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DEPARTMENT OF CHEMICAL ENGINEERING
BERKELEY, CALIFORNIA 94720

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Mel Silberberg
Branch Chief, Fuel Systems Research
Division of Accident Evaluation
Office of Nuclear Regulatory Research
Washington, D.C. 20555

Dear Mel:

Thank you for bringing us up to date with some of the progress on source term estimation since the APS Study Group completed its report. Thank you also for the opportunity to comment on the draft report NUREG-C956, "Reassessment of the Technical Bases for Estimating Source Terms".

Overall, I found the draft of NUREG-0956 to be a reasonably accurate account of progress made with the following important exceptions. In my view, this draft overestimates the level of documentation of the codes used in BMI-2104, the quality of the peer review process for BMI-2104, and the degree of validation (comparison with meaningful experiments) of the various codes. I also feel that this draft is unbalanced with respect to the very limited attention given to the experimental parts of NRC research on the source term as opposed to its coverage of code development and code output. I also think the sensitivity studies (QUEST) deserve considerably expanded coverage. I elaborate on these general topics below.

One of the greatest frustrations during the APS study was our repeated inability to get clear and appropriately thorough descriptions of the bases of the codes used. This is reflected in our conclusion V.E.5. and recommendation VIII.E.3. I personally feel the idea that everything must be published in peer review journals may have been overstated. What I believe is that as much as possible of NRC sponsored research be submitted for journal publication to gain addition review by a larger readership and to convince the technical community as a whole (not just the narrow source term community) that source term estimation is indeed soundly based. Not all of NRC's sponsored research is appropriate for journal publication. Nonetheless, these aspects must be written up as NRC reports with the same degree of clarity, precision, and thoroughness demanded of journal publication articles. I do not believe that ORNL/TM-8842, although considerably improved from the draft version available to the APS Study Group during its deliberations, meets these criteria. Would a reasonably well informed scientist or engineer, but one not steeped in source term research, be able to gain sufficient insight into what was done in order to be convinced that the approach was soundly conceived and soundly executed?

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I think not. I also think that poor documentation probably has impeded NRC progress. As one example, take the question of the decay heat generation rate which you mentioned had been treated differently in the MARCH and CORCON codes. Surely, since decay heat is the driving force for the accident its method of calculation should be clearly identified. I did find (page 80) mention that MARCH 2 uses ANSI/ANS-5.2-1979 but there was no indication in Chapter 5 on CORCON as to what it used for decay heat generation. While the numerical changes may or may not be important, I got the impression that the BCL people didn't know what the SANDIA people were using. Such inconsistencies, especially when found late in the study, do not inspire confidence in reviewers.

Without fully documented accounts of what was done, critical peer review becomes enormously difficult if not impossible. Of course, the technical Expert Peer Review panel of 14 scientists during the period when much of the source term analytical procedure was being developed was necessary. I regard this exercise more as the use of consultants to provide additional perspectives and guidance to the study, but it was not peer review of the final product. The transcripts of the meetings show that the members often arrived with inadequate time (or motivation) to review material sent them. Moreover, as with the presentations to the APS Study Group, these meetings were dominated by presentations of code output. The discussion of the engineering, physics and chemistry underlying the codes was not dealt with adequately. Moreover, it was a disappointment that when some of the same questions on the underlying science or engineering asked by the Technical Expert Peer Review Committee were subsequently asked by the APS Study Group, clear, convincing, thorough responses had not been formulated by the NRC contractors.

The question of code validation has barely been scratched. NUREG-0956 requires a clear definition of what is meant by validation. To me, this ultimately means comparison of code output with experiment. Both well controlled, limited phenomena, small scale experiments and realistic, adequately instrumented, large scale experiments are necessary to insure that all important phenomena are modeled with sufficient accuracy. See APS conclusions IV.C.10.e, f, g, V.E.7, VIII.C.5, and recommendation VIII.E.1, with emphasis on the word "integrated". The slowness with which data from large scale experiments are being released is quite distressing. In many sequences, it is calculated by TRAP-MELT that 85% of the iodine and cesium are retained within the primary system and this is one of the main reasons for lowering of the source terms of these elements compared to WASH 1400 (especially for by-pass sequences). Yet we know virtually nothing about the experiments at LACE and MARVIKEN and the experiments at PBF have not been completely analyzed from the point of view of fission product transport and deposition. Is it any wonder then that ORNL/TM-8842 concludes (page 362) that "the quality assurance level for the TRAP-MELT code is not presently very high" and (page 372) "TRAP-MELT is lacking in formal validation". I feel CORCON

is at a similarly low level of experimental validation. NAUA in virtue of experiments at ORNL/NSPP and DEMONA is probably one of the better validated codes, although questions of natural convection and thermal stratification still bother me.

Since our understanding and predictive capability is now and will continue to be incomplete, sensitivity studies must be an essential part of source term estimation. This is APS recommendation VIII.E.2; see also the summary for VI.D. The idea is stated explicitly for fission product removal by suppression pools in the last two sentences of conclusion VIII.B.7; our ability to predict suppression pool removal is related to the assurance that aerosol sizes produced earlier in the accident are indeed in ranges where the decontamination factors are large rather than at the minimum of the curve. Similarly, fission product releases computed by VANESSA are nsitively dependent on CORCON's predictions of melt temperature. It would help to interpret the more important sensitivities from the mechanistic point of view, i.e. releases from the melt are sensitive to temperature because of the exponential dependence of vapor pressure on temperature. Releases also may be very sensitive to chemistry (chlorides in the concrete).

I also have the following specific comments:

XIX Conclusion 2 - add qualifiers "A number of important" omissions and...now "partially" accounted for.

XIX Conclusion 3 - add qualifier "Some" remaining areas.

XIX Conclusion 4 - This conclusion needs to be considerably toned down.

XX Add experimental results as an area of improvement, esp. suppression pools, DEMONA.

XXI Add relocation of deposited fission products, especially where molten as an area of uncertainty. This can be important for revaporization or alteration of sequence. Also interpret natural circulation to include thermal stratification in containment.

XXII Conclusion 7 - Change last sentence to "A delay of several hours in containment failure, following the cessation of fission product release to the containment, is computed to reduce source terms significantly."

3-6 Point out in last paragraph how sensitive computed source terms are to these user selected inputs.

3-10 ¶3 - natural circulation above "and through" the core.

3-19 ¶1 - I believe this paragraph underestimates the potential importance of the time of release of volatiles from the core because this will feed into predictions of aerosol size and hence deposition.

3-19 If the silver is returned or retained in the core region, what influence does this have on aerosol generation? Are volatiles refluxed in code calculation? Will molten silver dissolve or alloy with zircalloy?

3-21 last ¶ - ...but "assuming the low gas velocities computed by MARCH rather than much higher natural circulation velocities", generally, the most important...

3-22 and 3-35 TRAP-MELT does not consider nucleation of new aerosols which might be important if flow rates are higher and plumbing is hotter due to natural convection.

3-36 first ¶ - I believe inadequate attention has been given to the possibility of a sustained release from the core following failure of the reactor pressure vessel. See APS IV.C.10.i.

I agree that we have a good understanding of many phenomena related to aerosols. But this does not mean we can predict everything with the needed accuracy because of the complex chemistry, complex geometry, unknown thermal hydraulics, etc. The impression given here is misleading. It is like saying we know the laws of classical mechanics and therefore can predict the motion of turbulent fluids. Point out that this is especially true because of the sensitive dependence of deposition rates on aerosol size.

3-39 Natural convection will also influence the timing of the release and the deposition rates because of changes in composition, temperature and residence times.

3-41 Weaken summary in view of discussion above.

4-18 Is the revaporization from the upper plenum and plumbing following reactor vessel failure, especially with some of the core remaining and with natural circulation, accounted for?

4-37 add qualifier...SPARC is high enough "assuming the size of aerosols reaching the suppression pool are sufficiently large, that the computed" releases to the...

I hope the above comments are decipherable and of use to you.

Sincerely,

Simon Goren

Simon L. Goren
Professor

SLG:ncm

copy to APS members