.

4. This point is addressed in Response A which follows the comments on this Chapter.

5. This issue is addressed in the comments on Chapter 1 in the report.

CHAPTER 6

Comments of Moeller

1. Further discussion of coagulation's influence on particle growth, and the role of residence time is required.

Response

1. The discussion of these matters in the draft report has been expanded for purposes of clarification and to include results of work which was being performed when the draft was circulated for review.

CHAPTER 6

Comments of Postma

1. "The TRAP code does not realistically treat self-nucleation or the attachment to pre-existing aerosol particles." (p. 1, ¶3)

2. "The size distribution of particulate CsI was assumed to be the same as fuel particles, which were stated to be in the size range of ten to hundreds of micrometers." (p. 7, Item II)

3. "Two phenomena not included in TRAP: self nucleation of CsI... and attachment of CsI to the small particle size fraction ..." (p. 7, Item II)

Responses

1,3. The TRAP code does not realistically treat self nucleation of particles. As discussed in Section 6.2, the very low vapor pressure species are assumed to nucleate in the core region, and particle formation elsewhere is not permitted. For the severe core damage cases considered in Chapter 6, with CsI co-emitted with the aerosol from the core, self nucleation of CsI is unimportant. The sequence involving minor fuel damage (Section 6.3.1) may present the opportunity for CsI nucleation to occur in competition with condensation on the relatively cool system surfaces.

The perception that CsI attachment to pre-existing aerosol particles is not treated realistically is due, I believe, to the misunderstanding expressed in pt. 2.

2. This fuel particle size distribution pertains only to the sequence involving minor or no fuel damage (Section 6.3.1). These particles are produced by mechanical attrition of the fuel pellets, and are therefore much larger than those produced in any of the sequences involving a core melt. In the core melt sequences, the CsI is associated with the much smaller particles which are present (if the conditions permit condensation of the vapor on the particles).

CHAPTER 6

Comments of Ritzman

1. "... the method and assumptions used to generate thermal-hydraulic input data for the TRAP-MELT calculations should be provided." (Item 6)

2. "... it should be stated in the conclusions that the results presented in Sections 6 are obtained entirely from computer code analysis, the outcome of which depends on the validity of the models and mechanisms that make up the code." (Item 6)

3. "The empirical mass release rate expressions are based on very limited data, the TRAP-MELT Calculations in Section 6 include no particle coagulation dynamics." (Item 10)

4. "... the conclusions ... should be identified as tentative and subject to change..." (Item 10)

Responses

1. This not really a straightforward matter, and this represents a significant source of the difficulties in the current state of the art. Presentation of this information in the report would be of interest to only a very few of the intended audience - probably only those interested in performing TRAP analyses.

2. The first and second sentences of the conclusions (Section 6.6) indicate that the conclusions are drawn form TRAP analyses. The author suspects that anyone reading this chapter will recognize that the validity of the code determines the validity of its predictions.

3. The mass release rate expressions are indeed based on limited data. This deficiency is being addressed in current experimental programs. The TRAP-MELT calculations do not include coagulation. This aspect of the analyses is discussed in Section 6.3.2.2.1.

4. While these specific words have not been used to describe the conclusions, appropriate qualifiers are now believed to be in place in Section 6.6.

CHAPTER 6

Comments of Scherer

1. "Review and evaluate past accident experience." (Item 2)

2. "Consider the time dependent aspects... in particular, the timing of the mechanisms required for core melting." (Item 3)

3. "Evaluate and quantify the assumptions and uncertainties inherent in the computer code models." (Item 4)

4. "Compare codes to data derived from previous accidents." (Item 4)

Responses

1. This issue is discussed in the comments on Chapter 1 in the report.

2. This issue is discussed in the comments on Chapter 6 in the report.

3. The assumptions and uncertainties have been identified in Chapter 6 as fully as is practicable. It is, of course, impossible to quantify the uncertainties in the TRAP code without first quantifying those in the input data used by the code. Unfortunately, this cannot presently be done for either the thermal hydraulics input or the source terms developed for melting cores. This represents a longer term goal than could be encompassed in the present report.

4. This issue is discussed in the comments on Chapter 1 in the report.

CHAPTER 6

Comments of Stratton

1. "... the various assumptions buried in the ... codes (should) be written as clearly as possible." (Item 4)

2. "One can question the existence of particles of the right size at the right time, timing of escape of CsI, temperatures of the fuel, effect of steam, temperatures of local surfaces, etc." (Item 6)

3. "Some of the postulated amounts of aerosols emitted by a molten core are extraordinary." (Item 10)

4. "I ask that the experimental validation of the several computer programs be described." (Item 11)

5. Independent examination of the codes, creation of competitive codes. (Item 7 and 8)

6. Not clear whether computer output or gedanken experiments are being discussed. (Item 5)

Responses

1. These are now listed in Section 6.2.

2. The timing of the releases of the various materials from the core is an important question. The resolution of this question requires quite detailed analysis of the thermal profile of the core and the geometries it assumes as the melt progresses. Such analyses could remove some significant uncertainties regarding the source terms to be used in TRAP.

3. A molten core is the center of truly extraordinary conditions. Independent estimates of the aerosol source strength of a molten core differ by surprisingly little. This is also true of the estimated mass concentrations achieved in the core region.

4. This issue is discussed in the comments on Chapter 1 in the report.

5. Independent examination of the TRAP code is beginning, through distribution of the code to interested parties. Creation of a competitive code seems premature since there is no experimental validation of the current code.

6. This is clarified in the revised chapter.

CHAPTER 6

Comments of Thompson

1. "The limitations, and the extent of validation, of computer codes should be explicitly discussed." (p. 2, Item 4, ¶2)

Response

1. The limitations are presented in Section 6.2 in the form of assumptions and uncertainties. For the validation point, this is discussed in the comments on Chapter 1 in the report.

CHAPTER 6

Comments of Vogel

1. "... codes are being used which are oversimplifications of the true situation." (p. 2, ¶1)

2. "The output from the primary system is assumed to be unchanged in particle size distribution by the time it reaches containment. The high temperature, high concentration agglomeration of particles is very rapid and particles entering containment may be as large as 100 μm." (p. 2, 3)

3. Lack of FP retention in primary system. (Specific comments, Abstract and Ch. 1, etc.)

4. "No aerosol agglomeration. Thermal-hydraulic data unclear." (Specific comments, Abstract and Ch. 1, etc.)

5. "Iodine assumed I_2 rather than CsI. Partially addressed." (Specific comments, p. 2)

6. "The TRAP-MELT code is deficient in many areas important to the assessment, e.g., the use of lognormal distribution for aerosols (instead of bimodal one)." (Specific comments, p. 2, bottom)

7. "The TRAP-MELT code lacks benchmarking. This is a major deficiency of the study. We realize that this deficiency is noted." (p. 4, comment 2)

8. "The list of processes included in TRAP-MELT differs from the list given in the User's Manual by omission of Brownian agglomeration." (p. 9, comment 5)

9. Regarding 1st paragraph of Section 6.2 "(it could be pointed out) that increases in released mass do not increase leaked mass proportionally." (p. 9, last comment)

10. Regarding 2nd paragraph of 6.2 "Slower diffusion of particulates would mean higher concentrations and thus more agglomeration and removal by sedimentation." (p. 10, 1st comment)

11. Regarding p. 6.4 "... the possibility of significant adsorption of I_2 by particulates is not properly taken into account. Old experiments at AI (ref.) show that there is indeed very effective scavenging of I_2 vapor by sodium oxide smoke..." (p. 10, 2nd comment)

12. Regarding 2nd paragraph of 6.3 The acceleration and turbulence associated with a sweeping of all particulates into the containment (at the time of vessel failure)... would, in itself, likely cause significant agglomeration and subsequent fallout. ... At the very least the particle sizes reaching the containment will be much larger than 1 μm." (p. 10, comment 3)

13. Regarding Section 6.3.1 ¶7-13 "... it is suspected that the wrong decay heat curves were probably used." (p. 10, 4th comment)

Vogel (Continued)

14. Regarding Section 6.3.2.2.1, ¶1 "CsI which is condensed on particles would be subject to aerosol removal processes and thus attenuate." (p. 10, last comment)

15. Regarding p. 6.10 "... gravitational agglomeration becomes effective in a relatively short time ... turbulent agglomeration is also very effective." (p. 11, 1st comment)

16. "¶2, p. 6.10 is contradicted by last ¶ on p. 7.10".

17. Regarding p. 6.17, Section 6.6. The conclusion regarding elemental iodine attenuation is weakened by adsorption of I_2 on particles, and its significance is greatly reduced by the comment that the iodine is mostly CsI.

18. Regarding p. 6.18 "... analyses that omit agglomeration and fallout in the primary system cannot be conclusive." (p. 11, last comment)

Response

1. The computer codes used in preparing this report certainly contain many simplifications of the true situation in order to make solution of the problem practical. The assumptions employed in making these simplifications have been presented in Section 6.2 so that the reader may assess where oversimplifications may be introduced. It should be noted that the codes used represent the best available means for analyzing LWR accident behavior and this point has been disputed by none of the reviewers.

2. The influence of agglomeration on the particle size distribution is discussed more fully in Section 6.3.2.2.1 than was true for the draft report. Points of clarification regarding this comment are: (1) temperature has a relatively minor influence on coagulation rate, and (2) particles of 100 μm diameter are very unlikely to escape the primary system.

3. Although the TRAP-MELT and QUICK analyses performed for the primary system are unverified, they represent the best option available for analysis. Results of sensitivity studies were included to provide perspective on the effects of uncertainties in thermahydraulic and deposition models.

4. Regarding agglomeration, see above. Some thermal-hydraulic data are presented in the Appendix D. Regarding the level of detail provided, see Response A, at the end of these comments.

5. It appears that the approach used in Chapter 6 has been misunderstood. To cover the uncertainty associated with the chemical form of the iodine in the primary system, the accident sequences were analyzed assuming the iodine to be present as I_2, then reanalyzed assuming the form to be C_sI as is indicated by the analysis presented in Chapter 5.

6. It is agreed that the TRAP-MELT code has deficiencies. The use of a log normal distribution having one mode instead of two modes as this commenter suggests does not introduce any significant error into the

analyses. Section 6.3.2.2.1 deals with the influence of particle size distribution in more depth than the draft report did. It is hoped that the added discussion clarifies this item.

7. The author agrees, and noted in the report, that TRAP-MELT has not had the benefit of experimental validation of its predictions in an integral fashion. It should be pointed out, however, that many of the components of the code are based on understood and accepted theory or have been verified in laboratory experiments.

8. The omission of agglomeration from the processes in TRAP-MELT used for these analyses was necessitated by the much higher strength of aerosol source terms, compared with those available at the time when TRAP-MELT was developed. The extent to which this omission may affect the results presented in Chapter 6 is discussed in Section 6.3.2.2.1.

9. The statement made is true for most, though not all, situations. The point made by the reviewer is amply demonstrated in the results presented in Chapter 7, which considers containment processes.

10. The author does not understand this comment. The rate of agglomeration is directly proportional to the diffusion coefficient of the particles.

11. This point is addressed in the comments on Chapter 6 included in an appendix to the report. It is important to keep in mind that adsorption of vapors is very dependent on the surface involved, and there is not likely to be any sodium oxide smoke in the primary system of a LWR.

12. Enhanced agglomeration would be associated with the turbulence generated at the time of vessel failure. It would be very difficult to quantitatively assess the importance of this effect for every accident sequence. In general, it would be of potential importance for those sequences with low flow rates and consequently large residence times. For these accidents a significant portion of the material emitted from the core may still be resident in the RCS at the time of vessel failure. For the higher flow rate accidents, however, only a very small fraction of this material will still be in the RCS, and therefore this effect would be insignificant for these sequences.

13. This issue is not germane to the analyses performed in Chapter 6. It is not the objective of Chapter 6 to determine whether or not fuel rods would rupture in the event of a terminated LOCA, but rather to analyze the behavior of material released if such ruptures occur.

14. This statement is true and is addressed throughout Chapter 6.

15. The discussion of agglomeration has been expanded to include further analyses not present in the draft report.

16. The paragraph referenced in Chapter 6 was concerned only with Brownian agglomeration, while Chapter 7 included other agglomeration mechanisms. This has been reconciled in the final version of Chapter 6.

17. See response #4.

18. To reiterate, agglomeration has been analyzed separately from the TRAP analyses, and these results are now included in the report. The conclusions have been modified to account for these additional analyses.

CHAPTER 6

Response A

The description of the TRAP-MELT code presented in Chapter 6 and the input data given in Appendix D are intended to describe the salient features of the code in sufficient depth to enable the reader to evaluate the shortcomings of the code and sources of uncertainty in its predictions. Judging from the number of comments received regarding the code's shortcomings and the relatively few reviewers' requests for a more detailed description of the code and its input, it appears that the level of information contained in the present report is appropriate. For the interested specialists, the TRAP-MELT code User's Manual (Reference 6.1) provides a more detailed description of the code, and the input data used for all simulations presented in Chapter 6 are available upon request from the authors. The authors do not believe that the inclusion of this material in the report would really assist the intended audience in its assessment of the Chapter and its conclusions.

CHAPTER 7

Comments by ACRS

1. Since computer codes play a major role in assessing risks from accidents, work should be continued in improving such codes. (p.2, item 5)

2. More attention should be given to the effects of chemical changes and chemical properties of fission products on the performance of removal systems, behavior in the environment, and associated health effects. (p.2, item 6)

Resolution

1. Agree. Work is currently progressing in this area.

2. Agree. Extension of TRAP-MELT code analyses into the containment, as is currently underway, will allow chemical form to be considered in more detail.

CHAPTER 7

Comments by Ainscough

1. Would be interesting to include computed accumulated mass leaked as a function of time to show the effect of the various models and the consequence of time to failure. (p.4, last ¶)

2. Steam condensation is dismissed as a minor effect. This is surprising and further studies are required to assess this effect. (p. 5, 1st full ¶)

3. Relationship between I_2 and CsI is not clearly explained. (p. 5, 2nd full ¶)

Resolution

1. Results for the TMLB' case have been included in Chapter 7. Results of calculations estimating time to failure are included in detailed description of containment calculation input.

2. Steam condensation was minor effect for the single case considered. Other sequences could show different effect.

3. Clarification is provided in Chapter 7.

CHAPTER 7

Comments by Anderson

1. Data from experiments in CDE facility at INEL were not considered. These data need to be reviewed for applicability to this report. (p. 3, item 7.1)

2. Some estimates of accuracy and validity of codes should be provided in terms of error bands assigned in describing transport and release of fission products. Perhaps this could be done by comparing calculations with available data from various accidents (TMI-2, SL-1, Chrystal River 3, TNT, etc.). (p. 3, item 7.2)

3. The aerosol mass source term appears to be an assumption or estimate. Because of its importance to filter plugging this needs to be based on a better treatment of data. (p. 3, item 7.3)

4. The assumed v lue for containment leakage needs to be listed in Table 7.3. (p.3 item 7.)

5. Were multi-compartment MARCH calculations used to provide input to CORRAL-2? A brief discussion of MARCH/CORRAL-2 interfaces should be provided as part of the uncertainty analysis. (p. 3, item 7.5)

6. Make clear that uncertainties in transport through the primary system are much greater than uncertainties in containment. (p.3, item 7.6)

7. Comments are made regarding best estimate failure times and conditions for some sequences.

Resolution

1. Agree that data should be reviewed but experimental basis for containment codes is well established.

2. Accidents are not good for comparing with codes because the conditions and results are not known well enough to allow comparisons to be made. Further, accidents have been for conditions leading to minimal leakage and not high risk cases.

3. Source used in containment calculation was consistent with release rates prescribed by Chapter 4 analyses. Further, parametric variations in source were considered to evaluate effects of source strength.

4. Agreed. This is being included.

5&6. Appropriate changes have been made in the text of Chapter 7.

7. Changes have been made in Chapter 7 recognizing the possibility of delayed containment failure.

CHAPTER 7

Comments by Buckholz

1. Similar behavior of iodine and iodide is assumed. (p. 1, Item 1)
2. The draft report did not reference a number of studies of DF in pools. (p. 2, Item 2)
3. Inadequate treatment of suppression pools. (p. 2, Item 2)
4. Dispute that chemical form of iodine doesn't affect release. (Category A, Item 1)
5. Overall attenuation factor of 2-10 is overly conservative for BWR sequences. (Category A, Item 2)
6. Zero removal credit was assumed for saturated pools (Category A, Item 3)
7. The assumption of suppression pool failure is not valid for all BWR containment designs and the assumption of DF in a boiling pool is not justified for any containment design. Therefore the chemical form of iodine is important for all accident sequences. (Category A, Item 7)
8. Scrubbing in suppression pool should be noted. [Category A, Item 8(a)]
9. Inconsistency between DFs used in Chapter 7 and results in Appendix E. (Category B, Item 4)
10. Since form of iodine is likely to be CsI or HI, larger partition coefficients should have been calculated using Eq. E-9. [Category B, Item 6(a)]
11. Solubility of CsI should be accounted for in scrubbing of CsI particles. [Category B, Item 6(b)]
12. Iodine chemical form does not have a major influence on consequences. (Attachment, 2nd Comment)
13. Consequences have not been overestimated by order of magnitude. (Attachment, 3rd Comment)
14. Behavior of iodine and iodide in the suppression pool is similar under severe accident conditions. (Attachment, p. 1.II, 1st Comment)
15. Assumed decontamination factors. (Attachment, p. 2.II, 2nd Comment)
16. GE comments on impact on risk. (Attachment, p. 2.III)

Resolution

1. Report assumes both CsI and I_2 to be vapor at high temperatures in primary system and CsI to be particulate at lower temperatures in containment and passing through ESFs. Extremely dissimilar behavior is assumed for particulate CsI.

2. References which were provided are being included in Appendix E with some discussion. (Also see Attachment Comment I.2.)

3. Additional discussion of suppression pools is provided in Chapter 7 and Appendix E.

4. Because of parametric treatment of pool DF, potential affect of chemical form could not be observed in the analysis. Conclusions of Chapters 1, 6, 7, and abstract are being changed to clarify this.

5. Because of magnitude of uncertainties, this conclusion has been modified. The GE comments do not adequately recognize the potential for bypass of the suppression in some accident sequences following containment failure.

6. In the cases analyzed in which the pool was saturated at the time of core meltdown, the containment had failed previously and it was assumed the suppression was subsequently ineffective. These cases all involved the Mark I design.

7. Considered under Items 1 and 2.

8. Scrubbing was credited in the analysis. The effect will be discussed in Chapter 7.

9. Discussion is being added to Chapter 7 on why DFs were treated parametrically. The assumed failure of the torus is the primary reason for the selection of small DFs.

10. Disagree. CsI is likely to be transported as an aerosol and Eq. E-9 is not applicable.

11. Disagree. Solubility is only relevant if particle contacts water. Even if a liquid film forms on the particle, it does not matter whether the CsI is in solution in the film.

12. Changes are required in the report to clarify the meaning and reasons for the apparent insensitivity of the calculated results to the chemical form of the iodine. The mechanisms conpassing through a suppression pool are substantially different. In the investigation of the BWR sequences, DFs were treated parametrically to examine the potential importance of retention in the pool. Recognizing that the DF of 100 used in the sequences analyzed with a subcooled pool could be low, an analysis of these sequences with a DF of 1000 will be included. For the two cases analyzed in which the ppol is boiling, the containment is calculated to fail in the suppression pool area prior to fuel melting. Not only is there great uncertainty regarding the amount of retention for particulate and elemental iodine in a boiling pool but in these cases there is some question as to whether there would be water in the pool.

13. As discussed above in some accident sequences, the suppression pool would not only be boiling at the time of fission product release, but, in addition, failure in the suppression pool structure would precede fuel melting.

14. The commenter appears to feel from his discussion that cesium iodide would transport through the suppression pool as a vapor. Analyses in Chapter 6 indicate that CsI would condense on particles and would subsequently transport through the pool as a suspended aerosol in the rising bubbles. The CsI partition coefficent is not relevant to this calculation. The data on aerosol removal in the suppression pool were inadequate to predict the DF and the DF was therefore treated parametrically. We agree that the mechanisms for removal of elemental iodine and particulate iodine differ substantially and that the DFs could be greatly different. This will be clarified in the report.

15. As stated in responses to other comments, in those sequences where a DF of 1 was assumed, the containment had failed in the region of the wetwell prior to core meltdown. Thus, there are two questions involved: the effectiveness of a boiling pool in the removal of fission products and whether the release will be into water because of the location of the failure in the wetwell region.

    The time of containment fialure predicted in the AE_{γ} sequence is very sensitive to the amount of oxidation of zirconium occurring in the period before pressure vessel failure. In the analysis presented, virtually all of the zirconium was reacted following slumping into the lower plenum are very uncertain. For analyses with less reaction of the zirconium, the failure time is extended.

    The ability of the pumps in the newer reactor coolant system designs to pump saturated water is certainly a positive feature. Accident sequences of this type were not analyzed in this report for the newer plant designs.

    Since an empirically supported model was not available to calculate the amount of decontamination in the pool, the DFs were treated parametrically. The final report will include results with DF = 1000 for the cases where the suppression pool is subcooled.

16. The authors are not as optimistic as the commenter regarding the decontamination available from the pool for some very low probability accident sequences. Results for in-vessel behavior that will be described in Chapter 6 indicate that for some important sequences there could be significant retention of aerosols in the reactor coolant system. Report conclusions are also being modified in this regard.

CHAPTER 7

Comments by Campbell

1. There will be a lot of water in the containment and condensation on surfaces. Is this taken into account? Apparently the water from the primary system is assumed to be not present to interact with fission products. (p.2, 1st ¶)

2. Why not analyze the TMI accident rather than a TMI-like accident and use it as a check of the methods? (p. 2, ¶4)

Resolution

1. The location and form of water in the containment building is of major importance regarding the potential for interaction with airborne fission products. Water in the sump is treated in the analyses as a horizontal surface and would be expected to be similar to other surfaces in the containment which would be covered with a water film. Transport of fission products to walls with condensing steam and the growth of droplets by condensation in the atmosphere are important processes affecting removal in the containment. These are treated by the methods in Chapter 7. More discussion is being provided in Chapter 7 regarding these processes and modeling uncertainties.

2. There is considerable information to be gained by the analysis of TMI. It is not, however, a good basis for verifying codes developed for core meltdown accidents. This does not mean that the orientation of these models has been improper. Because of conditions in the primary system during the period of release from the fuel (water in the pressurizer and closed block valve in the critical time period), the soluble fission products were dissolved in water before reaching the containment in the TMI accident. The analysis of behavior in containment focuses heavily, therefore, on partition coefficients under the expected conditions. Since the release to the containment atmosphere would be very small regardless of whether the iodine were initially in the elemental or iodide form, the consequences of this type of accident do not have the potential magnitude as those in which the pathway to the containment is dry. Hence, emphasis has been placed in model development on accidents of this type. Since the TMI accident is a unique opportunity to obtain information about a real certainly be undertaken of the accident. However, the inadequacy of the available thermal hydraulic data has been and will continue to be a serious limitation for any detailed analysis. Attempts by the authors to analyze the release and transport behavior of fission products within the primary system and containment of TMI have been frustrated by inadequate thermal-hydraulic data.

CHAPTER 7

Comments by Devillers

1. How would data reviewed in Section 5.3 (Aqueous Iodine Chemistry) affect WASH-1400 predictions? (p. 2, Item 4)

2. In containment, steam condensed on particles will form fog of high concentration CsI and CsOH solutions. Radiation fields inside fog droplets or oxidation of CsI during H_2 deflagration are 2 possible I_2 sources. These were not considered. (p. 2, Item 5)

3. Adequacy of CORRAL depends on realism of aerosol concentrations and size distributions. CSE tests used Cs concentrations in the range 1 to 10 mg/m^3. For accidents, total aerosol concentrations of 10 g/m^3 are possible. Agglomeration at these higher concentrations could lead to more effective fallout, particularly in compartments near the source. (p. 2, Item 6, last ¶)

4. Is CsI particulate material assumed to distribute uniformly at constant mass ratio onto other particles (total mass) or to be associated with small particles and therefore less sensitive to particle source term? (p. 3, Item 6, 2nd ¶)

5. What is size distribution of particles as they enter the containment? (p. 3, Item 6, 2nd ¶)

Resolution

1. In general, WASH-1400 sequences involve dry pathways to containment and Section 5.3 results do not directly impact. Suppression pool behavior is an exception. Discussion is being added to Chapter 7 about suppression pool DFs.

2. Magnitudes of these sources are unknown. The exclusion of these sources is being noted in Chapter 7.

3. Although the particle transport and deposition calculated with the CORRAL-2 code is independent of aerosol concentration, such effects are treated mechanistically by the aerosol behavior codes used in Chapter 7.

4. It is assumed to distribute with a uniform mass ratio onto other particles.

5. An initial mass median diameter of 0.1 μm is assumed in the containment calculations performed with the aerosol behavior codes. The geometric standard deviation was taken as 1.5. This is being noted in Chapter 7.

CHAPTER 7

Comments by Hilliard

1. CORRAL code has never been used to predict CSE experiments but should be done for cases with and without containment spray operation. (p.4, 4th ¶)

2. Airborne mass concentrations in CSE were at levels much lower than would be expected in many assumed LWR accidents. Higher aerosol concentrations would lead to more extensive particle growth and more rapid settling. (p.4, 5th ¶)

3. The mode of failure for the reactor containment building is very important and more detail should be given on this in the report. (p.5, 1st ¶)

4. Ratio of settling area to containment volume most important feature affecting scale and in this regard the CSE is close ($\sim$1/2) to full size. (p.5, 2nd ¶)

5. Principal questions regarding the CSE correlations concern the degree of realism of the CSE environment and fission product simulant. Low aerosol concentrations, high iodine concentrations, saturation at 250 F, probably low natural convection currents, low quantity of water on floor should cause CSE correlations to be conservative relative to severe reactor accidents. (p.5, 2nd ¶)

6. Tests in CSTF confirm basic premise of hole plugging by aerosols for ducts from 1 to 10 inches in diameter. (p.5, 3rd ¶)

7. A more exhaustive review of the literature on ...transport through the primary and secondary containment spaces should be performed. This should include a review of experiments and past accidents. (p.2, item 3)

8. The code input parameters, assumptions, and an estimate of the impact of their uncertainty should be provided. (p.2, item 5)

Resolution

1. Agreed. Although this is outside the scope of the report, a comparison of CORRAL-2 and CSE experiments is needed.

2. The effects of aerosol concentration levels are predicted by the aerosol behavior codes used in Chapter 7.

3. Answered elsewhere.

4. Partially agree. Settling area to volume ratio will be more important when aerosol sedimentation predominates but surface area is important for molecular iodine deposition and very low concentrations of small aerosols.

5. Possibly true but degree of conservatism unknown at this time and the effects are not expected to be major.

6. Hole plugging is an uncertain area. Not whether it occurs but what types of holes are likely to be present. Failure made analyses will be needed before aerosol plugging of holes can be reasonably included as an attenuation factor.

7. Agree. This exhaustive effort should be part of a continuing effort to upgrade the present report.

8. Additional information is being given in Chapter 7.

CHAPTER 7

Comments by Levenson

1. "Existing computer models and codes treat chemistry, aerosol physics and similar phenomena either inadequately or not at all." "In the text, some of the shortcomings of the codes are acknowledged. But the codes and models being questioned were just used again." (p.1, 2nd & 3rd ¶'s)

2. The "bulk of the available consequence data is a result of accidents and large experiments and that data does not confirm the calculated sequences." This is dismissed "by saying that the information from the accidents and large experiments is not what is required for model or code input (Just because the model doesn't fit what really happened doesn't mean it didn't happen.)." (p.1, 2nd & 4th ¶ s)

Resolution

1. CORRAL-2 (similar to the CORRAL code used in WASH-1400) was the only code used in both reports. TRAP, HAARM-3, QUICK, and NAUA calculations were performed to examine the effects of mechanisms treated empirically in CORRAL-2.

2. See Item 2, comments by Anderson.

CHAPTER 7

Comments by Levine

1. In containment calculations, source terms were used in which no credit was taken for attenuation in the primary system. Yet the source term "is crucially dependent on what takes place in the primary system, not only for determining the quantity of radioactivity present but also for determining important aerosol characteristics such as the particle size distribution". (General Comments, No. 2.i)

Resolution

1. Agree. This type of analysis was not possible within the scope of this review. Containment transport behavior could definitely be affected by conditioning of the source as it passes through the primary system. Capabilities for performing this type of consistent analysis are under development.

CHAPTER 7

Comments by Malinauskas

1. No discussion of aerosol formation is given. The NAUA code does not calculate aerosol generation. (p.11, 1st ¶)

2. To what extent is the condensing of steam onto vessel surfaces expected to enhance plateout? (p.11, 2nd ¶)

3. "Apparently no elemental iodine is allowed to deposit on the core melt aerosol in the containment building, whereas all the CsI so deposits." (p.11, 3rd ¶)

4. "The absurdity of the conclusions of this chapter, viz., that greater amounts of less volatile species will escape from the reactor than more volatile materials, merits a serious examination of all of the input, the code innards, and especially the assumptions employed." (p.11, 4th ¶)

5. ..."There are no experimental tests of the overall processes, and particularly of the thermal-hydraulic conditions assumed...". "In other words, we're not sure that the 'dry' accidents can actually occur at the time that CsI is being released!" (p.11, 5th ¶)

Resolution

1. See Chapter 6, Malinauskas' comments.

2. Expect small effect on I_2. Particle removal by sedimentation predominates and overall effect of condensation expected to be small although fraction deposited on wall may be considerably increased.

3. Effects of I_2 deposition on particles can be evaluated by examining the particulate analyses. Fraction of I_2 on particles is expected to be very low.

4. Volatility has nothing to do with the transport processes in the containment. Differences between aerosols and vapors are the significant factors.

5. Agree that overall experiments are desirable. See Chapter 3 fo sequence descriptions.

CHAPTER 7

Comments of Moeller

1. Only iodine source term was mentioned in the text. How about other FPs? (line 10, p. 7-1)

2. How does the ESF discussed in Chapter 7 relate to Chapter 8? (last sentence of paragraph 2, p. 7-1)

3. Is the filter effective measured as a part of the report? (paragraph 2, p. 7-2)

4. Are HAARM and QUICK applicable to LWR? (line 8 from below, p. 7-2)

5. The sentence does not read smoothly. Change it. (line 10, p. 7-3)

6. Where was NAUA developed? (paragraph 2, p. 7-4)

7. Discuss the advantages and disadvantages of NAUA. (paragraph 2, p. 7-4)

8. Are HAARM and QUICK independent? (paragraphs 1 and 2, p. 7-5)

9. 5 percent does not agree with the given table. (last paragraph, p. 7-24)

10. Discuss further (line 15, p. 7-32)

Response to Moeller's Comments

1. Basically iodine and cesium iodide are covered in the chapter. Other FPs are not greatly discussed because the state-of-the-art has not advanced sufficiently.

2. Added a sentence to indicate what is in Chapter 8.

3. No. The sentence has been revised to clarify the meaning.

4. Yes, and this is explained in p. 7-3.

5. The sentence has been revised.

6. In Germany. The clause was revised to indicate this.

7. Discussed already in Table 7-1.

8. Yes, and this is further clarified in the revision.

9. Revised.

10. Revised with the suggested discussion.

CHAPTER 7

Comments by Ritzman

1. Significant limitation results from the lack of containment failure mode analysis and qualitative treatment of fission product deposition along leak paths. These sources of uncertainty should be identified and the need for further work indicated. (p.1, item 3)

2. Major input data for containment codes should be provided. (p.2, item 6)

3. Containment aerosol behavior calculations assume well-mixed volumes which tend to limit coagulation and deposition rates. (p.4, item 10)

4. The effect of steam condensation on aerosol removal could not be treated properly because of inadequate thermal-hydraulic predictions. (p.4, item 10)

Resolution

1. See comment 6 by Hilliard.

2. Agreed. Additional data is being provided in Chapter 7.

3. Well mixed volumes do give most conservative estimates of airborne aerosol concentrations. This may not be true for molecular iodine. Effects are expected to be minor and for aerosols this question has been addressed analytically with the ZONE code which illustrated the minor effect.

4. Steam condensation was not necessarily limited by inadequate thermal-hydraulic predictions. Those used are considered to be best available.

CHAPTER 7

Comments by Scherer

1. Evaluate and quantify the assumptions and uncertainties inherent in computer code models. (p.2, item 4)

2. Compare code predictions to data derived from previous accidents. (p.2, item 4)

3. Investigate all removal mechanisms for aerosols in the containment. (p.2, item 5)

Resolution

1. Input data and assumptions are being described in Chapter 7. Uncertainties were evaluated by parametric variations and reported in Chapter 7.

2. See comment 2 by Anderson.

3. All known removal mechanisms except for turbulent deposition and diffusiophoresis were considered in the containment calculations with the aerosol behavior codes. All mechanisms are inherently included in the CORRAL-2 code.

CHAPTER 7

Comments by Stratton

1. Assumptions in codes should be clearly presented. (p.2, item 4)

2. Not always whether code output or experimental results. These should be clarified. (p.2, item 5)

3. Assumptions are sometimes made that lead to specific results which are dependent on the original assumptions. If equally plausible assumptions can be made that lead to differing results, these should also be evaluated. For example, it is assumed that C_sI condenses another condensation aerosols and is then transported as particles. This assumption can be questioned. (p.2, item 6)

4. Authors of various sections of this report should state their opinions regarding areas for needed future theoretical and experimental work. (p.3, item 9)

5. Experimental validation of computer codes should be described. (p.3, item 11)

Resolution

1. Additional information is being added in Chapter 7.

2. Text being revised for clarity.

3. Implications of assumptions and possible alternative assumptions are being addressed.

4. Areas for future work were previously summarized in Chapter 1 and are being reviewed and revised for the final version of the report.

5. Additional discussion about validation is being provided in the text.

CHAPTER 7

Comments by Vogel

1. Over-simplified models in WASH-1400 used without modification or qualification. (p. 2, ¶1)

2. Inadequate consideration is given to containment failure modes and deposition along leak paths. (p. 2, ¶2)

3. Treatment of aerosol processes is disjointed. (p. 2, ¶3)

4. WASH-1400 conservatisms were inadequately considered. (Specific Comments, p. 1 and 2)

5. No FP deposition in containment leak paths. (Specific Comments, p. 1 and 2)

6. No FP trapping in saturated water pools. (Specific Comments, p. 1 and 2)

7. No FP retention by auxiliary buildings. (Specific Comments, p. 1 and 2)

8. Total release of volatile FPs from fuel involved high bias of small experiments. (Specific Comments, p. 1 and 2)

9. Incomplete aerosol behavior modelling. (Specific Comments, p. 1 and 2)

10. No containment failure mode analysis. Puff release assumed. (Specific Comments, p. 1 and 2)

11. Further thought should be given to the sequencing of release phases. (p. 3, 2nd full comment)

12. Considering all of the structural material around the reactor cavity it is inappropriate to assume 50% release. Probability of sequences should be discussed. (p. 3, last comment)

13. Auxiliary building filter systems--primary system retention in V sequence. (p. 4, 1st comment)

14. TRAP-MELT code lacks benchmarking and should not have been used. (p. 4, 2nd comment)

15. Use of the phrase "relatively large residence times of the radionuclides in the containment" should be made more explicit because the time scale is set by Brownian agglomeration rates. (p.12, item 1)

16. Since HAARM-3 and QUICK both omit condensation, all LWR aerosol mechanisms have not been included. (p.12, item 2, ¶1)

17. The CORRAL 2 code is not very capable of representing agglomeration since its basis consisted of experiments performed at aerosol mass loadings about two orders of magnitude lower than could be encountered in a reactor accident. (p.12, item 2, ¶2)

18. Both HAA-3 and HAARM-3 will produce a source to a secondary containment so that multicompartmented analyses can be done. (p.12, item 3)

19. The potential for retention in the containment is not independent of aerosol behavior in the primary system. (p.12, item 4)

20. It would be helpful to know the particle or mass concentrations corresponding to the source mass for Figure 7.8. (p.13, item 1)

21. Leaked masses should account for attenuation along leak paths through the containment up to the point of plugging and sizes of leaked particles should account for growth in leak path. (p.13, item 2, ¶1 & 2)

22. In condensing steam atmospheres, the leak will quickly plug with water (p.13, item 2, ¶2)

23. COMRADEX-4 allows input particle size for attenuation calculations. The unpublished COMET code sums cases with different sizes to simulate a distribution. (p.1[illegible], item 3)

24. The containment seems to have been considered dry even though the large amount of water originally in the primary system may blow down into the containment and remain there unless removed. (p.13, item 4)

Resolution

1. Disagree. CORRAL was the only code used in this study from WASH-1400. Comparisons were made with a number of mechanistic aerosol codes: HAARM-3, QUICK, and NAUA. The treatment of core meltdown phenomena (MARCH code), primary system transport (TRAP), fission product release, and aerosol production represent significant extensions beyond WASH-1400 methodology. Further, these analyses were supported by a number of sensitivity studies.

2. Disagree. The conditions leading to containment failure, location of failure and mode of failure have a major effect on predicted accident consequences. This review has given limited attention to accidents in which the containment does not fail because the consequences associated with these accidents would be minimal. The consequences of these accidents could be reduced by the plugging of leak paths. The characterization of a major breach in containment is so far beyond the state of technology that analysis of deposition along the leak path would be very speculative.

3. Agree. An integrated analysis was beyond the scope of the program. Text is being added to Chapters 6 and 7 to better explain the implications of treating each phase separately.

Vogel (Continued)

4. Disagree. The intent of this report was to review the state-of-the-art for predicting fission product behavior. Although we feel that in many respects this review actually extend the state-of-the-art, there were clearly limits to what should be undertaken in the report. Furthermore, many of the so-called conservatisms claimed for WASH-1400 should actually be identified as possible conservatisms. In many cases, the presumed conservative assumption is made because there is no technical basis for selecting a less conservative model.

5. As discussed above, considering the state of ability to predict containment failure models assumptions about the potential for deposition in leak paths would be pure speculation.

6. The potential for trapping in saturated pools is discussed in Appendix E. However, data to support the verification of such a model is inadequate.

7. Potential was analyzed for event V and found to be small.

8. Best available data were used. The assumption that these results are highly biased is speculative.

9. The best available models were used to evaluate aerosol behavior.

10. The performance of structural analyses was considered to be beyond the scope of this report. At least a rudimentary consideration was given to containment failure modes for each design analyzed. No technical basis exists for the selection of a failure size small enough to prevent rapid depressurization following containment failure.

11. Agree. However, this is beyond the scope of this study. The statement that the early release would involve a lot more water present in the primary system might be true for some sequences but is not an accurate generalization.

12. Structural surfaces are taken into account in the analyses. More detailed analyses of behavior in the flowpath from the cavity might indicate greater potential for retention but are beyond the scope of this effort. Additional perspective is being provided in Chapter 3 regarding the probabilities of core melt accidents.

13. Disagree. There is a range of thermal hydraulic conditions that could happen in event V depending on the mode of check valve failure, location of low pressure system failure and details of auxiliary building design. In some cases, the auxiliary building filter system would have some effectiveness. It should be recognized that the well mixed assumption used to describe retention in the auxiliary building could be non-conservative and that the consequences could be higher as well as lower.

14. Disagree. TRAP-MELT has indeed not been verified against integral experiments to date. However, the intent of this review was to use the best information available. The TRAP code is the best model available for examining fission product transport mechanisms in the primary system. Not only have the deficiencies in TRAP been identified in the report, but the results of uncertainty analyses and sensitivity studies have been provided.

Vogel (Continued)

15. The potential for significant agglomeration is, of course, dependent on rates of aerosol removal, aerosol input and residence times. The general, introductory sentence in question has been modified to avoid the unnecessary discussion of these rates at their point in the text.

16. As was pointed out in the text, neither the HAARM-3 nor the QUICK code include condensation and were therefore used for cases in which condensation was a relatively minor effect and to serve as a basis for evaluating codes that include condensation. The CORRAL 2 code by virtue of its basis in CSE experiments includes condensation effects implicitly. The NAUA code considers condensation from a mechanistic approach and includes all aerosol behavior term expected to believed to be of major significance.

17. We agree that the CORRAL 2 is not very suitable for analyzing cases with significant aerosol agglomeration. The main usefulness of the CORRAL 2 is in analyzing systems where condensation is occurring. For these reasons, the HAARM-3, NAUA and QUICK codes were employed.

18. For once-through flows, multiple runs with the HAA-3 and HAARM-3 codes will accommodate multiple compartments. The HAARM-3 code is even equipped with a special output procedure to facilitate such calculations. For recirculating or reversing flows their use is not practical.

19. This is true but complete sequential calculations were considered outside the scope of thi_ report. Calculations with the output from one analysis (source, primary) providing input to the next analysis (primary, containment) are to be carried out as a part of the NRC research in this area.

20. The mass concentrations corresponding to the calculated results presented in Figure 7.8 were provided in Figure 7.12.

21. See comment No. 6, Hilliard.

22. This comment is in contradiction to data obtained in the Markiven full-scale containment experiments where it was found that subsequent to blowdown the leakage rate was increased.

23. We agree. The text is being revised to recognize these capabilities.

24. See comment No. 1, Campbell.

CHAPTER 7

Comments by Zumwalt

1. It is agreed that the assumed form of iodine does not appear to have a major influence on iodine release when there is core meltdown accompanied by containment failure. However, the effect of chemical form will have an important effect for lower risk accidents such as ones with partial core melting without containment failure. (p.1 item 2)

Resolution

1. It is important to recognize the magnitude of uncertainty in the predic 've capability of existing methods. Although very little dependence on chemical form was observed in the comparisons in Chapter 7, it is not appropriate to conclude that chemical form is not important. Transport behavior in the primary system and subsequent effects of conditioning of the radionuclide source by the primary system on containment behavior could be particularly important effects which at present are inadequately understood. Chapter 6 and Chapter 7 and report conclusions are being modified to better recognize these uncertainties.

CHAPTER 8

Comments of Anderson

1. The ice-condenser containment would not fail by over-pressurization due to non-condensibles alone.

Response

1. We recognize there are significant uncertainties related to the predicted failure of the containment. If substantial oxidation of the steel from the lower head and internals takes place, the quantity of hydrogen produced would result in containment failure. Qualifying statements will be made in Chapter 8.

CHAPTER 8

Comments of Buchholz

1. There is confusion in Table 8.2 about TW and TC sequences.

2. Most Mark III containments have an SGTS.

Response

1. Typographical errors are being corrected. In addition, values of temperatures in the table have been revised to represent steady temperatures in the drywell for the BWR and in the containment for the PWR.

2. The text is being corrected.

Reviewer	Comment	Resolution
ACRS Item 7		Gieseke/Kuhlman

Reviewer	Comment	Resolution
Ainscough Chapters 6&7		Gieseke/Kuhlman

Reviewer	Comment	Resolution
Anderson	Specific Comments	
	P1. Last comment, AB and S_2C are not necessarily risk dominant sequences.	Further discussion on the bases for selecting sequences has been added to Chapter 3 which should provide adequate qualifications.
	P2. Last comment. Affect of uncertainties in thermal-hydraulics should be emphasized.	Conclusions to Chapter 6 and Chapter 7 have been augmented accordingly.
	P3. 6.2. Chapter 6	
	7.1. Chapter 7	
	7.2. Chapter 7	
	7.3. Chapter 7	
	7.4. Chapter 7	
	7.5 Were multi-compartment MARCH runs used to support CORRAL-2 analyses?	The MARCH 1.1 code available through the National Energy Software Center provides multicompartment data for use in CORRAL-2. The interfaces are straight forward.
	7.6. Page 7-27 should not imply that there is more uncertainty in containment retention than primary system retention.	Appropriate changes have been made in the text of Chapter 7.
	8.1. Comments are made regarding best estimate failure times and conditions for some sequences.	Changes have been made in Chapter 7 recognizing the possibility of delayed containment failure.
	8.2. Ice condenser spray system would operate after containment failure.	Assumed failure of pumps was related to uncertainty in mode of containment failure rather than NPSH requirements.
	8.3. Ice-condenser containment would not fail by over-pressurization due to non-condensibles alone.	We recognize significant uncertainties. Qualifying statements will be made in Chapter 8.

Reviewer	Comment	Resolution
Buchholz Set 1 Pg1 ¶2	Conclusions are based on incomplete information on accident sequences and plant design characteristics.	Conclusions are being modified to better account for modeling uncertainties.
Pg1 Item 3	Assumptions about dominant accident sequences affect the results of the report.	The report attempted to examine a spectrum of accident sequences with particular emphasis on those that were predicted to dominate the risk in WASH-1400 analyses. In Chapter 3, discussion is being added regarding uncertainties associated with thermal, hydraulic, and structural behavior of the plant in accident sequences and the potential impact of assumptions about plant behavior on fission product release estimates.
Pg2 ¶1	Same as Attachment comments I.1, II.2.	
Pg2 Item 1	Same as Attachment comment I.4.	
Pg2 Item 2	The draft report did not reference a number of studies of DF in pools.	References which were provide are being included in Appendix E with some discussion. (Also see Attachment comment I.2.)
Pg2 Item 3	BWR capability to prevent severe core degradation and mitigate conseqeunces is not considered adequately.	See response to Comment Pg I, Item 3.

Reviewer	Comment	Resolution
Bucholtz Set 2 Pg2 Item 1	Technical adequacy of BWR sequences.	Some qualifying statements have been made to the discussion of sequences in Chapter 3.
Pg2 Item 2	Inadequate treatment of suppression pools.	Additional discussion of suppression pools is provided in Chapter 7 and Appendix E.
Pg2 Item 3	BWR transport pathways in containment inadequately considered.	No changes are required in sequences analyzed. More discussion is provided in Chapters 3 and 7.
Category A Item 1	Dispute that chemical form of iodine doesn't affect release.	Because of parametric treatment of pool DF, potential affect of chemical form could not be observed in the analysis. Conclusions of Chapters 1, 6, 7, and abstract are being changed to clarify this.
Item 2	Overall attenuation factor of 2-10 is overly conservative for BWR sequences.	Because of magnitude of uncertainties, this conclusion has been modified. The GE comments do not adequately recognize the potential for bypass of the suppression in some accident sequences following containment failure.
Item 3	Zero removal credit was assumed for saturated pools.	In the cases analyzed in which the pool was saturated at the time of core meltdown, the containment had failed previously and it was assumed the suppression was subsequently ineffective. These cases all involved the Mark I design.
Item 4	Should be Chapter 8 not Chapter 7.	
Item 5	Should be Chapter 5 not Chapter 7.	
Item 7	Considered under Items 1 and 2.	

Reviewer	Comment	Resolution
Item 8(a)	Scrubbing in suppression pool should be noted.	Scrubbing was credited in the analysis. The effect will be discussed in Chapter 7.
Item 8(b)	Confusion in Table 8.2 about TW and TC sequences.	Typographical errors are being corrected. In addition, values of temperatures in the table have been revised to represent steady temperatures in the drywell for the BWR and in the containment for the PWR.
Item 8(c)	Most Mark III containments have an SGTS.	Text is being corrected.
Category B Item 2	Gieseke/	
Item 4	Inconsistency between DFs used in Chapter 7 and results in Appendix E.	Discussion is being added to Chapter 7 on why DFs were treated parametrically The assumed failure of the torns is the primary reason for the selection of small DFs.
Item 6(a)	Since form of iodine is likely to be CsI or HI, larger partition coefficients should have been calculated using Eq. E-9.	Disagree. CsI is likely to be transported as an aerosol and Eq. E-9 is not applicable.
Item 6(b)	Solubility of CsI should be accounted for in scrubbing of CsI particles.	Disagree. Solubility is only relevant if particle contacts water. Even if a liquid film forms on the particle, it does not matter whether the CsI is in-solution in the film.

Reviewer	Comment	Resolution

Attachment

Pg 1 I.
1st Comment

CsI or HI dominant form, but some I_2 in some situations.

As discussed in Chapters 4 and 5, the results of investigations by ORNL and Sandia staff indicated that CsI would be the predominant form expected to be released to the drywell for most accident sequences. However, the data are inadequate to preclude the possibility of release as elemental iodine.

well clarify

2nd Comment

Iodine chemical form does not have a major influence on consequences.

Changes are required in the report to clarify the meaining and reasons for the apparent insensitivity of the calculated results to the chemical form of the iodine. The mechanisms controlling the removal of aerosols and elemental iodine in bubbles passing through a suppression pool are substantially different. In the investigation of the BWR sequences, DF's were treated parametrically to examine the potential importance of retention in the pool. This approach was taken because an ~~experimentally verified model~~ for treating removal in the suppression pool was not available. Recognizing that the DF of 100 used in the sequences analyzed with a subcooled pool could be low, an analysis of these sequences with a DF of 1000 will be included. For the two cases analyzed in which the pool is boiling, the containment is calculated to fail in the suppression pool area prior to fuel melting. Not only is there great uncertainty regarding the amount of retention for particulate and elemental iodine in a boiling pool but in these cases there is some question as to wheter there would be water in the pool.

← rewrite

Reviewer	Comment	Resolution

3rd Comment Consequences have not been overestimated by order of magnitude.

As discussed above in some accident sequences, the suppression pool would not only be boiling at the time of fission product release, but, in addition, failure in the suppression pool structure would precede fuel melting.

4th Comment Additional comments.

In Appendix A, discussion of accident sequences was divided into two sections: A.2.1. Conditions in the Reactor Coolant System and Drywell. A.2.2. Containment Conditions. The intent was to consider the flowpath and conditions in the reactor coolant system and drywell generically in A.2.1 and then in A.2.2 to consider the flowpath and conditions in the suppression pool and the vapor space of the wetwell for the three different principal design variations in the containment. This apparently led to confusion as to why on pg. A-13 the discussion stops in the drywell. The discussion will be made clearer in the text.

Pg 1.II
1st Comment Behavior of iodine and iodide in the suppression pool is similar under sever accident conditions.

The commenter appears to feel from his discussion that cesium iodide would transport through the suppression pool as a vapor. Analyses in Chapter 6 indicate that CsI would condense on particles and would subsequently transport through the pool as a suspended serosol in the rising bubbles. The CsI partition coefficient is not relevant to this calculation. The data on aerosol removal in the suppression pool were inadequate to predict the DF and the DF was therefore treated parametrically. We agree that ~~parametrically~~ the mechanisms for removal of elemental iodine and particulate iodine differ substantially and that the DFs could be greatly different. This will be clarified in the report.

Reviewer	Comment	Resolution

Pg 2.II 2nd Comment

Assumed decontamination factors.

As stated earlier, in those sequences where a DF of 1 was assumed, the containment had failed in the region of the wetwell prior to core meltdown. Thus, there are two questions involved: the effectiveness of a boiling pool in the removal of fission products and whether the release will be into water because of the location of the failure in the wetwell region.

The time of containment failure predicted in the AEγ sequence is very sensitive to the amount of oxidation of zirconium occurring in the period before pressure vessel failure. In the analysis presented, virtually all of the zirconium was reacted following slumping into the lower plenum. The models describing behavior in the lower plenum are very uncertain. For analyses with less reaction of the zirconium, the failure time is extended.

The ability of the pumps in the newer reactor coolant system designs to pump saturated water is certainly a positive feature. Accident sequences of this type were not analyzed in this report for the newer plant designs.

Since an empirically supported model was not available to calculate the amount of decontamination in the pool, the DFs were treated parametrically. The final report will include results with DF = 1000 for the cases where the suppression pool is subcooled.

Pg 2.II Comment 3

Modeling and assumptions used for dominant severe accident sequences.

The transport pathway in the analysis is as described in the comment. As discussed previously, the consideration of the flowpath on pg. A-13 ended with the drywell because the topic of Section A.2.1 is "Conditions in the Reactor Coolant System and Drywell". The following Section A.2.2 describes conditions in the transport pathway through the suppression pool to the overlying vapor space. This will be clarified in the report.

Reviewer	Comment	Resolution
Pg 2.III	GE Comments on Impact on Risk.	

The authors are not as optimistic as the commenter regarding the decontamination available from the pool for some very low probability accident sequences. Results for in-vessel behavior that will be described in Chapter 6 indicate that for some important sequences there could be significant retention of aerosols in the reactor coolant system. Report conclusions are also being modified in this regard.

Reviewer	Comment	Resolution
Campbell Pg1 Comment on Chapter 2	There should be a table like Table 2.1 that gives the amounts of fission products in grams as well as curies.	This will be provided in Chapter 4.
Pg1 last ¶	Could a common classification of accidents be used in the Regulatory Report and Technical Bases Report?	Disagree. The classifications in the two reports serve different purposes. In the Regulatory Report, the groupings relate to off-site release characteristics. In the Technical Bases Report, the classifications relate to in-plant behavior.
Pg2 ¶1	Is the water taken into account that depressurizes or is pumped into the containment? Won't this water interact with fission products?	The location and form of water in the containment building is of major importance regarding the potential for interaction with airborne fission products. Water in the sump is treated in the analyses as a horizontal surface and would be expected to be similar to other surfaces in the containment which would be covered with a water film. Transport of fission products to walls with condensing steam and the growth of droplets by condensation in the atmosphere are the important processes affecting removal in the containment. These are treated by the methods in Chapter 7. More discussion is being provided in Chapter 7 regarding these processes and modeling uncertainties.
Pg2 ¶3		Gieseke/Kuhlman
Pg2 ¶4	Why not analyze the TMI accident rather than a TMI-like accident and use it as a check of the methods?	There is considerable information to be gained by the analysis of TMI. It is not, however, a good basis for verifying codes developed for core meltdown accidents. This does not mean that the orientation of these models has been improper. Because of conditions in the primary system during the period of release from the fuel (water in the pressurizer and closed block valve in the critical time period), the soluble fission products were dissolved in water before reaching the containment in the TMI accident. The analysis of behavior

Reviewer	Comment	Resolution
		in containment focuses heavily, therefore, on partition coefficients under the expected conditions. Since the release to the containment atmosphere would be very small regardless of whether the iodine were initially in the elemental or iodide form, the consequences of this type of accident do not have the potential magnitude as those in which the pathway to the containment is dry. Hence, emphasis has been placed in model development on accidents of this type. Since the TMI accident is a unique opportunity to obtain information about a real accident, further analysis should certainly be undertaken of the accident. Attempts by the authors to analyze the release and transport behavior of fission products within the primary system and containment of TMI have been frustrated by inadequate thermal-hydraulic data.

Reviewer	Comment	Resolution
Castleman Pg3. 1st full ¶ last 2 sentences		Gieseke/Kuhlman

Reviewer	Comment	Resolution
Devell Item 3	Additional knowledge tends to decrease release figures and probabilities for risk dominant sequences.	Partially agree. Uncertainties in release fractions are skewed toward lower values. We expect additional research to lead to reductions in release estimates. It should not be assumed, however, that this will necessarily be true. No changes are planned in text.

Reviewer	Comments	Resolution
Develler	1. Would like table summarizing differences with WASH-1400 results.	Integrated analysis was beyond scope of the report.
	2. Should have case with delayed failure.	TMLB' without failure was intended to show typical airborne conditions for case of delayed failure. Explanation is added to Chapter 7.
	3. Report did not look at long enough timescale to see effect of chemical form.	Disagree. Report looked at characteristic timescales. However, conclusions of draft report are misleading regarding the apparent insensitivity of the results to chemical form. Changes are made to Chapter 7. Chapter seven
	4. How do Section 5.3 results affect WASH-1400 predictions?	In general, WASH-1400 sequences involve dry pathways to containment and Section 5.3 results do not directly impact. Suppression pool behavior is an exception. Discussion is being added to Chapter 7 about suppression pool DFs.
	5. Two potential sources of I_2 are not treated.	
	6. (Chapter 7)	
	7. Chapter 5	
	8. Chapter 6	
	9.1 Predicted high release of Te in Chapter 4 is not considered in later chapters.	Assuming Te is primarily transported through the primary system as particulate, generic particulate analyses in Chapters 6 and 7 are applicable.
	9.2 Chapter 4.	

Reviewer	Comment	Resolution
Gehl	NONE.	

Reviewer	Comment	Resolution
Hilliard Items 3,5		Kuhlman/Gieseke
Specific Comments Pg2 Last ¶	Conservative assumptions have probably been made about thermal-hydraulic conditions in primary system.	Mostly disagree. The attempt was to be as realistic as possible. There are some variations of sequences, such as partial ECC operation where there could be water injected into the pathway. There is significant uncertainty about the presence of water in the pathway in other sequences such as TMLB'.
Pg3 1st ¶	An attempt should be made to identify conditions in which an oxidizing atmosphere could exist at the time of release.	In Appendix A, an evaluation was made of accident phases for a variety of meltdown sequences to explore this possibility. Although there were some possible conditions of this type identified, they appeared to be comparatively unlikely.
Pg 4 1st Item	CORRAL should be run against CSE tests.	Comparison with CSE experiments is being undertaken as part of the CORRAL-3 verification process.
Chapter 7 Item 2		Gieseke/Kuhlman
Item 3	More detail should be provided regarding containment failure modes.	In Chapter 3, greater discussion is being provided regarding the importance of containment failure modes and the related uncertainties.
Items 4,5		Gieseke/Kuhlman

Reviewer	Comment	Resolution
Levenson	¶3. Codes and models from WASH-1400 were used again with same results.	CORRAL-2 (similar to the CORRAL code used in WASH-1400) was the only code used in both reports. TRAP, HAARM-3, QUICK, and NAUA calculations were performed to examine the effects of mechanisms treated empirically in CORRAL-2.

Reviewer	Comment	Resolution
Levine 2(i)	Consistent analysis should be made between primary system and containment transport.	Agree. This type of analysis was not possible within the scope of this review. Containment transport behavior could definitely be affected by conditioning of the source as it passes through the primary system. Capabilities for performing this type of consistent analysis are under development.
2(ii) ¶2	Conclusions about the conservatism in WASH-1400 risk dominant sequences are premature.	Agree. The conclusion is being rewritten with better recognition of the magnitude of uncertainties in the analyses.
3		Kuhlman
Important Technical Points Item 2	The difficulties in defining flowpaths and failure modes are not stated.	In Chapter 3, discussion is being added regarding uncertainties in predicting failure modes and release pathways.

Reviewer	Comment	Resolution
Malinauskas Section 2,3	Editorial comments.	Revisions have been made to address a number of editorial comments.
	The authors disagree with the following comments.	
Pg 2.2	Radioactivity cleanup system is not the principal source of radioactive material released to the environment during normal operation. Activation products are major radioactive species in coolant.	Disagree. Reference PWR-GALE report. Since activation products do not present the magnitude of hazard as fission products in an accident, it was not considered necessary to discuss them in Chapter 2.
Pgs 2.3, 2.6	The comment "(early fatalities) would at worst be expected to occur. .within a few miles of the plant" indicates bias. "Iodine-131... are potential major contri butors to the does...from the passing cloud...in severe accidents".	Disagree. It is helpful to the reader to understand the significance of different radionuclides to health effects according to the risk analyses that have been performed to date. Clarification of the source of these results will be provided, however, with qualifications regarding uncertainties in release magnitude.
Pg 2.[illegible]	Engineered safety features were not effective in retaining chemically reactive fission products at TMI.	Disagree. Partial performance of the ECC system was effective in providing water which made up for loss through the relief valve, keeping the pressurizer full and blocking the release of iodine and cesium to the containment atmosphere. Containment building was very effective in retaining fission products that did become airborne.

Reviewer	Comment	Resolution
Postma Pg 1 ¶3		Gieseke/Kuhlman
Pgs 7&8		Gieseke/Kuhlman

Reviewer	Comment	Resolution
Ritzman Item 3	Report should identify containment failure mode analysis and deposition in leak paths as areas of uncertainty requiring further work.	Agreed. This suggestion being incorporated into Chapter 7.
6,7		Gieseke/Kuhlman

Reviewer	Comment	Resolution
Scherer Item 3	The time dependent aspects of fission produ t transport relative t the accident sequence events requires further attention.	Agreed. However, within the scope of the review, there is very little additional that can be done.
Items 4,5		Gieseke/Kuhlman

Reviewer	Comment	Resolution
Schweitzer	NONE.	

Reviewer	Comment	Resolution
Thompson Pg2 Item 2 ¶2		Gieseke/Kuhlman

Reviewer	Comment	Resolution
Tong	Are the statements regarding risk-dominant accident sequences plant specific or generic? Different sequences would be expected to dominate the risk of different plant designs.	The statements are based upon the reanalysis of the WASH-1400 plant sequences. This will be clarified in Chapter 3. A common characteristic of sequences predicted to dominate the risk in past studies, however, is that they involve failure of the containment near the time of core melting and reduced effectiveness of containment safety features in trapping fission products.

Reviewer	Comment	Resolution
Torgerson	NONE.	

Reviewer	Comment	Resolution
Vogel		
Pg2 ¶1	Over-simplified models in WASH-1400 used without modification or qualification.	Disagree. CORRAL was the only code used in this study from WASH-1400. Comparisons were made with a number of mechanistic aerosol codes: HAARM-3, QUICK, and NAUA. The treatment of core meltdown phenomena (MARCH code), primary system transport (TRAP), fission product release, and aerosol production represent significant extensions beyond WASH-1400 methodology. Further, these analyses were supported by a number of sensitivity studies.
Pg2 ¶2	Inadequate consideration is given to containment failure modes and deposition along leak paths	Disagree. The conditions leading to containment failure, location of failure and mode of failure have a major effect on predicted accident consequences. This review has given limited attention to accidents in which the containment does not fail because the consequences associated with these accidents would be minimal. The consequences of these accidents could be reduced by the plugging of leak paths. The characterization of a major breach in containment is so far beyond the state of technology that analysis of deposition along the leak path would be very speculative.
Pg2 ¶3	Treatment of aerosol processes is disjointed.	Agree. An integrated analysis was beyond the scope of the program. Text is being added to Chapters 6 and 7 to better explain the implications of treating each phase separately.
Specific Comments Pg1&2	WASH-1400 conservatisms were inadequately considered.	Disagree. The intent of this report was to review the state-of-the-art for predicting fission product behavior. Although we feel that in many respects this review actually extended the state-of-the-art, there were clearly limits to what should be undertaken in the report. Furtherfore, many of the so-called conservatisms claimed for WASH-1400 should actually

Reviewer	Comment	Resolution
		be identified as possible conservatisms. In many cases, the presumed conservative assumption is made because there is no technical basis for selecting a less conservative model.
	Lack of FP retention in primary system.	Although the TRAP-MELT and QUICK analyses performed for the primary system are unverified, they represent the best option available for analysis. Results of sensitivity studies were included to provide perspective on the effects of uncertainties in therma-hydraulic and deposition models.
	No FP deposition in containment leak paths.	As discussed above, considering the state of ability to predict containment failure models assumptions about the potential for deposition in leak paths would be pure speculation.
	No FP trapping in saturated water pools.	The potential for trapping in saturated pools is discussed in Appendix E. However, data to support the verification of such a model is inadequate.
	No FP retention by auxiliary buildings.	Potential was analyzed for event V and found to be small.
	Total release of volatile FP's from fuel involved high bias of small experiments.	Best available data were used. The assumption that these results are highly biased is speculative.
	Uninhibited fuel oxidation and Pu released in steam explosions not addressed.	As discussed in Appendix A, the oxidation release term was not investigated because it is the opinion of the authors that experimental evidence since WASH-1400 has downgraded the importance of steam explosions.

Reviewer	Comment	Resolution
	Iodine chemical form only partially addressed.	Most of this report is directed at this issue.
	Incomplete aerosol behavior modelling.	The best available models were used to evaluate aerosol behavior.
	No containment failure mode analysis. Puff release assumed.	The performance of structural analyses was considered to be beyond the scope of this report. At least a rudimentary consideration was given to containment failure modes for each design analyzed. No technical basis exists for the selection of a failure size small enough to prevent rapid depressurization following containment failure.
Last comment Pg2	TRAP-MELT is deficient in many important areas. The well-mixed assumption is wrong and greatly affects results.	It is not the intent of the authors to claim high accuracy for TRAP-MELT. More comments are being added to Chapter 6 regarding the assumptions in TRAP. TRAP was, however, the best tool available for evaluating the potential for primary system deposition. We are aware of no analyses of primary system transport in LWR accidents that show that the well mixed assumption greatly affects the results.
Pg3. 2nd full comment	Further thought should be given to the sequencing of release phases.	Agree. However, this is beyond the scope of this study. The statement that the early release would involve a lot more water present in the primary system might be true for some sequences but is not an accurate generalization.
Pg3. Last comment	Considering all of the structural material around the reactor cavity it is inappropriate to assume 50% release. Probability of sequences should be discussed.	Structural surfaces are taken into account in the analyses. More detailed analyses of behavior in the flowpath from the cavity might indicate greater potential for retention but are beyond the scope of this effort. Additional perspective is being provided in Chapter 3 regarding the probabilities of core melt accidents.
Pg4. 1st comment	Auxiliary building filter systems--primary system retention in V sequence.	Disagree. There is a range of thermal hydraulic conditions that could happen in event V depending on the mode of check valve failure, location of low pressure

Reviewer	Comment	Resolution
		system failure and details of auxiliary building design. In some cases, the auxiliary building filter system would have some effectiveness. It should be recognized that the well mixed assumption used to describe retention in the auxiliary building could be non-conservative and that the consequences could be higher as well as lower.
Pg4. 2nd comment	TRAP-MELT code lacks benchmarking and should not have been used.	Disagree. TRAP-MELT has indeed not been verified against integral experiments to date. However, the intent of this review was to use the best information available. The TRAP code is the best model available for examining fission product transport mechanisms in the primary system. Not only have the deficiencies in TRAP been identified in the report, but the results of uncertainty analyses and sensitivity studies have been provided.
Pg4. 3rd comment	Chapter 2 needs rewriting.	Partially agree. Chapters 2 and 3 are intended to provide introductory material to a non-technical reader in order that he can achieve some context for results presented in Chapter 1. A number of changes are being made in Chapter 2 to clarify the discussion. Adding a discussion of fission in plutonium isotopes is not necessary to meet the objectives of this chapter. Yes; genetic effects can be carried forward into future generations. A footnote has been added to Table 2.1 describing the basis of the inventories.
Pg4. 4th comment	Probability of containment failure in TMLB' should not be considered high.	Disagree. The arguments provided relate to the probability of occurence of the TMLB' sequence not to the likelihood of containment failure given TMLB'.

Reviewer	Comment	Resolution
Pg4. Comment 5	Chapter 4.	
Pg4. Last comment	Chapter 4.	
Pg8. 1st comment	(a) Uncertainties of $\pm 100°F$ shown in Tables B-2 and B-3 are not correct.	Agreed. The notation was intended to show the interval width, not the uncertainty. It is being changed in order not to be misleading.
	(b) Existing models do not predict the failure of the grid plate or core barrel.	The sentence is being modified to remove any implication that the codes model the support plate failure modes mechanistically.
	(c) Kress should answer.	
	Chapters 6 and 7 comments.	Gieseke/Kuhlman
Pg13. Last comment	Containment cannot be considered "dry" because of water from system blowdown, possibly appearing as rain.	Text is being added in Chapter 7 to clarify this point. The phenomena of internal condensation and enhanced gravitational settling are precisely the mechanisms investigated in the NAUA-4 analysis. In addition, it should be recognized that the CORRAL code treats these phenomena implicitly since these effects were observed in the CSE tests.

Reviewer	Comment	Resolution
Wallace	NONE.	

Reviewer	Comment	Resolution
Zumwalt Comment 2	Assumed form of iodine does not appear to have a major influence on complete core melt with containment failure but would influence lower risk accidents such as partial core melting without containment failure.	It is important to recognize the magnitude of uncertainty in the predictive capability of existing methods. Although very little dependence on chemical form was observed in the comparisons in Chapter 7, it is not appropriate to conclude that chemical form is not important. Transport behavior in the primary system and subsequent effects of conditioning of the radionuclide source by the primary system on containment behavior could be particularly important effects which at present are inadequately understood. Chapter 6 and Chapter 7 and report conclusions are being modified to better recognize these uncertainties.