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OFFICE OF SECRETARY
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Secretary of the Commission
U.S. Nuclear Regulatory Commission
Washington, D.C. 20555

Attention: Docketing and Service Branch

Subject: Comments on NUREG-0956
"Reassessment of the Technical Bases for Estimating Source Terms"

Gentlemen:

Comments were requested on NUREG-0956 in the August 7, 1985 Federal Register. Bechtel interest in the use of realistic source terms for regulatory and for design purposes has been reflected in a number of public statements by our people, our active support of the IDCOR program and our leadership in the AIF's new special action group on regulatory application of new source term information. Since the "source term" is the only source of public risk from a nuclear plant accident, source terms are a fundamental building block for a large body of regulations and have a large impact on the public perception and public acceptance of nuclear power.

We are pleased that NUREG-0956 basically supports our view that the source terms used for regulatory purposes in the past have been significantly overestimated. Perhaps even more important is recognition of the fact that the nature of the "source term" is quite different from the original concept used as a basis for many of the existing regulatory guides -- the cloud of elemental iodine gas concept led to many design features and regulations that do nothing to reduce any residual risk due to hydrogen explosion, steam explosion, etc. As stated in Conclusion 10, the most thoroughly evaluated plant, Surry, showed an overall reduction in risk compared with the Reactor Safety Study which in turn showed significantly reduced risks compared with previous risk concepts. A major part of this reduction is due to the recognition that most of the iodine will combine with cesium to form a salt rather than remaining as elemental iodine and recognition of other natural process which would retain radionuclides in the reactor coolant system and in the plant structures. This is discussed in connection with Conclusion 2. This is recognized in Recommendation One on the same page, XXIII, which says "Improvements are so significant that utilization of new methods is warranted while additional, confirmatory research is being completed." Another aspect is noted on page C.1-6 -- steam explosions were found not to be a source of significant loads. We believe that enough information is available now to justify using lower source terms than have been used in the past and that the NRC should make the necessary judgments now. We believe that NUREG-0956 confirms the original reassessment done over six years ago -- the assessment that led to the conclusion that the source terms originally used for regulatory purposes were unnecessarily restrictive.

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OCT 16 1985
Acknowledged by card.......... pl

We believe that the almost complete reliance on computer codes is not warranted. While the title of the document is The Reassessment of the Technical Basis for Estimating Source Terms, it does not utilize much of the experimental information developed from laboratories or the factual information developed from actual accidents and incidents. The discussion in the last paragraph on page 4-3 highlights the fact that many very low frequency events were evaluated only to provide insight into the source term phenomena and not because they are significant contributors to risk.

Page 4-28, first whole paragraph, states that the calculations in this chapter are not meant to characterize the expected or most likely containment behavior, rather they represent simply a study of the potential challenges in a parametric study of the source term. Parametric studies are of limited value in trying to determine public risks and should not be used for regulatory processes especially if it is known that the cases identified are not likely to happen. We believe that this purely mathematical approach is very confusing and misleading. For example, on page 3-6, it is stated that for Surry an early containment failure scenario was analyzed even though the peak pressure was well below the estimated failure pressure. In the discussion relating to Recommendation 2 on page 8-7, it is stated that the "Source Term Code Package" was reviewed only in the context of severe accidents that completely melt the core. Again at the bottom of page C.1-7 it is stated that the steam spike induced failure of the Zion containment is an event of very low probability contrasted with the high likelihood events which came out of the Reactor Safety Study methodology.

Page C.1-15 (and the bottom of page 14) is another example. It is stated that "Rather extreme assumptions had to be utilized to produce loads of sufficient magnitude to challenge a large dry containment" including dispersal of 100 percent of the core and 100 percent oxidation of all cladding. There is too much emphasis on these improbable sequences and too little discussion of more probable sequences that should be considered in the plant design and procedures.

The sensitivity of the conclusions to containment failure or bypass of suppression pools (for pressure suppression containments) raises questions about the types of reviews performed to date. This would appear to be a question of civil engineering rather than a question of basic physics. IDCOR devoted a significant part of their effort to the containment question. They concluded that the containment capability was much greater than previously estimated and that even if the containment failed the fission product release to the environment would be much less than estimated in past studies.

The American Physical Society group recommends further research but it does not state that research must be done before regulatory actions can be taken as is stated in Conclusion 13. In fact, the American Physical Society report essentially states the opposite - the APS specifically stated that they included no considerations of regulatory actions and did not connect the two at all.

More detailed comments are contained in the attachment. We appreciate the opportunity to comment on this important subject and would be pleased to meet with NRC personnel to discuss our views in more detail.

Sincerely,

M. Levenson

M. Levenson

ML:bbm
Attachment

COMMENTS ON NUREG 0956

Page XXII, Item E, Conclusion Six, states: "These sequences have provided a sufficient test of the capabilities of the computer codes", but there is no indication that the codes have been benchmarked against accidents, incidents or experimental work. An analytical analysis that has not been benchmarked has only limited credibility.

Page 2-8, Item 2.4 - 2. The attenuation for elemental iodine is also greater than assumed in the Reactor Safety Study since this study did not recognize that iodine reacts with material such as steel, light metals and concrete.

Page 2-10, the references to the German risk study are only to the original old German risk study. The newer, revised study which shows much lower source terms has not been referenced and should be used instead of the older report.

Page 3-6, large full paragraph, center of the page. The statement is made "An early containment failure scenario was then analyzed, even though the predicted peak pressure was well below the estimated failure pressure". This is an example of a parametric analysis being supplied an input that the actual analysis says will not happen! This is very unfortunate because the subsequent analysis and risks associated with it have not been properly identified as being parametric rather than the result of the predictive analysis.

Page 3-9, Paragraph in the center of the page. The entire core, the support steel and molten steel from the bottom head of the vessel are all assumed to fall coherently from the vessel upon vessel failure. This appears to be in conflict with all experience in the physical world in such phenomena. It may be easier to model it this way for the computer but doing so ignores the physical factors that yield incoherency.

Page 3-9, bottom paragrah. The discussion that the calculation of containment loads and the calculation of container performance with the March 2 code "is not considered reliable", is extremely important. Why is the code used for a calculation if it is not considered reliable? This is especially true when the same paragraph says that predicted failure of the containment is controlled by the criteria input by the user rather than by the scientific analysis.

Page 3-29, Item 3 at the bottom of the page. The early containment failure is an assumed event for parametric reasons and should not be included in risk studies that people will assume represent "calculated" cases. It also should not be used for regulatory decisions.

Page 3-32, Item 7, top of the page. This discusses resuspension. There are no references to support the assumption that a major fraction of the resuspended material is an aerosol. Material that has coalesed, plated out, settled out, etc., might be ejected by violent forces but there does not appear to be a mechanism for making any significant fraction of such material a stable aerosol. It is most likely to be "chunks" or "flakes", that fall like rocks.

Page 3-39, Discussion of in-vessel fission product release from the fuel and aerosol generation uses the assumption that 100 percent of anything vaporized becomes a stable aerosol. I believe that in the past the government has spent hundreds of millions of dollars attempting to generate stable aerosols in connection with biological and radiological warfare programs. None of these programs had any success -- only the computer seems capable of generating large quantities of high density stable aerosols.

Page 3-41, in containment failure modes, why is the extensive experimental program on reinforced pretensioned/post-tensioned concrete structures that was performed for the gas cooled reactor programs not referenced? This represents a major source of data on failure modes of concrete pressure vessels. The experimental evidence is that they do not fail catastrophically -- they crack, vent and close up.

Page 3-41. The summary states that the codes can be run successfully, can be transferred and have been peer reviewed and therefore by implication they are correct -- but there is no statement of whether the answers given are accurate and no statement about benchmarking them against the physical world.

Page 4-3. The first full pargraph at the top notes that the signficance of this accident sequence lies entirely in its challenge to the codes since it is generally agreed that its risks signficance is very low. Such analyses should not be included in a report whose objective is to analyze risk and make regulations.

Page 4-3. The last paragraph refers to a number of sequences that were assumed to progress to a severe core melt. Here again the basic idea of letting the codes help one predict when there is or when there is not a risk is completely bypassed when one interjects such assumptions.

Page 4-4, under TMLBC sequence, last item. Such technical documents should differentiate between "could", meaning "not impossible", and what is really possible. (40,000 747 airliners "could" crash in one week but it is not possible -- that many 747s do not exist.) The general public translates could to will and while this document is not written for the general public, the intervenors and certain members of Congress will make this a public document.

Page 4-10, first full paragraph at the top, sequences including containment failure have been selected to provide a test of the codes. This philosophy has been commented on before.

Page 4-14, Figure 4.2. Logically it seems difficult to believe that the temperature of a peripheral node at the middle of the core will rise so much faster than the central node at the top of the core. The peripheral nodes at the center of the core have a heat sink in the form of the reactor core barrel.

Page 4-28, first and second full paragraphs. It is not clear how one can come to the conclusion that the analysis is specific to sequences and to plants when some of the analysis being done are parametric analysis and not actual modeling.

Page 4-35, last full paragraph, puff releases at containment failure are not likely to be largely aerosols. Material which has settled out or plated out or coagulated may be dislodged or ejected but what mechanism exists to disperse it finely enough to generate an aerosol?

Page 4-36, Figure 4.14. It is not clear how the quantity of material leaked to the environment can be almost an order of magnitude greater than what has become airborne. This applies to the time between between 200 and 350 minutes - maybe this is just difficulty in interpretation but it isn't clear.

Page 4-44, top of the page. It is not clear how one can assume that the time of release for everything can be taken as the start of core melting -- even if the containment building airlock doors were wide open. There is a significant time between start of melting and when anything could be released. Secondly, the input by the user of values such as failure pressure or failure temperature, independent of the design, makes this a parametric analysis and not a true risk analysis.

Page 5-5, first paragraph at the top referring to the APS review. While the APS review group might have been broad based and independent, they appear to have only analyzed the NRC funded code work. Therefore it cannot be stated that their study accurately reflects the status of all the source term analytical work and support research. The programs of other countries and the programs of organizations, such as EPRI, do not appear to have been thoroughly reviewed by the APS group.

Page 7-9, second paragraph under Containment Pressure Loads. Some of the hydrogen behavior work done at Sandia is done in shock tubes and care must be taken in assuming such data is relevant to actual containment building conditions. Sandia people involved with this program have stated that the work was not directly relevant but was done only to get a fundamental understanding of the hydrogen ignition process.

Page 8-7, Recommendation 2. Since the computer codes that comprised the code package were reviewed only in the context of severe accidents that completely melt the core, no recommendation is made regarding analysis of lesser severity. Since the complete core melting does not necessarily arise from the code itself but from either input assumptions as to when and how failure occurs or by parametric inputs, it is not clear what this means.

Page B-8. The arbitrary assignment of values to verbal descriptors is entirely arbitrary. The range from unlikely to certain has been given a factor of 10 in two cases and 100 in 2 other cases. It would be no more arbitrary to differentiate certain from unlikely by at least four orders of magnitude.

Page B-9, last paragraph. It is not at all clear why the opinions of a "reactor safety community" have any special validity on a subject that is technical and somewhat narrow in scope. These opinions might be more meaningful if the opinions of the people who are structural/mechanical experts were separated from the group of people who are analytical/code/mathematician/computer experts (and not necessarily experts in questions of structural mechanics).

Page B-15. The discussion under Surry, first paragraph, raises a very interesting question. The implication is that the building spray system reduces safety because it increases the probability of a hydrogen burn. -- such a burn being one of the few residual risks left. Therefore, one has to ask -- is the spray system a safety item or does it increase the risk?

Page B-15, under Surry, the assumption that a meltdown is coherent is not technically supported. Every operating reactor has a significant range of power densities, has a range of cooling factors, has a range of heat sink availability, etc. To assume it is coherent is the same assumption that for some years prevented the boiling water reactor from being developed. The argument made by many of the reactor safety community at the time of the original safety reviews of the BWR was that all of the steam bubbles might collapse coherently and the reactivity added would be enough to blow up the plant. In fact, page C.1-5 identifies that core melting incoherencies is widely recognized.

Page B-17, second paragraph, under Sequoia Discussion. It is hard to understand the assumption of a 50 percent probability of containment building failure. This seems to conflict with all the discussions on containment integrity.

Page C.1-5, last paragraph. Why were the standard problems not structured to be best estimates rather than perhaps high by several orders of magnitude?

Page C.1-6, under Probablistic Aspects, the conversion from analyses that are parametric to a statement that this represents the uncertainty range is questionable. The parametric range is absolutely arbitrary and does not necessarily reflect any technical uncertainty.

Page C.1-7, Results for SP 1. Since it does not appear that particle size or reaction rates have been taken into account, it isn't clear how the results can have "negligible uncertainty". The role of the passive heat capacity of the thousands of tons of concrete, steel and other materials must certainly play a part -- a part that varies depending upon rate of heat release.

Page C.2-3, the third paragraph. This identifies that the capability pressure has been assumed to be the catastrophic failure pressure and that 1 percent tendon strain in a prestressed concrete containment has been assumed to cause catastrophic failure. The actual experimental work done on the HTGR tests, etc., would indicate that this is very far from the real physical world. Not only is there no evidence that catastrophic failure occurs for reinforced concrete structures at this pressure -- there is in fact no evidence that reinforced concrete structures of this type can be catastrophically failed at any pressure. All the evidence indicates that at some strain, concrete cracks, relieves the pressure buildup, and then closes up under the tensions of the steel.