RLTZMAN

February 2, 1983

Dr. Mel Silberberg
U.S. Nuclear Regulatory Commission
7915 Eastern Avenue
Silver Springs, MD 20910

SUBJECT: Comment on NUREG-0956 (DRAFT)

Dear Mel,

The Commission and Staff are to be congratulated for holding the peer review of the referenced draft report last January 25 and 26. I appreciated the opportunity to serve as an invited member of the review group. Although comments were offered for the record at the meeting, it seems appropriate to submit further comments of a clarifying or amplifying nature in writing as follows:

(1) The amount of work accomplished in assembling the calculational tools and in performing the analyses presented in the report is very impressive. Battelle-Columbus Laboratories staff who participated in the effort are to be commended for their diligence.

(2) The mass of numerical results generated during the analyses and given in the report require considerable time to examine, digest, and compare with other calculations. This process could not be completed in the brief review period and thus the comments given here are incomplete. Consequently, the opportunity to review the work again in April is welcome.

(3) The additional analytical method and newer computer codes applied to the source term problem are noteworthy and of course determine the results obtained. I concur with other reviewers at the meeting who recommended that a detailed description of this new methodology should be a part of the project documentation.

(4) The core/concrete radionuclide release and aerosol generation model developed by Sandia staff for this study appears to be a clear advance over previous methods used to specify source material releases for this portion of severe accident phenomenology. Early publication of the details of this work should be encouraged.

(5) A comment made with respect to the NUREG-0772 report should be re-emphasized here. The empricial fission product and structural release model from NUREG-0772, which is used in the CORSOR code, is of very doubtful validity when core melting has progressed to the stage where slumping is occurring. The model will lead to an erroneous prediction of the composition of released structurals and probaby an overestimate of the amount of aerosol generated within the reactor pressure vessel. A thermodynamic based vaporization model would be preferable which would include contributions from control rod materials as well.

(6) Use of thermal-hydraulic information from the MARCH 1.1 code in these more mechanistic source term release and transport analyses places undersirable limitations on the work and leads to over conservative and even erroneous results. An example of this latter tendency is the magnitude of the steam spike that is predicted for the so-called TMLB' sequence at reactor pressure vessel failure as shown in Figure 6.8 of the report. It is suspected that the leakage rates calculated by MARCH 1.1 for the containment in sequence AB are also artificially high. Improvements in this important area of accident analysis is needed if the source term definition effort is truly to be regarded as a "best estimate" endeavor.

(7) It should be noted that SAI analyses of upper plenum temperatures for a PWR during core uncovery, heatup, and fuel melting in severe accidents, which have been performed for EPRI, produce results that generally lie between the Case 1 and Case 2 predictions obtained by Battelle using the MERGE code. Since we also used MARCH 1.1 to define the characteristics of the gas flow that would be entering the upper plenum in such accidents, it is not too suprising that similar heat-transfer results were obtained.

(8) The listing and description of the numerous uncertainties in analyses and procedures presented in the report in Section 3 needs to be highlighted for all readers of the report. I personally agree that each of these items represent significant sources of uncertainty or error. Perhaps readers could be directed to this section in the abstract, summary, or introduction to enhance the probability that proper qualifications on the results will be registered in a timely manner.

I trust that the above comments will be of help in completing the next iteration on the report. If questions arise or clarification of any particular point contained in the comments is needed, please contact me.

Very truly yours,

R.L. Ritzman

RLR:kar

cc: Z.T. Mendoza

REYNOLDS

COMMENTS ON PEER REVIEW OF NUREG-0956
A. B. Reynolds
January 31, 1983

1. Need for Simultaneous Development of Four Topics Related to the Source Term

I see a serious danger in NRC's present plan to publish a large new report on calculated radioactivity releases in relative isolation from several essential interrelated topics--danger to NRC's potential future position in the area of source term and danger to the creation of a reasonable perspective in the minds of the public in this area. I view NUREG-0956 as such an isolated report.

I recommend that four topics be developed simultaneously to avoid this danger, namely

1. Data Base
2. Radioactivity Releases
3. Containment Failure
4. Probability Risk Analysis

NUREG-0956 covers only the second topic. Either separate reports on each topic should be issued simultaneously or each topic should comprise one volume of a four-volume report.

The data base, which was the dominant topic in NUREG-0772, is hardly mentioned in NUREG-0956. An updated review of the data on which Battelle's calculations are based needs to be published. A description of experiments in progress and planned and the data expected from those experiments should be included in the data-base reports.

The Battelle-type analysis of radioactivity release is complex enough to warrant a separate report devoted solely to it. (This is in disagreement with some views expressed at the meeting to add data-base material--like chemistry--to NUREG-0956.) Each input quantity in the Battelle analysis should be referenced specifically to data provided in the data-base report.

The peer review meeting brought to light the enormous importance of early containment failure in developing a perspective on the source term. The question is crucial enough to require a study and review comparable to the data-base review and the radioactivity-release analysis. Some industry observers at the meeting apparently believe that the early failure in the AB and TMLB' sequences (i.e. AB-γ and TMLB'-γ--or TMLB'-early δ) cannot occur. Or if they can, there will surely be a vigorous debate on the proper probability to attach to such failures. The radiation release report should not be published until NRC developes a position, also subjected to peer review, of the probability of early containment failure. It should be possible for NRC to develope an interim position based on current knowledge, with the

recognition and acknowledgement that full resolution of this issue may not occur for several years--perhaps until after the Sandia containment experiments and further work on hydrogen burn pressures in AB and steam spikes in TMLB' sequences. NRC's position on the entire source term question must remain interim to the same extent that NRC finds their position on containment failure to be interim.

The fourth topic is the PRA which, in addition to probabilities of events in each sequence, must use the results of the radioactivity-release analysis and the containment-failure information. The peer review committee did not have "Chapter 8" of NUREG-0956 which we were told would be forthcoming and would contain PRA. I am very uncomfortable with this situation. The PRA is being treated like an afterthought, not subject to the same peer review as the release calculations. I share the concerns of some of the industry observers that NUREG-0956 is concentrating only on release results, and that publication of these results without appropriate accompanying PRA results will create an unrealistic perspective of the source term issue in the minds of the public.

2. Status of Mechanistic Approach

The mechanistic approach being proposed by the NRC for calculating fission product releases for specific accident scenarios is commendable. The methods being used by Battelle appear to be the best currently available.

The NUREG-0956 results are interim values in a field that is still evolving, however, both with regard to data base and methods development. This status should be emphasized more strongly in the report. The two sentences devoted to this caveat in the middle of page 2-2 represents insufficient emphasis.

If for reasons unexplained to the peer review members the Commission insists on publishing new release values by the summer of 1983, the status of more advanced codes like SCDAP, MELCOR, etc. (which were not discussed at the peer review meeting) and experimental programs like Marviken, DEMONA, etc. should also be discussed in the report to provide perspective on the continuing evolution of the field.

3. Disagreement Between NUREG-0956 and Industry Results

A number of representatives from industry appeared to disagree that radioactivity releases as high as some reported by Battelle for the AB-β and γ, TMLB'-γ, and V scenarios are possible. I recommend that NRC request the appropriate computer codes from industry and have NRC contractors compare the methods and results of Battelle and industry groups at each possible step in the accident scenarios. Requests should be made of EPRI, IDCOR, and Stone and Webster. If these groups do not provide NRC with their codes, or if alternative methods are not really available, then NRC could more effectively discount the objections to NRC results.

This comparison is especially important with regard to condensation phenomena in the auxiliary building in the V sequence. The Battelle methods show superheated steam entering the auxiliary building with the fission products and little attenuation there. Stone and Webster calculations show prolonged condensation (i.e. a "rain forest" for 3.5 hours) in the auxiliary building with an accompanying high rate of fission-product attenuation. The Stone and Webster calculation has been well publicized and has an effective forceful proponent in Ed Warman. There is enough plausibility in the Stone and Webster scenario that, providing Stone and Webster will make their methods available to NRC, I think it is imperative that NRC have its contractors make the necessary comparisons to settle an issue of this importance before publishing results of its own.

A second important area of disagreement is the magnitude of a steam spike in the TMLB' sequence shortly after vessel melt-through. Reference was made several times to Henry's analysis. NRC should initiate a detailed comparison of Henry's model and the models in MARCH due to the high importance of the steam-spike phenomenon on potential early containment failure with accompanying high release rates.

4. More Specific Comments

- The Battelle models should be modified to include fission product decay heat on structures from fission products deposited on them--in the upper plenum and in the piping.

- I was unconvinced that flow patterns in the containment building have little effect on fission product retention or behavior--which was the impression I got from Lee's response to Ginsberg's question. Also the discussions of diffuseophoresis--both the lack of its treatment in the NAUA code and the reference to German data indicating its importance--left me with the impression that this phenomenon had not been adequately investigated.

- Behavior associated with the high-pressure ejection of the molten debris upon vessel melt-through in the TMLB' sequence was not specifically treated. Is the subsequent scenario influenced by the uncertainty in the aerosol particle size distribution from this ejection?

- Regarding the steam spike in the TMLB' sequence, consideration should be given to the melt falling into water in the reactor cavity (or sump) in addition to the reverse. The reverse case, treated in NUREG-0956 as I understand it is water from the vessel and accumulators falling onto a melt which has fallen into a dry cavity or sump. Had the TMI core continued to melt and melt through the vessel, my understanding is that there would have been water present below the vessel at the time of melt-through due to overflow from the drain tank. An observer at the meeting suggested that the basis for Henry's argument against rapid enough steam generation to produce a steam spike large enough to threaten containment was that the core debris particles would be too large for fast enough heat transfer to water falling on the bed. The reverse situation--molten material falling into water--would result in fine fragmentation of the melt so that subsequent heat transfer may be faster. (Here I am not concerned with the steam produced during a steam explosion but instead with the steam produced during several minutes after contact between the melt and water.)

  If the case treated in NUREG-0956--i.e. accumulator and primary system water falling on hot debris--is the more important, then the peer review members should be presented with an assessment of Henry's model or with arguments why such a large steam spike can be produced.

- During the discussion of the effect of containment sprays on purging fission products, there appeared to be a lack of understanding by the Battelle representative of the difference in mass median diameter and number median diameter. The position was then taken that it doesn't matter anyway since purging is effective

regardless of spray droplet size. I question this position. The equations added to NAUA for the analysis were carefully described in NUREG-0956, and one wonders why if droplet size is irrelevant. Moreover, while "most" fission products might be purged, one is concerned with the fraction not purged, i.e. which is available for leaking, and I would like to see some parametric variation of spray droplet size to be convinced that the fraction not purged is insensitive to droplet size.

- Regarding containment failure, two items are of concern: First, the justification for assuming a 7 ft^2 hole appeared weak. How sensitive are the results to this parameter? Second, how much attenuation might take place through a containment failure? It appeared that no credit for further attenuation was made in NUREG-0956. Several observers suggested that some attenuation could be justified so it would be useful to elicit from them how this might be done. These two items reinforce the notion that containment failure is itself a complex question involving failure mode, location, etc., and that a detailed review of containment failure must be an integral part of a source-term review.

- I took special note of Levy's remark about the need to combine MARCH and MERGE type analyses on the way toward significant improvements needed for MARCH. Do any of the new codes like SCDAP and MELCOR move in this direction? Will MARCH 2 be much better, and what is the schedule for MARCH 2? I am not against using MARCH 1.1 as in NUREG-0956 as long as a proper perspective on its shortcomings and plans to improve or replace it are appropriately emphasized.

- I agree with the need expressed to provide figures showing the control volumes used in MARCH, MERGE, and TRAP-MELT. Also, Burns's concern about the need for more volumes in long systems in which mixing is occurring should be considered.

- Concern was expressed several times about the small contribution of silver to aerosols. The explanation of the findings of Parker of ORNL together with thermodynamic arguments regarding the vapor pressure and dispersal of cadmium and silver sounded logical to me, and my confidence in Parker's ability leads me to feel that Battelle is right in basing their analysis on his results. The finding of silver and cadmium in TMI in proportion to the proportions in the control rods appears to have nothing to do with silver aerosol production.

- Battelle says that mass balances are calculated; and to a limited extent, results are reported in NUREG-0956 for materials like CsOH and CsI on retention in various parts of the system. Warman of Stone and Webster claims that he and his colleagues could not

follow where the important fission products ended up in each scenario after trying to do so. I have not tried to do this, but I think it would be useful in the report to be sure that it is easy to follow the final location of all the important fission products for each scenario, both to demonstrate the mass balance and to make comparisons with other calculations more effective. (Perhaps this is done sufficiently in Table 7.18 for the TMBL'-γ sequence. I rather like that table although I can't help but be surprised that condensation does not cause more of the CsI to settle prior to containment failure. See next comment.)

- How good is the calculation of 0.8 for the iodine containment release fraction in TMLB'-γ, as shown in Table 7.20? Why doesn't enough steam condense after failure to cause enough rain inside the containment to dissolve most of the CsI before it has a chance to escape from the rupture? It is difficult for me to believe that all but 20% of the CsI aerosol that enters the containment escapes. I think that if NAUA is telling us this, some intermediate results should be added to the report to convince the reader of its validity, especially since this is probably the most pessimistic result in the report (other perhaps than the 0.2 release of sequence V).

- Tables 7.16 and 7.20 show a factor of about 10 increase in iodine release of AB-β over AB-γ. I question whether this increase should be so high. First, Fig. 7.17 shows only a factor of 3 increase in aerosol mass released in AB-β over AB-γ. Since the CsI in AB-β enters a containment full of condensing steam--a rather thoroughly wet atmosphere--and since CsI is so soluble, I would expect the difference between CsI release between AB-β and AB-γ to be less than the difference in total aerosol release, hence less than a factor of 3. Second, there must be <u>some</u> time between CsI release from the primary system and release through the opening in the containment, and I would expect that much of the CsI should be swept out in the wet atmosphere during this transition. Nowhere in the report is the opening in the unisolated containment of AB-β discussed (neither in Section 6.1.1 nor in Chapter 7). Surely the release from the opening must depend on the size and location of the opening. For these reasons I am very suspicious of the validity of a release as high as 0.5 for Iodine as reported in Tables 7.16 and 7.20.

- In Table 7.16 I noted a factor of 3 release for the AB-γ Cold Te and Hot Te over the AB-γ Cold and Hot sequences. Why does the Te release increase so much? It does not appear to me that retention of Te in the primary system in AB-γ could account for this large a reduction relative to AB-γ Te; hence, I question the large release result (0.12) of AB-γ Te. Unless there is strong evidence that no Te is released from the fuel in the primary system, it may be misleading to report the AB-γ Te result.

- I question whether the CORRAL-2 results should be included in the report. Since CORRAL-2 treats iodine entirely as elemental instead of CsI, it is clearly incorrect. I expect that this error leads to the factor of 10 increase in release of iodine for the AB-γ calculation for CORRAL-2 as compared to NAUA (about 0.04 in Table 7.16 versus 0.4 in Table 7.17). The high CORRAL-2 result is almost surely unrealistic for this sequence; hence it's inclusion in the report is detrimental to the perspective being transmitted by the report.

- In general I was pleased to see the use of NAUA which treats steam condensation on aerosols and the emphasis the report places on this effect. In fact the lack of this effect in CORRAL-2 on CsI is the main reason that CORRAL should not be used. If another code is to be used for comparison that treats condensation, I would suggest MAEROS, or perhaps some of the work of Loyalka. I also am aware of how weak the data base is for demonstration of the extent of CsI removal in steam containment environments. About the only steam environment containment test with a soluble aerosol like CsI was the AB-2 test at CSTF at HEDL, in which Na_2O and Na_2O_2 were the soluble aerosols. The major steam containment facility in the U.S. is the ORNL NSPP facility--which is using non-soluble Fe_2O_3 and U_3O_8 aerosols--and which has been analyzed by Schöck with the NAUA code but with success only by using artificially high diffuseophoresis. The larger German steam containment facility, DEMONA, is not yet in operation. I expect that wet steam environments get rid of CsI rapidly (as various industry spokesmen are arguing) so that proper modelling of the containment environment is essential to NRC's revision of its source term position.

- Application of the Battelle methods to TMI-2 was brought up several times at the meeting. Is it obvious such a calculation would predict the small measured iodine releases and large noble gas releases from the auxiliary building? Likely the TMI-2 release through the auxiliary building was enough milder than the V sequence analyzed in NUREG-0956 that analysis of TMI-2 by Battelle would shed little light on the validity of their methods for the V sequence. However it would be useful to be assured that the Battelle methods applied to TMI-2 do not show higher iodine releases than measured while simultaneously showing the rather large noble gas releases observed.

- The containment failure Greek letters--β, γ, δ, ϵ--should be identified in Table 4.2 in addition to the Roman letters.