

OAK RIDGE NATIONAL LABORATORY

OPERATED BY
UNION CARBIDE CORPORATION
NUCLEAR DIVISION



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OAK RIDGE, TENNESSEE 37830

February 3, 1983

Dr. Mel Silberberg
Division of Accident Evaluation
U.S. Nuclear Regulatory Commission
MS 1130 SS
Washington, DC 20555

Dear Dr. Silberberg:

I find the NUREG-0956 draft to be a major disappointment, and I strongly recommend that it not be published in its present form. There are several reasons for this, based largely on my opinion that the report blends some fact, some modeling, some speculation and assumption, and a few very specific scenarios to arrive at conclusions that will be taken out of context, over-generalized, and accepted by some as far more definitive than justified. There are occasions when it is better to admit the answer is unknown than to insist in presenting an apparently precise, but perhaps erroneous, answer.

At the same time, I agree that there has been progress that should be reported, although I am concerned there has been so little progress. My objection here is with the degree to which definitive impressions are likely to be derived from this report (unjustifiably in my opinion), particularly from the first and last few pages which constitute the part that the majority might read.

The minimal attention to chemistry (and to some extent physics), along with the superficial and confusing mechanistic description that is given, is indicative of a very serious shortcoming - namely that the principal authors not only do not take such factors seriously, but they don't appear to be really aware that they exist. Chemistry is relegated to some simplistic assumptions, either buried within the codes or supplied as undocumented input, without adequate definition in either case.

The treatment of physical and chemical adsorption described at the review meeting suggested a lack of understanding of this subject, and some confusion of the adsorption processes with possible subsequent processes such as diffusion into an oxide film on the plenum. Mechanisms going beyond adsorption seem to be grouped under the name of chemisorption.

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The TMI data, although limited, should be examined for indications of chemical behavior. There is significant cesium associated with surfaces of the plenum, even after nearly four years immersion in water. There is significant tellurium outside the primary system, both on painted containment wall surfaces and in precipitate on the floor. This precipitate also contains significant fission product iodine and control elements. There are separate particles of different control rod constituents in solids recovered from the reactor coolant, suggesting selective evaporation of cadmium.

I will make little specific comment about the computer codes and models because others are better qualified. I will add my support to the contention that (1) they include hidden and unrealistic assumptions about the behavior of nature and (2) whether intended or not, their results tend to assume an aura of respectability and rigor that is not warranted. It seems to me that the computer codes are sometimes used to "launder" opinion (input assumptions, for example). I don't think the overwhelming opposition to the codes, expressed at the review meeting, is in the nature of sour grapes; you would be advised to take it seriously.

I would suggest that a much more satisfactory approach would be to issue a series of reports, the first including part of this one and constituting essentially an update of NUREG-0772. There have been improvements in our understanding of the chemistry of some of the fission products, especially aqueous iodine chemistry. There has been a lot of work on aerosol generation and behavior, and computer models have been improved. All this sort of thing should be grouped in a single report as the basis for subsequent calculations, and there should be periodic updates; this report would contain what science there is, along with the technical state of the art. This material could then be referenced, and not repeated in toto time and time again, as it is now in the excessively long and repetitious safety studies.

There should then follow a series of reports on the results for designated reactors, including all the site- and scenario-specific factors, the first being taken from the bulk of this report. These should exclude the information common to all the studies, by referencing the revised 0772, so they would be shorter and more manageable. I think it would be greatly advantageous to separate the specific studies, which tend to get bogged down in a myriad of details, from the more general considerations that apply across the board.

Comments more specifically directed to the text follow:

Last paragraph, p. 2-2, says physical and chemical processes are incorporated on a "mechanistic basis". This term is not just vague; it is inaccurate, because in many cases the input assumptions, not mentioned here, are more important than mechanistic models. Later on (p. 5-13)

Dr. Mel Silberberg
Page 3
February 3, 1983

there is mention of the input assumptions. However, there is no good indication of the significance of errors in either the codes or the assumptions. The inadequacy of the approach used is not properly described.

On p. 3.3 and elsewhere these calculations are referred to as "best estimates", but there are various places where clearly conservative estimates were taken, and where effects were omitted from treatment because there was no readily apparent way to model them [paragraph (8) on p. 3-5, for example, but most cases not overtly noted]. This is a best estimate only in some respects, and conservative in others.

Bottom paragraph, p. 4-5: The distinction here, compared to TMI, is not clearly drawn. In all cases, fuel heatup begins only after the water level drops below the top of the core.

Page 5-8 and vicinity: The text in this area is unacceptable. It is stated that control rod material melt release is simulated as tin and steel. Other materials known to be present (cadmium, indium) are ignored. Although compounds, alloys, and such will have different properties, it is of some use to consider the boiling points of some elements that are present. The order of increasing boiling point, after water, is cadmium-765°C; tellurium-937; (CsI-1280); antimony-1637; indium-2050; silver-2193; chromium-2642; tin-2687; nickel-2835, and iron-2385. The control rod elements boil at 765, 2050, and 2193°C, while the stand-ins (tin and iron) boil in the 2700-2900°C range, several hundred degrees higher. This sort of modeling appears completely erroneous to me.

Apparently it is assumed that control material, or at least silver, is not released; this is based on a single preliminary report that is not referenced. In contrast, there are repeated observations of cadmium boiloff. Alloying can reduce volatility, but tin is certainly alloyed with zirconium and it has a much higher boiling point than silver, yet it is assumed to be released while silver is not. Similarly, it is assumed tellurium is not released, based on an undocumented observation that is contradicted by TMI. Also, note that the effects of alloying or compound formation may be eliminated if the zirconium is completely oxidized. Models that depart substantively from known properties of materials must be well documented and explained at least qualitatively. In such respects the present text is totally inadequate.

Near the middle of p. 5.8, it is stated that control rod silver is not available for aerosol generation, but no reference is given. This probably refers to recent German work, and ignores contradictory work elsewhere. Unless such behavior can be unambiguously documented and rationally explained, it would be advisable to relegate it to a footnote, at best. Meanwhile, the analysis should not be based on this assumption.

Dr. Mel Silberberg
Page 4
February 3, 1983

Page 5-10 and Fig. 5.2: The curve for structural material (from Table 5.1) should be added. Other curves might be better labelled, such as adding "Sn from cladding" to the Sb curve.

Middle of p. 5-15: Are the "particles" referred to in the first bullet the same as the ones in the third bullet? Are ONLY CsI, CsOH, and Te considered as constituents of particles (first bullet), with no other material?

Top of p. 5-16 is also unclear. Terms in equation are poorly defined. The terms "deposition mechanism" in line 4 and "deposition terms" below the equation are ambiguous.

The last two sentences of the first paragraph are not directly related to the foregoing, and are unclear. Perhaps these ideas should be in a separate paragraph. The comments on "particle agglomeration" should be separated from the others since this process does not have a shorter time constant.

Middle of p. 5-20: Is a steam explosion an oxidation?

Bottom of p. 5-22: Is steam condensation (and particle growth) treated as steam condensing on inert surfaces or on a chemically active surface? Salt particles can take up water, grow, and "melt" when the relative humidity is well below 100% and water would not condense on inert surfaces.

Page 6-3, line 15: Should "existing" be exiting?

Bottom p. 6-20 and top p. 6-21: This is another example of a conservative assumption. You permit radiative heating from below, but not radiative cooling from a structure around 1400°C to the relatively cool structure above.

Middle paragraph of p. 6-21: Maybe the thermal-hydraulic conditions inside containment can be predicted with confidence, but are they? I have been surprised at the extent of compartmentalization in the lower part of the TMI containment. Are sequential control volumes used? Is the containment ventilation system included because, even if not operating, it can provide one leg of a thermal convection loop? (Ditto stairwells, elevator shafts.) At TMI the cooling-ventilation system ended up nearly the highest radiation source (except the sump) and the top end where it discharged suggests high containment surface temperatures (scorched paint) and high radiation levels. (The reactor for this study does not have containment coolers like TMI, but does it have a containment ventilation system?)

Page 6-22: Apparently 33,000 MW day/ton is an error.

Middle paragraph and Table 6.7 are in error because of omitting cadmium and indium. The text says what the silver release was not, but not what it was; was it zero? See also comment above re. p. 5-8. It is stated again

Dr. Mel Silberberg
Page 5
February 3, 1983

that control rod silver is not a source of aerosol, but this is not documented and, in my opinion, not firm enough to base this study on.

Page 6-31: Iodine section says iodine is present as "nonvolatile iodide", but CsI is really rather volatile compared to many other constituents; suggest "relatively nonvolatile". In last half of paragraph, was the 2×10^{-7} fraction per hour based on reappearance of iodine after Kr venting at TMI? It is not clear where it came from. The 0.05% figure appears conservative, since no TMI measurements were quite that high. It is stated to be conservative; it is not a "best" estimate.

Page 6-32: This seems to repeat much of what was just said. It might be worth summarizing by pointing out that volatile iodine is always a very small fraction of the total.

Page 7-27 and -28: Tables appear to be misnumbered.

Page 7-2: The statement of a 25% variation in temperature, using the Centigrade scale, causes concern about the scientific insight of the authors.

Sincerely,

D. O. Campbell

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Chemical Development Section
Chemical Technology Division

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