March 30, 1981

Dr. Mel Silberberg, Chief
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Technology Branch
U. S. Nuclear Regulatory Commission
Washington, D.C. 20555

Dear Dr. Silberberg:

The general comments below pertain to the draft report, "Technical Bases for Estimating Fission Product Behavior during LWR Accidents," (NUREG-0772 draft, March 6, 1981). In addition to these general comments you will find attached a group of more specific comments. The comments amplify and emphasize the verbal comments which were offered at the "Peer Review" in Washington on March 17 and 18. It is, of course, clear that a great deal of work has been expended in preparing the document. The intensive efforts of NRC and its contractors should be acknowledged.

The general comments are as follows.

- There is insufficient time before early April (the target date for completing the report) to turn out a good defensible document. The completion should be delayed.

- The abstract does not adequately reflect the technical contents of the document. It should be rewritten. Furthermore the preliminary nature of the present study needs to be plainly stated.

- The original issue raised was that there was a significant body of empirical information from accidents and destructive tests which suggested that the iodine source term was overpredicting the potential iodine release. The current draft report ignores this information. The draft report therefore does not address the original question.

- We feel that the evidence is reasonable in supporting the contention that CsI is the form for much of the released iodine in an irradiated fuel rod. However, even if the evidence is not reasonable, it is certainly clear that elemental (gaseous) iodine is not the predominant form of released iodine; and therefore considerations based on this hypothesis should be modified.

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- Also related to the preceding point is that codes are being used which are over-simplifications of the true situation. It was pointed out that the inplant-consequences model developed for WASH-1400 incorporated several significant conservatisms. It appears that this model was basically used in this report without modification or qualification.

- A significant limitation of the study results from the lack of containment failure mode analyses. Fission product deposition along leak paths is discussed in only a qualitative fashion and then ignored in the various release calculations.

- The treatment of aerosol processes is disjointed. 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. The hypothetical output from the containment is not evaluated for size effects on dispersal.

We would like to repeat our offer made at the March 17, 18 meeting that groups from industry work with NRC selected groups to further develop the technical basis for estimating fission product behavior. This could be done on a chapter by chapter basis. EPRI would, of course, be willing to participate significantly in this effort. We feel that the preparation of this document represents an opportunity to clarify the risks associated with nuclear power and as such deserves the significant efforts of talented and appropriately trained people.

Sincerely yours,

Richard C. Vogel

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SPECIFIC COMMENTS

on Draft NUREG-0772

Reference	Comment
Abstract	As pointed out at the peer review meeting we do not feel that the abstract accurately reflects the contents of the report. We understand that it is being rewritten
Abstract & Chapter 1, Summary & Conclusions	The report concludes (Abstract, page iii) that the best estimate release fraction for iodine is 0.1 to 0.5. This range compares to WASH-1400 estimates of 0.2 to 0.7 for PWR and 0.1 to 0.9 for BWR. This small reduction is unsurprising since the same models and computer programs have been used with little modification. Certain areas of conservatism in WASH-1400 were pointed out by Ian Wall before the NRC on November 18 and are repeated below followed by the NUREG-0772 response.

WASH-1400 CONSERVATISMS IMPACTING CONSEQUENCES FOR DOMINANT ACCIDENT SEQUENCES

Lack of FP retention in primary system
Parametric calculations with TRIP-MELT. No aerosol agglomeration. Thermal-hydraulic data unclear.

No FP deposition in containment leak passages
Noted briefly & dismissed as impractical.

No FP trapping in saturated water pools
PWR quench tank not analyzed. BWR suppression pool addressed parametrically.

No FP retention by auxiliary buildings
Dismissed as unimportant.

Total release of "volatile" FP's from the fuel
Used expression based upon small experiments - "high bias." More conservative than WASH-1400.

WASH-1400 CONSERVATISMS IMPACTING CONSEQUENCES FOR DOMINANT ACCIDENT SEQUENCES (Cont.)

	Uninhibited fuel oxidation and Ru release in steam explosions . Not addressed.
	Iodine assumed I_2 rather than CsI Partially addressed.
	Incomplete aerosol behavior modeling Well-mixed single volume model for containment. Steam condensation partially addressed. Parametric calculations.
	Puff discharges upon containment over-pressure failure No containment failure mode analysis. Puff release assumed.
Chapter 1, pg. 5, lines 13 and continuing	Section 1.1.3 comments on the iodine risk perspective. The point of this section is that other fission products are equal to or more important contributors to risk than iodine. This assessment of relative importance is apparently discussed in the NRC staff report "Regulatory Impact of Nuclear Reactor Accident Source Term Assumptions." This document should be made available for peer review and comment as has been done with the current document now under review since the implication is that the subject of non-iodine fission product source terms is equally important.
Chapter 1 pg. 7, lines 9-16	The lack of a systematic analysis of fission product transport from the fuel to the environment is a serious omission and should be completed before the report is published. A parametric analysis is not an adequate substitution.
Chapter 1, pg. 11, lines 7-16	The TRAP-MELT code is deficient in many areas important to the assessment, eg. the use of log-normal distribution for aerosols (instead of a bi-modal one)

The assumption in paragraph 2 that particulate and/or gaseous species are assumed to be well mixed within each control volumn is wrong and greatly affects the results.

Chapter 1, pg. 12, lines 25 and continuing

An important observation in the literature was overlooked -- iodine was associated with Cs as CsI crystals in deposits of f.p. on the cladding i.d. of reactor irradiated fuel. Therefore there is further evidence for CsI in fuel rods. (Cubicciotti and Saneki, J., Nuclear Materials Vol 78, 96 (1978)

Also in the experiments of Lorenz et al they used a quartz sleeve. Thus the chemical form of released CsI was probably converted in part to gaseous iodine by reaction and was not released as gaseous I_2 from the fuel rod.

Chapter 1, pg. 13, lines 27-34

The assumptions of the early release of cesium and iodine prior to the bulk of the aerosol implies that these fission products also will agglomerate and settle before a significant challenge to the RCB occurs due to non-condensible gases. Also, an adequate treatment of this early release (prior to core melt) in the "dry" accident scenario would show a lot more water present in the primary system during such early release. We feel that further thought should be given to the sequencing of the steps hypothesized.

Chapter 1, pg. 14, lines 13-16

We disagree with the statement that TeO_2 is the "sole significant Tellurium species" in steam. Te_2 vapor and H_2Te are more important in our opinion.

Chapter 1, pg. 14, lines 29-41

Partition coefficients for aqueous iodine are sensitive to the presence of other dissolved species, see GE Report NEDO-21159-2 by T.R. Marrero. Also on line 32 there is a serious typo. "Iodine" should be "iodide."

Chapter 1, pg. 16, lines 8-14

In consideration of all of the structural material in and around the reactor cavity, we do not think that it is appropriate to assume that 50% of the material released into the containment will escape from it. In addition to place this statement in perspective, the probability of the accident described, TMLB' with 16 continuous hours of absolute on/off-site power loss, must be brought into this discussion to be technically useful.

Chapter 1, pg. 17, lines 13-19	Auxiliary Building Filter Systems - significant amounts of fission product transport through a relatively small line in the check valve V event without plateout, scrubbing, deposition, line plugging, supersonic effects (i.e. choke flow etc.) doesn't seem reasonable. There is a 10" line outside containment; however, if the check valves failed, they are likely to fail partly open, (i.e. crack) so the effective leak area might be quite small.
Chapter 1, pg. 20, lines 1-4	The TRAP-MELT code lacks benchmarking. This is a major deficiency of the study. We realize that this deficiency is noted. However, to note the deficiency does not remove it since the code is used.
Chapter 2	This chapter needs rewriting. For example, at EOC nearly one-half of all fissions occur in Pu (see Section 2.1). Does radiation really cause genetic effects which can be carried forward into future generations (see Page 2.6)? Does the radioactive inventory, (Table 2.1) mean equilibrium core EOL?
Chapter 3, pg. 3.7, lines 30-40	The statement that during a TMLB' accident (transient→scram→loss of on/off-site power →loss of secondary heat sink→ primary system blowdown through safety valves), "...the likelihood of containment failure by overpressurization in this sequence would be very high..." seems inappropriate. The containment cannot be even seriously threatened unless the loss of <u>all</u> power extends for roughly 10 or more hours <u>and</u> absolutely no attempt is successful in providing containment cooler or spray operation during this period.
Chapter 3, pg. 4.3, line 22	This sentence gives the erroneous impression that in an intact fuel rod cesium will form a uranate compound in preference to CsI. The reverse is true. The Cs reacts preferentially with I and then with UO_2.
Chapter 3, pg. 4.3, line 39	This is no reason to believe that ZrI_4 forms. CsI is much more stable -- same applies to pg. 4.4., line 21.

Chapter 4, pg. 4.4, line 31

The suggestion that CsI dissociates to form iodine at a pressure great enough to condense as liquid I_2 is impossible. The example presents the false impression that such a reaction could occur.

Chapter 4, pg. 4.11 and Table 4.3 pg. 4.12

The fission product release data are a vital ingredient in this report. There seems to be general disarray in this area.

For example, equal weights are being placed on experiments involving very low levels and higher levels of iodine in flowing gas (Table 4.3 pg. 4.12 column 5 should be labeled micrograms). Of the 17 experiments on iodine release presented in Table 4.3 as part of the data base, 8 involved less than 20 micrograms of iodine each.

Some of these experiments involved flowing gas atmospheres for as long as 20 hours. If one assumes the following as a typical experiment

2.54 cm diameter reaction tube
1 atmosphere pressure for steam
100 cm/min flow rate.
1 hour duration for experiment
10 micrograms of iodine
900 °C

it can be calculated that the O_2 in the steam must be less than 0.1 ppm by weight to avoid converting all of the CsI to iodine (CsI very likely will have been vaporized from the fuel sample and condensed on the apparatus wall). The use of sufficiently pure gas is unlikely. Therefore at least half of the data should be discarded. Indeed if one considers the data of Table 4.3 involving more than 100 micrograms of iodine the percent released as I_2 is always less than 1%. For iodine levels less than 100 micrograms the iodine releases as I_2 are always greater than 1% rising to as high as 88%.

Chapter 4, pg. 4.11 - 4.21

The report lists five major fission product release mechanisms and ignores three major mechanisms applicable to degraded core accident condition. These mechanisms are:

1. Burst release (in report)
2. Fuel-clad gap diffusional release (in report)

3. Grain boundary release - a high temperature-caused interlinking of FP bubbles on grain boundaries to form highways to pellet surfaces for enhanced release of the FP's in the bubbles (in report)

4. The above pathways have been mechanically formed; and, as a result, the solid-state diffusion pathways for the FP's in the fuel grain matrices are now only 10's um long versus the pre-step (3) 1000's um long. Temperature-enhanced fission product release rates will remain at increasingly higher levels (not in report). (Item 5 of p. 4.21 misinterprets this gross long-term impact increasing fission product release).

5. Thermal-stress induced fuel fragmentation greatly increases surface area to volume ratios and directly exposes many grain surfaces to low-restriction gas diffusion pathways (not in report).

6. Diffusion from the UO_2 grains (in report). This mechanism now is of great enhancement importance because of steps 4 & 5.

7. Loss of fuel pellet physical stability due to thermal/chemical decomposition in the degraded core accident hot, steamy and hydrogen bearing environment - the pellet (or its pieces can weaken to the point of crumbling, leading to large effective increases in solid state FP diffusion rates (not in report).

8. Finally, core melt (in report), which has much less significance now due to the integral impact of steps 1 to 7.

Chapter 4, pg. 4.16 (Section 4.2.2)
(Note: pp. 4.14, 4.15, and 4.16 are misnumbered and should read respectively 4.15, 4.16, and 4.14.)

The authors have selected three sets of experiments to provide a data base for f.p. release. We have serious questions about the applicability of each of these sets of data to real systems. We also do not feel that the data from accidents and large scale tests should be ignored. The tests of Lorenz had quartz liners and possibly traces of oxygen (both of which would shift the volatile species toward I_2) which are not realistic of reactor situations. The tests of Parker were tracer level and had possibilities of moisture and O_2 contamin-

ation. This makes application of these experimental results to real systems questionable. The tests of Albrecht with synthetic additions of f.p., raise questions about the applicability of these results to real systems.

Chapter 4, pg. 4.19, Figure 4.1

The data used to generate fission product release rates are suspect. For example Figure 4.1 reports sets of "HT runs" for 0.4 min, 3 min, 7 min, and 10 min. The released rates expressed as read from Fig. 4.1 as fraction released per minute at 1200°C should be roughly the same. However, they differ by more than three orders of magnitude. Obviously a better data base is needed.

Chapter 4, pg. 4.21

In summary the fission product release rate expressions presented in Section 4.3 and Appendix B are taken from results of small scale experiments, which cause a high bias. The surface to volume ratio is quite different from a true core situation and the experiments are frequently subject to invalidation due to impurities in the gas atmosphere. The expressions proposed and the fractional release rates for fuel, clad, and structure (adapted from SASCHA air data) are functions of temperature only. Thus, they are quite empirical in nature and should not be applied to conditions beyond those covered in the referenced experiments. For example, using these expressions to define release rates at system total pressures of 100-150 bar may result in considerable over-estimates since the experimental work was done at system pressures of 1-2 bar. These particular expressions also contain no surface area or superficial gas flow rate terms and hence are strictly applicable only to the range of values used for these parameters in the experiments. A more complete discussion of the limitations involved in the use of the release rate expressions should be incorporated in Section 4 and also clearly acknowledged in the report conclusions.

Chapter 4, pg. 4.21 last paragraph

Before the core hits the bottom of the pressure vessel it would seem reasonable that most of each of the volatile fission products would be vaporized.

Chapter 4, pg. 4.24 to 4.27

The accidents selected, AB and S_2C, are misrepresented regarding accuracy of analysis as they affect temperature-dependent fission product release for the following reasons.

a) MARCH code is not a verified code, yet temp. uncertainties at or near core melt are stated as $\pm$ 100°F (Tables B.2 and B.3). A realistic evaluation of these uncertainties would increase them greatly.

b) p. 4.24 - last paragraph in 4.3.1 - Existing models do not "predict" such a failure mode, the MARCH code assumes that mode.

c) An extremely crude method of predicting core regional fission product release as a function of temperature has been used based on MARCH results. The net effect is to impose high temperature rise rates for AB (230°C/min) and S_2C (80°C/min) for all temperatures > 1000°C. Such rates are more appropriate for Zircaloy-oxidation-enhanced conditions $\lesssim$1500°C and are ~10 times too high for $\lesssim$1500°C - reducing time at temperature effects dramatically.

Chapter 4, pg. 4.26, 2nd paragraph

2 μm aerosol particles with small standard deviations and a log normal distribution is a bad assumption and not consistent with other experimental work.

Chapter 4, pg. 4.27, 2nd paragraph

The melting point of UO_2 is depressed less than 200°C by the addition of ZrO_2 and only when one has greater than 30 m/o ZrO_2. Also Figure 4.5 on pg. 4.29 does not seem consistent with a similar phase diagram in NUREG-0205.

Chapter 5, general

Water leaching of fission products from the fuel should be considered as a potential mechanism for fission product release. Such a process will lead to redistribution of the heat sources.

Chapter 5, pg 5.1, 2nd paragraph

Considering the statement starting with "once released from the fuel..." it appears that an error in assumptions has been made. It is assumed that the noble gases are swept out by steam flow. However the rapid blowdown of an accident is over before gross fuel melting. If not there is sufficient heat transfer available to cool the core.

Chapter 5, pg. 5.7, Section 5.2.2.2

It appears to us that incorrect thermodynamic data are being used for tellurium compounds. The stable gas species is not TeO_2 when the system contains hydrogen. The stable gas species are Te_2 and H_2Te. TeO_2 is important only when oxygen is present. This confusion appears also on pg. 14, (Chapter 1).

Chapter 5, pg. 5.10, lines 7-17

The reaction of CsOH vapor with stainless steel should yield Cs vapor which would react with I and there would be no net increase of iodine pressures. The reaction of CsOH with stainless steel will tie up the Cs (as Cs_2CrO_4) only when the oxygen pressure is relatively high.

Chapter 5, pg. 5.1 and continuing (Section 5.2)

In this section (and in Appendix C.1 to C.4) the authors consider the equilibrium chemistry of fission product species in the vapor. They take a very unrealistic view of the likely situation in that they don't evaluate the interaction of the vapor with condensed phases. It does not seem realistic to assume that all parts of the system will be above the condensation temperature of species such as CsI or CsOH These temperatures range from about 400 °C to 700 °C (see pg. C.3).

Chapter 5, pg. 5.26 (Section 5.3.9)

This section predicts that the formation of CH_3I is a less serious problem than was once anticipated. This is an important conclusion and it is not reflected in the rest of the report or in the abstract.

Chapter 6, pg. 6.1

The list of processes included in TRAP-MELT differs from the list given in the User's Manual by omission of Brownian agglomeration and addition of gravitational settling. Since the User's Manual is cited, it would be well to call attention to their differences to avoid misunderstanding. The effect is that the Manual describes a code lacking all gravitational effects, while the document at hand describes one lacking all agglomeration effects.

Chapter 6, pg. 6.2, 1st paragraph of Section 6.2

After 1st sentence reference could be made to Fig. 7.8 as an example, namely that increases in released mass do not increase leaked mass proportionally (and if high enough may even decrease it) due to increased aerosol agglomeration and subsequent sedimentation.

Chapter 6, pg. 6.2, 2nd paragraph of Section 6.2

Slower diffusion of particulates would mean higher concentrations (than the TRAP-MELT homogeneous model) and thus more agglomeration and removal by sedimentation. (Settling is likely to predominate over plating for particulates)

Chapter 6, pg. 6.4

I_2 vapor is said not to interact significantly with particles. This appears to be based on the high vapor pressure of I_2 at relevant temperatures. Adsorption of I_2 on aerosol surfaces is not controlled by this vapor pressure, however, until the surfaces are completely covered by the adsorbed I_2. Thus, the possibility of significant adsorption of I_2 by particulates is not properly taken into account. Old experiments at AI (L. Baurmash, et al, "Behavior of Iodine in the Presence of Sodium Oxide Aerosols," Proc 11th AEC Air Cleaning Conf., Richland, Wash, 31 Aug-3 Sept. 1970, CONF 700816, 1, 373 (1970)) show that there is indeed very effective scavenging of I_2 vapor by sodium oxide smoke and this could as easily be adsorption as by condensation.

Chapter 6, pg. 6.4, 2nd paragraph of Section 6.3

The acceleration and turbulence associated with a sweeping of all particulates into the containment (at the time of vessel failure) in such a short time as to limit further attenuation would, in itself, likely cause significant agglomeration and subsequent fallout. Thus the statement that: "...any radionuclides suspended in the gaseous phase at the time of pressure vessel failure will be will be swept into the containment with insignificant attenuation" is incorrect. At the very least the particle sizes reaching the containment will be much larger than 1 um.

Chapter 6, pg. 6.5, Section 6.3.1, lines 7-13

Although it is not certain it is suspected that the wrong decay heat curves were probably used, i.e. ANS standard is much lower than licensing curves, and experimental ones even lower!

Chapter 6, pg. 6.10, Section 6.3.2.2.1, 1st paragraph

CsI which is condensed on particles would be subject to aerosol removal processes and thus attenuate.

Chapter 6, p. 6.10

The discussion of "Large Size Particle Source (TMLB'-2)" treats agglomeration, apparently in lieu of including such effects in TRAP-MELT. The claim that agglomeration is not a very effective growth mechanism is not adequately substantiated by this discussion, as the assumed constant coagulation kernel is appropriate only for Brownian agglomeration. At the assumed concentration of 10^3 g/m^3 gravitational agglomeration becomes effective in a relatively short time, and once effective produces very large agglomerates very suddenly. Turbulent agglomeration is also very effective. The AI-NOAA Idaho tests show that very large agglomerates are produced in times no longer than those required for 4 to 8 m/s winds to cross 1m wide burn pans (H. A. Morewitz, et al, "Combustion of Sodium in the Open Atmosphere," paper 25, IAEA/IWGFR SPECIALIST MEETING ON SODIUM FIRES, CADARACHE, FRANCE Nov. 20-24, 1978 (1979)) i.e. less than 1s.

Paragraph 2, p. 6.10 is contradicted by the last paragraph on pg. 7.10.

Chapter 6, pg. 6.17, Section 6.6

The conclusion in the first paragraph, that elemental iodine is able to reach the containment with little attenuation if its pathway thereto remains dry, is weakened by the adsorption of iodine on particles (see previous comment). Moreover, its significance is greatly reduced by the comment in the third paragraph that iodine is mostly CsI with very little I_2. The amount of I_2 is probably small enough to be adsorbed by particulates without exhausting their adsorptive surface, which is sufficient to adsorb about 12% of the iodine according to the source rates of Appendix D of the draft.

Chapter 6, pg. 6.18

The failure of the primary system to retain more than 15% of the particulates (including condensed CsI and adsorbed I_2) is believed to depend on the neglect of particle agglomeration. As shown by recent HAA-4 calculations at AI, Brownian agglomeration is supplemented by gravitational agglomeration in shorter times than those discussed on pg 6.10. Thus analyses that omit agglomeration and fallout in the primary system cannot be conclusive.

Chapter 7, pg. 7.2, Section 7.1.2

The discussion of Aerosol Agglomeration opens with the remark that the process owes its significance to "the relatively large residence times of the radionuclides in the containment." This seems to illuminate the reasons for omission of agglomeration from the TRAP-MELT analyses, where residence times are much shorter. "Relative," however, is a relative term. The relevant time-scale is set by the time for Brownian agglomeration to bring the particles to sizes at which gravitational agglomeration becomes effective, and this time is much smaller for the high concentrations near the source than for the lower concentrations in the containment. As indicated previously, this time was not adequately discussed for the TRAP-MELT analyses.

Chapter 7, pg. 7.4, Section 7.2.1

Gravitational coagulation should be added to the list of aerosol mechanisms modeled by the NAUA code. Since HAARM-3 and QUICK both omit condensation, there is no code used in their analyses that includes all LWR aerosol mechanisms.

We find, indeed, at Section 7.4, pg. 7.12, that analyses of severe core damage sequences used only HAARM-3 and CORRAL-2, of which the latter, being interpolative rather than mechanistic (according to 7.2.2, pg. 7.4) and operating with particles of fixed size, is also not very capable of representing agglomeration. In any event, the experiments on which CORRAL-2 is based had a maximum initial aerosol concentration of 10mg/m^3, and hence cannot be used with confidence at higher concentrations. Even a uniform aerosol concentration of an initial 2 metric ton source term in a large dry PWR containment would be ~ 100 times as concentrated as these experiments

Chapter 7, pg. 7.5, Section 7.2.3

Both HAA-3 and HAARM-3 will produce a source to a secondary containment, so that multi-compartmented analyses can be done.

Chapter 7, pg. 7.12, Section 7.2.4

Potential for retention in the containment is not independent of aerosol behavior in the primary system - specifically the particle size and distribution depends on it. A simple superimposition would not be adequate. A general but quantitative estimate of the primary system effect, if possible, would seem appropriate here.

Chapter 7, pg. 7.26, Figure 7.8

It would be helpful to know the particle or mass concentrations corresponding to the source mass.

Chapter 7, pg. 7.32, Section 7.6, 3rd paragraph

Leakage into the duct, not through the duct is accounted for in the correlation: $\bar{m} = KD^3$. The leakage out of the duct is also a function of its length and tortuosity and is very much less than $\bar{m}$. The correlation has been validated for 24 μm <D< 30 cm. Consequently the leaked masses given in table 7.4, Fig. 7.8, Table 7.5, Figure 7.9, & 7.10, Table 7.6 are quite wrong even for dry containments and are certainly even in greater error for wet containments. Even for failed containments, a substantial fraction of the aerosols in the containment atmosphere will never reach the site boundary because of growth in the leak path to very large sizes (due to agglomeration on the walls of the leak path).

It has been shown that the leak from a 2×10^6 ft. containment at 0.10% per day can be represented by a single straight 1mm capillary which will plug when 10 mg of dry aerosol has entered. Considerably less than 10 mg will be released as aerosol to the atmosphere. In condensing steam atmospheres the leak will quickly plug with water.

Chapter 7, pg. 7.36, Section 7.7, 2nd paragraph

COMRADEX-4 allows input particle size for attenuation calculation. Unpublished COMET code sums cases with different sizes to simulate a distribution.

Chapters 6 and 7 (general)

In Chapter 6, the paths from core to containment may be wet or dry, the dry cases producing the greatest releases. In Chapter 7, the containment seems to be considered dry except for pressure-reducing pools and scavenging sprays. The large amount of water originally in the primary system may well blow down into the containment, but there it should remain unless removed, and this seems to indicate that the containment is even more likely to be wet than the paths to it. This should, as in the case of Chapter 6, substantially reduce the releases from containment found in Chapter 7. The primary system can remain dry only by having an elevated temperature - maintained by the decay heat

from the core - while the outer surface of the containment is cooled by the ambient atmosphere. The resulting temperature gradiant should in general, as found in the particular case of TMI, result in continual condensation of steam at containment walls and accumulation at warmer regions where evaporation occurs. If the accumulation should be by rain through the containment, a very effective removal process for Iodine and aerosol is provided.

For core meltdown sequences, as well as other cases, as the hot gases leave the primary system and expand into the containment volume, the exiting gas is cooled in this expansion and water vapor condenses on the gas borne particles forming large particles.

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