UNIVERSITY OF VIRGINIA
SCHOOL OF ENGINEERING AND APPLIED SCIENCE
CHARLOTTESVILLE, 22901

DEPARTMENT OF NUCLEAR ENGINEERING AND ENGINEERING PHYSICS
REACTOR FACILITY

TELEPHONE: 804-924-7134

REYNOLDS

June 6, 1983

TO: R. Bernero and M. Silberberg

FROM: A. B. Reynolds

SUBJECT: Comments on Source Term Review Second Meeting

I am sending you several comments and general impressions gained from the source term review meetings of May 24 and 25.

ABR:sb

Al Reynolds

Enclosures

Comments on Source Term Review from Second Meeting

A.B. Reynolds
University of Virginia

1. NRC Approach

I was exceedingly pleased with the revised approach to source term evaluation being taken by NRC, as described by Silberberg. Elements 1 through 4 effectively identify the important parts of a comprehensive source term review. Despite the ambitious nature of the program, a less comprehensive one could easily result in misleading conclusions.

On Silberberg's chart for the scope of Element 4, he did not specifically list the probability analysis. He showed "uncertainty and sensitivity analysis", but this differs, it seems to me, from probability analysis--which, when combined with the consequence analysis of Element 2, would be required to give the overall risk appraisal. I expect the absence of probability analysis from the list was an oversight; but if not, where is it to be included?

The Battelle team is doing an impressive job of performing and coordinating Element 2. My hat is off to them.

The review process is useful despite its limitations. It forces Battelle to justify their results against both expected and unexpected questions. It allows input from others; some of the remarks from the observers indicates to me that there is often insufficient communication between Battelle and some industry groups knowledgeable about the specific reactor designs being analyzed. I sense that the peer review opens the door to increased communication between knowledgeable parties between review meetings. On the other hand, the peer review committee is limited in its effectiveness since we are not actually performing calculations or exercising the codes which is really the way to understand the models more completely. This was underscored by some rather perceptive comments by Bob Burns which he is able to provide because of his experience with detailed independent analysis being done by IDCOR. For this reason the review/appraisal of IDCOR results (as shown by Silberberg as part of Element 4) will be particularly important.

2. Further Documentation

It is important to make available to the review members a description of the NAUA code. Our present lack of understanding of the details of the NAUA models or their verification is a serious gap. We hardly know what to pursue in detail or how to assess the reliability of one of the important stages in the scenerios. Regarding MARCH and MERGE, I think a more complete physical description of the models would be useful; and I agreed with Don Rowe in his plea for more diagrams of the flow volumes and more flow and heat transfer transient results in the various volumes during the scenarios. There is a limit to how much information should be presented--which is a problem to Battelle--but I think they have erred so far on the side of not presenting enough.

I look forward to receiving information from Sandia on Te, C_sOH, and C_sI surface chemistry--and from others if others are working on this. Surface chemistry appears to be important and we are in the dark about whether Battelle's models are reasonable--and several of the review members (Ritzman, Johnson, Castleman, for example) appear to be capable of judging the reasonableness of the data.

3. MARK I Drywell Relief Paths

I recommend that Battelle follow up on a remark made by Bernero with regard to alternate relief paths through the drywell of the MARK I containment. In conversations with Steve Hodge of ORNL, I gained the impression, later addressed by Bernero, that the conduits for the electrical leads which penetrate the drywell represent large potential flow areas. These pathways may become available for pressure relief when the drywell pressure rises so that drywell failure may be prevented altogether. Perhaps without sudden rupture of the drywell the SGTS could handle the flow from the drywell to reduce greatly the probability of a 6' RCB failure.

In addition to accounting for the drywell relief paths, accounting for the reduction in power label below 30% for the TC sequence may help reduce the likelihood of drywell failure. In the TC sequence the power eventually drops from 30% to about 20% since the ECCS flow is ~ 20% of full flow. This may reduce the rate of steam or hydrogen pressure buildup enough to allow relief through the electrical lead conduits to prevent sudden drywell failure.

4. Design Features Not Covered by Selected Reactors

While recognizing the need to analyze specific plants, I remain concerned that the plants selected may not include certain design features that can affect the source term. Two such features were discussed at the review meeting. The first is whether the concrete below the reactor vessel contains limestone since this would affect CO_2 release from reaction of core debris with the concrete. The second is whether water released early from a PWR primary system to the containment can flow to the cavity below the reactor vessel so that core debris might fall into a pool. This water would add to the water in the accumulators as water that could boil to produce a steam spike in a TMLB' accident that could threaten containment, and this steam spike might be larger than that for Zion or Surry. Cybulski included an analysis of a Babcock and Wilcox plant where water can flood the cavity below the vessel and he calculated higher steam pressures for this case (~100psi versus ~80psi, I recall). We should be sure that there are no W or CE designs out there where the steam spikes can get higher because I think we are getting close to the threshold of containment failure.

Since the effort in the source term review is focused on specific reactors, I am left confused about the generality of the conclusions. I would hope that these reactors are typical enough that broad general conclusions can be reached. Recall that TID 14844 provided one set of numbers for all LWR's. Then WASH 1400 provided a set of numbers general enough but also limited enough for people to assimilate, understand, and communicate. We have not yet talked about how general our conclusions will be from the present source term

review, but I hope that the review of the five specific plants will lead to fairly general conclusions about all LWR source terms.

5. Gradual Slump Versus Coherent Slump Model in MARCH

The answer provided by Denning for a gradual slump model was persuasive, I thought. This should be written up as part of one of the elements supporting the computer code models. The data base to support the model is still weak, however, and I never got an answer as to how sensitive the source term is to variations in the model. I expect that there is relatively little sensitivity. The phenomenon that might have been affected most strongly by coherent versus gradual slump into a water pool in the lower head is a steam explosion. I am confident that a steam explosion even in a coherent slump cannot rupture the upper part of the vessel--much less the containment--but it might influence the later course of events in the primary system.

6. Condensation in the Auxiliary Building in Sequence V

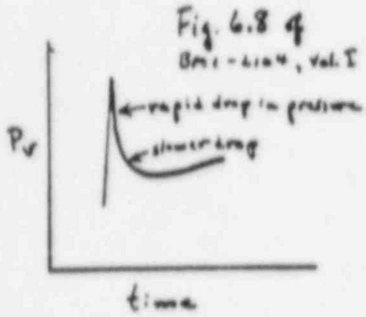
I remain uneasy about the Battelle calculation that minimizes the effect of condensation in the auxiliary building in the Sequence V scenario, but I confess that I don't know what to do about it. Perhaps don't do anything until we find out what the relative probability, hence risk, of the sequence is. I understand what Battelle is saying, i.e. that the rain forest effect is over by the time dry steam with fission products arrives. I expect that industry analyses will continue to disagree with this. Alternately there is the industry result hinted at the first meeting by Bob Burns that the pipes would plug up with aerosols before dry steam with fission products would get to the auxiliary building. If the probability for this accident remains the same order of magnitude as in WASH 1400, then we had better be pretty sure of ourselves about the consequences since, the way Batelle is analyzing the sequence, it will continue to be the highest risk sequence, as it was in WASH 1400.

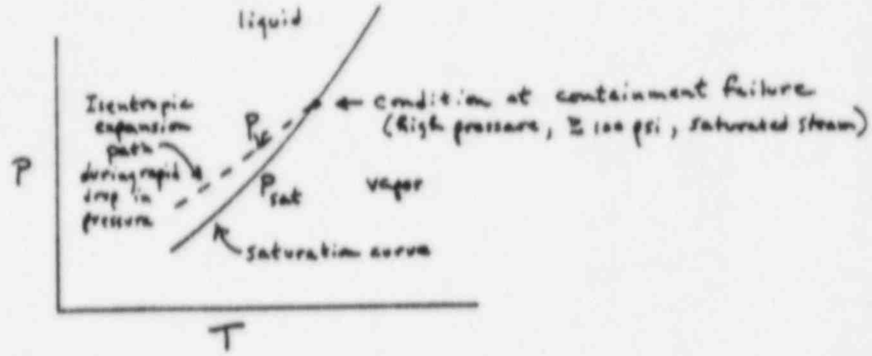
7. Condensation and Fission Product Sweepout inside the Containment after Containment Failure

A remark by Cooper at the review meeting should be followed up. Battelle was discussing PWR containment failure from overpressure from a steam spike when Cooper pointed out that the resulting supersaturation would lead to condensation which could sweep out much of the fission product inventory. I presume this part of the scenario would be calculated by the NAUA containment rode, but I doubt whether there is any provision in NAUA to analyze supersaturation, nucleation, and droplet growth.

The phenomena may be important also for MARK I drywell failure and MARK III containment failure if and when the failure results from steam pressure.

I envision the following scenario after containment failure by steam overpressure.





The steam leaving the containment would flow through the break in choked flow at first--either isenthalpically or isentropically. The steam remaining inside the containment would expand isentropically to continue to fill up the space in the containment, as shown by the dashed curve on the P,T diagram. Hence the steam would be supersaturated and would want to condense; the "supersaturation ratio", S, would be P_v/P_{sat} at the particular steam temperature. Nucleation would occur throughout the containment at a rate that can be calculated with reasonable confidence from classical nucleation theory. The nuclei would then grow into droplets as condensation proceeded. I would expect that a quite dense rain would occur that would sweep out most of the fission products and aerosols. On the other hand, the fog droplets might remain submicrometre in size, in which case they may not have time to settle out.

These theories have been used for the formation of rain, for fog formation in cloud chambers and in steam turbines, and even by us at the University of Virginia for the successful analysis of droplet sizes in the condensation of UO_2 in ORNL's FAST tests (Nucl. Sci. & Eng., 83, pp. 459-472, April 1983). There are probably simpler methods than we used; but, regardless, some type of analysis should be applied if it is concluded that early failure from a steam spike is possible. In ORNL's aerosol experiments with steam, have they ever looked at the effuct of higher than atmospheric pressure saturated steam followed by a sudden decompression?