Letters to the Editor

Comments on "An Assessment of Steam-Explosion-Induced Containment Failure. Parts I–IV"

Theofanous et al.[1] address the probability that a steam explosion occurs during a core melt accident and leads to containment failure—the alpha-mode failure issue. This is an important and timely question. The conclusions of these papers[1] will affect the public's perception of this risk, severe accident regulations, proposed mitigation systems for reducing overall risk, and the priority of additional research required to resolve remaining questions. I believe that some aspects of the study provide significant advancements over previous work. However, I also conclude that the authors[1] have incorrectly interpreted or presented some of their *own* work as well as the work of others, especially with respect to predictions of failure probabilities, coarse mixing, and the effects of venting on explosion dynamics; the modeling uncertainties in the papers are inadequately represented and discussed; the sensitivities of the final probabilities to reasonable and plausible changes in the underlying models have not been investigated in a meaningful way; the interim guidance suggested by an author of Ref. 1 could result in implementing regulations that contribute to an *increase* in actual risk, rather than a decrease; and the bottom-line alpha-mode failure probabilities are misleading. These conclusions are developed and substantiated in the following comments.

INTRODUCTION

The four papers in Ref. 1 can be characterized as "policy science." Policy-science papers have appeared in many areas of current technology controversies such as Strategic Defense Initiative feasibility and medical practices, as well as reactor safety issues. The goal of policy science is to provide an improved basis for decision making in the face of large uncertainties. One hopes that the conclusions of policy science will ultimately be confirmed by experience and by the "hard science" of experiments and validated models. Hence, policy science is an interim approach in lieu of the time and effort required to reduce uncertainties to more acceptable levels. The quality of guidance provided by policy science depends strongly on the quality of the physical premises and logical extrapolations of an inadequate data base. Perhaps more so than hard science, the assumptions, models, and logic of policy-science papers must be carefully and critically reviewed. Both my comments and Ref. 1 contain many subjective judgments, and the main issue is not one of *subjectivity versus objectivity*; judgment is involved in all human endeavors. Rather, the readers and the policy makers must decide whether *particular* judgments and their *rationale* are supported by the evidence, and whether the potential for large consequences is adequately offset by low subjective probabilities.

Most persons, experts or not, tend to subjectively predict future events in accordance with their desires. A prophecy of the probability of an undesirable outcome will reflect the biases of the prophet. The National Aeronautics and Space Administration estimated that the probability of a shuttle accident like the *Challenger* disaster was 1 in 100 000 (Ref. 2). Three days before the Japanese attacked Pearl Harbor, the Secretary of the Navy said, "No matter what happens, the U.S. Navy is not going to be caught napping."[3] Policy-science studies are susceptible to the high "probability" that the majority opinion is false, as supported by historical evidence (e.g., see Refs. 4 and 5) and more recent scientific studies (e.g., see Refs. 6, 7, and 8). For subjective opinions to have any value, they must not become simply popularity contests.

Three policy-science studies have addressed the question of alpha-mode failure.[1,9-11] Because of some concerns with the models, assumptions, and probability distributions employed in the first study,[9] and to account for more recent experimental data, Berman et al.[10] and Berman[11] readdressed the failure issue. These authors showed that low probabilities *would* result if some current hypotheses and beliefs were ultimately validated; however, in the absence of such experimental validation, there was no technical basis for an uncertainty range less than 0 to 1. This conclusion was also supported by another independent study by Rivard et al.[12] Reference 1 implies a low probability based primarily on assumptions concerning fuel-coolant mixing processes and on the explosion dynamics. These Ref. 1 assumptions are critically reviewed in the following comments. [Additional technical discussions can be found in four letters to Theofanous et al. and the U.S. Nuclear Regulatory Commission (NRC) program manager.[13-16]]

SUMMARY AND CONCLUSIONS

After careful consideration, it is obvious to me that this study confirms the conclusions of many previous studies that the probability of alpha-mode failure is highly uncertain and indeed could be very large. This is true despite the fact that the bulk of the discussion in Ref. 1 claims the contrary. My conclusions are based on the following facts and observations, which are discussed further herein.

1. The "bottom-line result" understates the actual calculated frequency by a factor of 1000 (0.1 rather than 0.0001).

2. The first of two sensitivity calculations in Ref. 1 is irrelevant since (a) it treats stratified mass only for small failure areas where it cannot influence the calculated failure probabilities and (b) it does not add this mass to the existing premixture.

3. The second sensitivity calculation in Ref. 1 addresses the real and significant probability that the effects of lower plenum venting are grossly exaggerated in Ref. 1. This calculation predicts a probability of 0.2 that the failure frequency is >0.1, and a probability of 0.18 that failure is certain (frequency equals one).

4. Alpha-mode failure is a threshold phenomenon.[9-12,17] Very small changes in missile energy (~10%) can change the failure probability almost seven orders of magnitude. Hence, uncertainties in the underlying assumptions can produce drastic changes in the final probabilities.

5. The review in Ref. 1 of an earlier probabilistic study[10] is incorrect and misleading.

6. Reference 1 faults an earlier mixing study with SIMMER (Refs. 18 and 19) for limitations that similarly affect the K-FIX calculations[1]; namely, both codes are limited to only two fields and subject to numerical diffusion due to their Eulerian nature. (However, Bohl and Butler[18] and Bohl[19] combined the water and fuel into one field, while Theofanous et al.[1] combined water and steam into a single field.)

7. Experiments on water-air mixing are presented without sufficient data for their analysis, without a scaling rationale, and without any arguments establishing their relevance to reactor accidents or to their use in any of the analysis.

8. Reference 1 questions the applicability of calculations by Bankoff and Han[20] and Bankoff and Hadid[21] because of assumptions concerning ambient pressure, degree of venting, and inlet fuel fraction. However, all of these assumptions are as valid (or invalid) as those in Ref. 1 for some accident scenarios, and neither study has necessarily addressed a significant fraction of the possible scenarios.

9. There is no evidence, experimental or theoretical, that any of the probability density functions (pdf's) or causal relations in this study are conservative. There is also the distinct possibility that a seemingly "conservative" choice of a subissue will lead to a nonconservative result because of unanticipated coupling of different phenomena.

10. This study is limited to accidents at 1-bar ambient pressure, with no multiple or precursor explosions, with melt-water contact occurring in certain geometries only, and with hypothesized conditions concerning contact mode and explosion propagation. These accident scenarios may not correspond to any real accident, nor be representative of even a small fraction of the accident scenarios that might occur.

11. The model for hydrodynamic breakup of large pours has no experimental validation, may be completely incorrect or inapplicable to reactor situations, is not necessarily conservative, and does not treat stratified layers in a conservative manner. The model contains no physics whatever, and hence does not account for any of the important effects of initial and boundary conditions, and thermal, physical, chemical, and scale parameters describing the melt-coolant mixing process.

12. The model for thermal limits may not be applicable to any possible accident scenario, is not necessarily conservative, ignores transient fragmentation processes, treats only uniform-sized particles, and has not been experimentally validated. The authors have not attempted any comparisons of the code with existing experimental data (available, for example, in Refs. 22 through 27).

13. Both the hydrodynamic and thermal mixing models represent modest extensions of previous work by Theofanous and Saito,[28] Bankoff and Han,[20] and Bankoff and Hadid.[21] The minor modifications to previous models are incapable of changing any of the conclusions in previous studies demonstrating large uncertainties, e.g., Refs. 10 and 11.

14. Reference 1 indicates that lower plenum failure results in reductions of slug energies by factors of 6 to 11. Other two-dimensional calculations[10,19] have shown much smaller reductions closer to a factor of 2.

15. Two-dimensional calculations as performed in Ref. 1 necessarily predict a complete "unzipping" of the entire lower plenum and its subsequent downward motion. This assumption greatly exaggerates the benefits of lower plenum failure compared to the more likely asymmetric perforation and minimum venting through a relatively small aperture on the time scale of the explosion.

16. MELPROG calculations[29] and other analyses[30-33] indicate that, in station-blackout accidents, natural circulation will lead to upper head heatup and weakening. A water-filled lower plenum could be much stronger than the upper head, making its failure less likely *vis-à-vis* upper head failure and missile generation.

17. The assumption that the explosion occurs instantaneously and coherently could be very nonconservative.[19]

18. The entries in Table V, Part III, do not sum to the available energy. This indicates the possibility of a numerical or calculational error.

19. This study is incomplete because of a lack of experimental verification of any of the distributions and modeling assumptions.

20. This study is misleading because it lacks a sensitivity study of the key parameters, especially the mass of material participating in the explosion. Such a study is necessary because of the extreme sensitivity of the results to input assumptions and because of the high uncertainty associated with those assumptions. I show in these comments that changes of many orders of magnitude in probabilities and frequencies would ensue from relatively minor changes in some of the underlying inputs and models.

The following discussion provides additional technical details and references to support the 20 concerns listed above. The comments correspond to the general chronology and original section headings of Ref. 1.

PART I: Probabilistic Aspects

The authors state that many previous probabilistic models and mechanistic considerations were complementary and that they "perceived a need to consolidate a common approach." However, many of these models are *contradictory*, especially with respect to underlying subjective probabilities and the models used to justify them. The authors also have failed to develop an acceptable common approach as shown in these comments and other reviews.[11,13-16]

The Ref. 1 statement concerning the "generally prevalent expectation that steam explosions do not pose a significant threat to containment" is misleading. Eight major studies in three countries have produced "upper limit" failure probabilities of either 1.0 or 0.1, many orders of magnitude larger than is implied in this study.[1,10-12,34-39] [Note that Ref. 1 itself provides evidence for an upper limit failure probability of ~0.2, as will be shown in later comments (see Part I, Sec. X); the *implication* of a lower failure frequency is a consequence of the authors' misinterpretation of their own calculations.]

Of equal importance are the published technical criticisms of essentially all opinions, hypotheses, and models that have been used to defend low probabilities.[17,40-47]

IV. POINT ESTIMATE PROBABILISTIC MODELS

Theofanous et al. have reviewed most of the previous studies of alpha-mode failure. Unfortunately, their reviews often tend to misrepresent these earlier works and sometimes criticize them for having the same faults or limitations as the new work reported in Ref. 1. In this section, the study by Bohl and Butler[18] is criticized because of the limited "capability of SIMMER to represent the important phenomena."[1] These limitations include the following: "SIMMER cannot handle three fluid fields;" and ". . . as in any other Eulerian code, SIMMER results are affected by numerical diffusion."[1] However, Theofanous et al. employ the K-FIX code to perform their own mixing calculations. K-FIX is *also* a two-field Eulerian code[48] and is just as subject to numerical diffusion as is SIMMER. In contrast to the Ref. 1 application of K-FIX, the SIMMER calculations were based on a code that has received extensive work on flow regimes, constitutive relations, and mass and momentum exchange models.[49] It also includes models of reactor structures. Finally, and most importantly, it has received extensive assessment in the literature, including assessment against vapor explosion experiments.[18] The lumping of steam and water into one field (in K-FIX) could be as misleading an approximation as lumping fuel and water together (in SIMMER). Experimental data (currently lacking) would distinguish which of these limitations is more important or demonstrate that the limitations can only be overcome with a three-field code. Such a code is under development at Sandia National Laboratories[50] (SNL). The key point here is that Theofanous et al. have not established that their calculations do not have similar limitations as previous work.

VI. DISTRIBUTED PARAMETER MODELS

This section is devoted to a critical review of the previous two similar studies.[9,10] The second study, i.e., Ref. 10, called B-S-W in Ref. 1, is seriously misrepresented. Theofanous et al. incorrectly state that "the B-S-W model rejected both procedures" (i.e., uniform distributions over the full range or triangular distributions). As attested to in Table 1 of B-S-W, p. 35, the *very first calculation* addresses a flat distribution "over its full range of possible variation" for all five variables.[10]

Theofanous et al. misconstrue or misunderstand the objectives and results of B-S-W. The B-S-W study clearly stated its two aims: "to provide an uncertainty estimate for the conditional probability of containment failure by steam explosions . . . [and] to identify important contributors to this uncertainty, in order to provide understanding of the reasons for its magnitude and to indicate what additional information would be needed to reduce it." Furthermore, B-S-W deliberately eschewed selecting a "best-estimate" subjective probability. Neither of these aims is included in the statement of purpose of Ref. 1 (p. 259).

The B-S-W study did not assume that "indeed nothing is known about the behavior. . . ."[1] Similarly, the statement,[1] "Obviously probability models cannot tolerate inputs that purport to reflect complete ignorance," is a misleading characterization of B-S-W.

Theofanous et al. also perform a calculation based on the extrema of the various distributions. The conclusion of that calculation is that containment would fail; this is obviously true but irrelevant for both Ref. 1 and B-S-W.

Although Ref. 1 did not have the same objectives as the B-S-W study, it has not resulted in a narrowing of uncertainty. In fact, the results of Ref. 1 and its two sensitivity calculations show substantial agreement with the uncertainty ranges calculated in B-S-W.

VII. A NEW PROBABILISTIC FRAMEWORK

The definitions of probability levels and the order-of-magnitude *quantization* (not quantification) of subjective probability present both technical and philosophical problems that have been discussed previously.[11] In brief, Theofanous et al. cannot defend their claim (in italic type) that the "likelihood figures . . . are . . . upper bound (conservative) estimates," unless those ranges represent physically determined constraints of nature.[11] Furthermore, restricting probabilities to only orders of magnitude can often accomplish the reverse of the original intent to avoid the appearance of excess certainty; i.e., uncertainties will be understated and excessive multiplications can result in probabilities that are too small.[11]

VIII. QUANTIFICATION

As the authors note, the probability distributions and causal relationships are highly subjective. The authors have not supported these relationships with appropriate experimental data. Rather, support is provided by subjective analogies to idealized situations and calculations and by personal opinions.

The authors variously interpolate between the limits of the different pdf's and causal relationships using uniform, normal, or lognormal distributions. It would be valuable to learn the effects of these arbitrary decisions by conducting sensitivity studies using all flat distributions versus all normal.

Figure 13 of Ref. 1, reproduced here, in my opinion represents the most important judgmental relationship in the probabilistic model, and simultaneously the one with the least

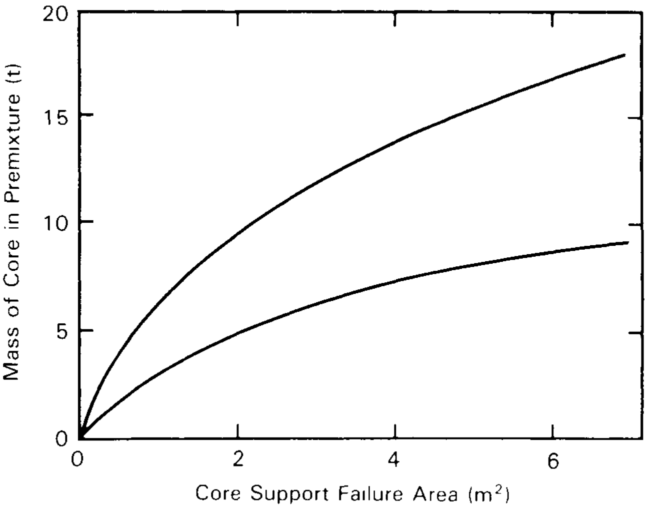

Fig. 13. ID-3. Probabilistic function between failure area and quantity of fuel in premixture. A flat distribution is assumed between the 5 and 95% limit lines (from Ref. 1, Part I).

theoretical or experimental support. Based on current knowledge, these curves in no way represent the 5th and 95th percentiles of the possible mass in the premixture as a function of failure area. Indeed, the true mass in the premixture may be orders of magnitude larger than these curves would allow (see comments on Part II, Sec. VI).

Theofanous et al. claim without citation that Fig. 14, Part I (Ref. 1) (pdf for thermal energy of the fuel) represents the "normally accepted uncertainty range. . . ." There is no such thing as a normally accepted uncertainty range, nor is the particular complex probability distribution shown "normally accepted." Other experts[9,10] claim wider or narrower ranges and different distributions.

Figure 20, Part I (Ref. 1), reproduced here, provides striking evidence of the threshold nature of the alpha-mode failure problem. As Berman pointed out previously[17] (see Fig. R-1), this sharp threshold indicates that the important uncertainty concerns explosion energy rather than failure probability. Based on Fig. 20, a 10-MJ increase in missile energy (~7%) increases the frequency of containment failure a million times from 10^{-7} to 10^{-1}. An increase of ~65 MJ (46%) makes containment failure a *certainty*. This curve shows that the underlying probability distributions and causal functions must be sufficiently accurate to avoid changing the missile energy by even a few percent; otherwise the failure probability cold change by many orders of magnitude from essentially 0 to essentially 1! The authors recognize this extreme sensitivity and believe that it has been addressed by their adoption of a "consistently conservative approach throughout the quantification process." I strongly disagree. Many aspects of the process are not conservatively portrayed in Ref. 1; furthermore, there are other plants, accident scenarios, mixing assumptions, and initial and boundary conditions that have not been adequately addressed in these papers. It is impossible to defend the view that the missile energy could not be 10 or 200% larger than the assumed 95% limit line in Fig. 20 (as discussed, for example, in Refs. 10 through 16, 43, and 47).

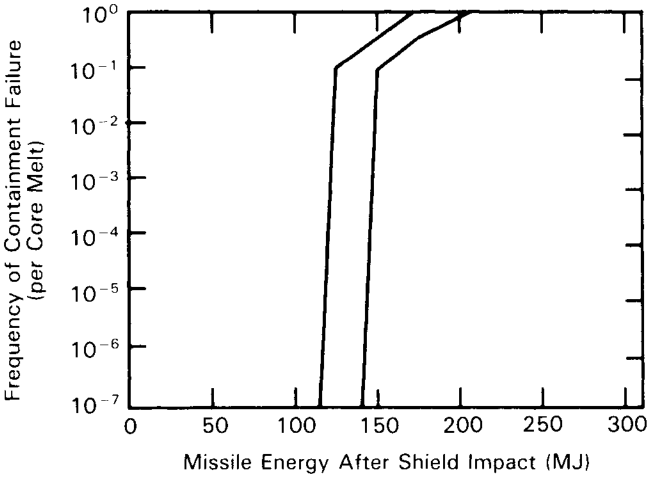

Fig. 20. ID-10. Probabilistic function between frequency of containment failure and missile impact energy. A lognormal distribution is assumed between the 5 and 95% limit lines (from Ref. 1, Part I).

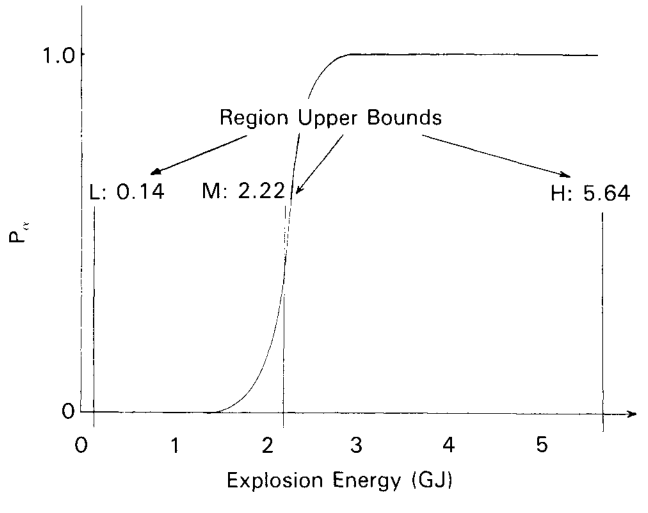

Fig. R-1. Possible dependence of P_α on steam explosion energy (from Ref. 17).

This threshold behavior provides further evidence for the essential need for conducting sensitivity and uncertainty studies in Ref. 1. It is vital to know how uncertainties in subjective assumptions concerning probability distributions and causal relations *propagate* through the probabilistic model and influence the final failure estimates.

Stringent accuracy requirements are not required for all input relations. Indeed, a sensitivity study would show which of the underlying assumptions have the most influence on missile energies. A convincing technical argument for low failure probability would have to show that we are far enough below the failure threshold to provide an adequate margin for safety and regulation.

IX. INTEGRATION PROCEDURES

It is important to recognize that for all discrete probabilities, the data sum, rather than integrate, to one. This section also states that the rebinning procedure was set "to keep output distributions at ~100 nodes." Nevertheless, the actual output distributions plotted in Sec. X contain far fewer than 100 nodes.

X. RESULTS

The probabilistic model in Ref. 1 does not simply supply a subjective probability for alpha-mode failure, as in all previous studies.[9-12] Rather, an infinite population of core melt accidents is imagined, of which some fraction results in such failures. There must be only one such fraction, or *frequency*, but its value is and must always be unknown. The authors then produce a subjective probability distribution quantifying their degrees of belief that the true frequency will take some value over the range of 0 to 1. This complex and highly subjective approach makes it difficult to compare the results of Ref. 1 to earlier studies. Berman[11] has defined and distinguished between two types of subjective probability based on a degree of belief that an event either will or will not occur, P_{sc}, or based on a prophecy, P_{sf}, of a frequency that would be measured if a large population of events could be sampled. Theofanous et al. have chosen to present their results in a form that treats both of these types as complex functions of each other.

Recognizing the difficulty involved in comparing previous single subjective probability estimates to multivalued probabilities of probabilities (i.e., frequencies), it is nevertheless essential to interpret the authors' conclusions in the light of earlier studies. The authors'[1] "bottom-line result" is that 1.3×10^{-4} "is the total probability of α-failure events with frequency $>10^{-4}$." This is a misleading representation of the actual calculation. Consider Fig. 32 of Part I (Ref. 1), reproduced here. There are no data points shown between frequencies of 10^{-4} and 10^{-1}! Hence, the calculations actually say that *essentially* the same total probability ($\sim 1.3 \times 10^{-4}$) applies to a frequency >0.1, 1000 times larger than the stated frequency, based on the same calculations! [It is possible that points exist between frequencies of 10^{-4} and 10^{-1}, but are not shown because they have probabilities $<10^{-6}$, the cutoff in Fig. 32. Of the decades that contain data, the number of bins per decade varies from 1 to 6. Assuming the largest number of bins per decade (6) occurs in the apparently "empty" interval, and the maximum probability per bin of 10^{-6}, the total contribution could not exceed $6 \times 3 \times 10^{-6}$, or 1.8×10^{-5}. Hence, the maximum possible contribution from the "invisible" interval could only change the probability from 1.3×10^{-4} to $\sim 1.1 \times 10^{-4}$; the *actual* change is probably insignificant.]

Strictly speaking, both interpretations are "correct." However, the situation can be compared to saying that an average man is taller than 1 mm, or taller than 5 ft. To properly assess the impact of this calculation on risk estimation, it is *mandatory* that the higher frequency (0.1) be used in safety evaluations, not the lower.

The correctly interpreted bottom-line result is now seen to be quite similar to the full-range and central estimates of failure probability determined in the previous study by Berman et al.,[10] which yielded failure probabilities of 4.6×10^{-2} and 10^{-4}, respectively.

The assignation of the words "physically unreasonable" to probabilities of 10^{-3} represents only the authors' subjective concepts and has no connection whatsoever with actual "physical unreasonableness."

Only two sensitivity results are shown. As I will discuss in Part II, the sensitivity study on the premixing limitation is irrelevant. Additional premixture mass was inserted in such a way that it could not influence the results.

The sensitivity study on energy losses due to lower plenum failure is very informative. The authors state that this calculation results in "an increase in probability by about two orders of magnitude." This is an incomplete and incorrect representation of the calculation. Figure 34, Part I (Ref. 1), is reproduced here. Note again that no data are plotted (above the figure's cutoff probability of 10^{-3}) for frequencies between 10^{-7} and 10^{-1}. Summing probabilities between 0.1 and 1 yields a probability of 0.2 of a failure frequency >0.1. (My calculations are based on estimates from reading the figure.) Even more enlightening is the high probability of ~0.18 that containment failure is certain (frequency = 1). [For this case, the complex probability/frequency function can be reasonably approximated by a bivalued function for frequencies of 10^{-7} (i.e., essentially 0) and 1. Only a single probability number is needed, and the results could be restated as "the subjective probability of containment failure is about 0.2."] Hence, a reasonable interpretation of this calculation would conclude that the probability of containment failure is 1000 to 10 000 times higher for frequencies that are also 1000 to 10 000 times higher than the bottom-line results discussed in Ref. 1. Furthermore, there is a very high probability of certain failure.

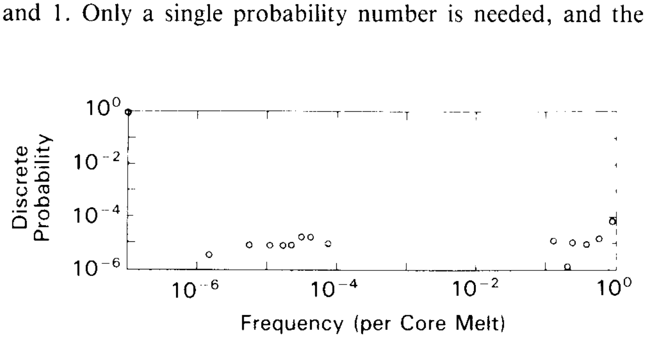

Fig. 32. Probability distribution of containment failure frequency (conditional on major core melt) (from Ref. 1, Part I).

Table I summarizes the differences between the authors' interpretations of their calculations and mine, based primarily on the zero probability of having frequencies between 10^{-4} and 10^{-1} for the base case, and frequencies between 10^{-7} and 10^{-1} for the "parametric" study.

PART II. Premixing Limits

The concept of limits to mixing is crucial to the results of this study. It is qualitatively self-evident that steam explosion energetics would be severely limited when the mixture is excessively fuel or water rich. However, an accurate assessment of the threat from steam explosions requires a *quantitative* assessment of the amounts of fuel, water, and steam that could participate in prototypical explosions. Unverified hypotheses and some computer calculations are used to subjectively justify the causal relations shown in Fig. 21 of Part II (same as Fig. 13 in Part I). The following discussion argues that the assumed 5 and 95% lines drawn in Fig. 21 *do not* represent conservative or even realistic estimates of the amount of mass that could participate in an explosion.

Theofanous et al. state that "the issues of transient and two-dimensional effects on fuel-coolant mixing in the lower plenum of a pressurized water reactor are addressed and resolved." This claim is exaggerated and clearly false. The mixing was very

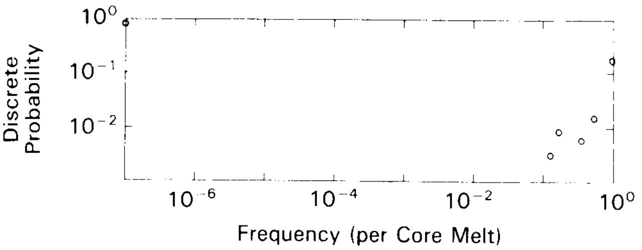

Fig. 34. Result of sensitivity study on energy losses due to lower plenum failure (from Ref. 1, Part I).

TABLE I

Interpretations of Ref. 1 Probability Calculations

Calculation	Theofanous et al.	Berman
Bottom-line		
Probability of frequency $> 10^{-4}$	$\sim 10^{-4}$	NA[a]
Probability of frequency $> 10^{-1}$	?	$\sim 10^{-4}$
Parametric		
Probability of frequency $> 10^{-4}$	$\sim 10^{-2}$ (?)	NA
Probability of frequency $> 10^{-1}$	?	0.2
Probability of frequency = 1	?	0.18

[a] Not appropriate, misleading, or incorrect.

incompletely addressed by a highly simplified geometric model using a computer code that is unable to calculate dynamic and three-dimensional fragmentation processes, cannot treat slip between steam and water, and has not been experimentally validated for this reactor application. Furthermore, the code is not capable of determining the fraction of the mass that can actually participate in an explosion or the potential contributions to the explosion of stratified layers of melt, and does not calculate the explosion propagation or expansion phases. This paper by no means resolves the questions of transient two-dimensional fuel-coolant mixing.

II. SOME GEOMETRIC AND PHYSICAL CONSIDERATIONS

The authors state without proof that "favorable premixtures can be obtained only during the initial transient penetration and prior to significant accumulation of melt into the lower head." This is not true. References 16 and 47 discuss the potential for accumulating large amounts of melt on horizontal surfaces in the lower plenum, and the subsequent participation of this melt in a steam explosion. The occurrence of steam explosions in stratified layers has been observed frequently in experimental studies[51,52] and in a major industrial accident.[53]

III. HYDRODYNAMIC BREAKUP OF LARGE COHERENT POURS

This section presents a discussion of large-scale pours of water through air onto a concrete slab, claimed to be "a qualitative demonstration of phenomena associated with the coherent release of large-scale, gravity-driven jets. . . ." The reader is not told the scaling rationale, the pour height (and hence the length-to-diameter ratio), the definition of "breakup," or the breakup observed for long pour lengths. We *are* told that the "most interesting aspect of these tests . . . was the violent splashing associated with its impact with the ground." There is no discussion (or testing) of the effects of intervening multiple perforated structures present in the lower plenum and discussed in this same section. We are informed that strong impacts can cause triggering, but the potential for impacts to greatly enhance the premixing process is claimed, without citation or proof, to be "limited." Obviously, these tests provide no useful information with respect to the basic physics of fuel-coolant jet mixing, nor can they in any way be extrapolated to questions concerning alpha-mode failure. They do not contribute to either the qualitative or quantitative understanding of reactor issues. Indeed, they could be very misleading since there is no evidence at all that large-scale pours of molten fuel into water resemble large-scale pours of water through air! Compared to molten fuel and water, the density ratio in these simple tests is more than two orders of magnitude too high for water/air. Pilch[54] has indicated that this is the most important dimensionless group to scale in the absence of boiling. That would imply that water/air is a very bad pair of fluids for simulating corium/water interactions. In addition, steam generation may dominate the mixing process, but is totally absent in these isothermal tests.

A far more relevant set of boiling and isothermal jet mixing experiments was initiated at SNL (Refs. 22 and 23). Preliminary results showed strongly nonlinear mixing behavior, effects of water temperature, and important differences between boiling and hydrodynamic effects. Figure R-2 compares the mixture region profiles between two particular experiments. The isothermal experiment involved the mixing of Freon and water at the same temperatures; the boiling experiment involved the mixing of molten iron-alumina with water. The profiles show very different behaviors for these two liquid-liquid systems. The behavior of the water-air system, discussed in Ref. 1, would seem to miss essentially all of the features of the fragmentation and mixing of a hot, dense, boiling liquid penetrating water. Indeed, many experiments have shown that neither of the two mixing models used in Ref. 1 (and discussed below) may represent reasonable approximations of the complex mixing and fragmentation processes of boiling liquids.[22,23,27]

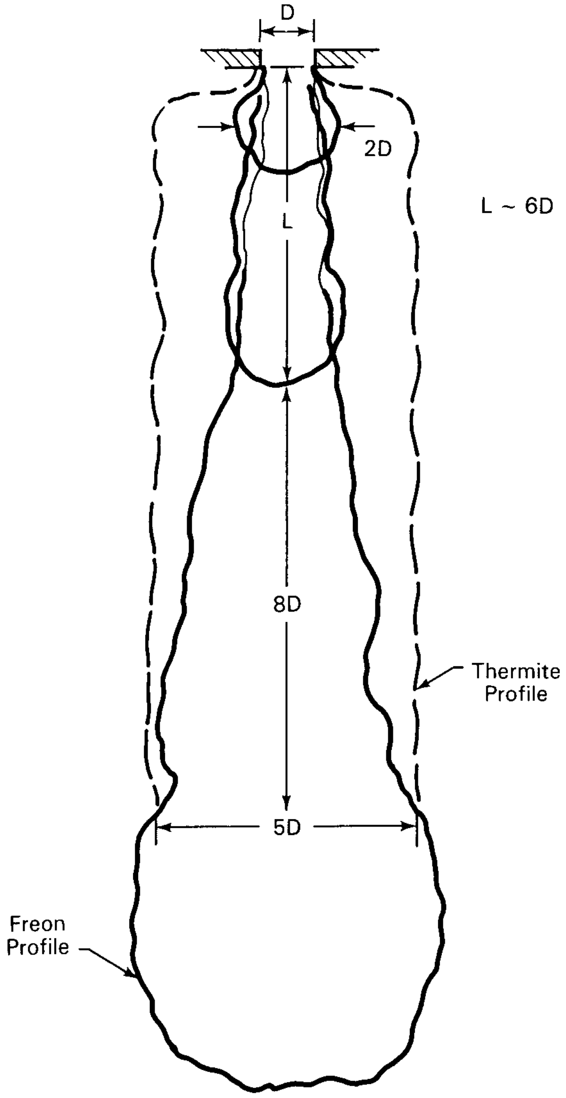

Fig. R-2. Isothermal and boiling jet mixing; comparative sketch of thermite versus Freon-TF in water.

The sketch in Fig. 7, Part II (Ref. 1), reproduced here, illustrates the Theofanous-Saito[28] mixing idea. The text claims that a depth of ~1 m is an upper limit.[1] This is not true. For some reactors, the distance from the bottom of the core to the top of the core support plate is much larger, possibly 1.5 to 2 m. This difference of 50 to 100% is very important, compared to the threshold uncertainties. Furthermore, the mixing concept can be applied to stage C as well as stage B or A [see Fig. 5, Part II (Ref. 1) (reproduced here)]; hence, a mixing depth of 3 m is possible. Distinct or merged jets could emerge as the melt pours

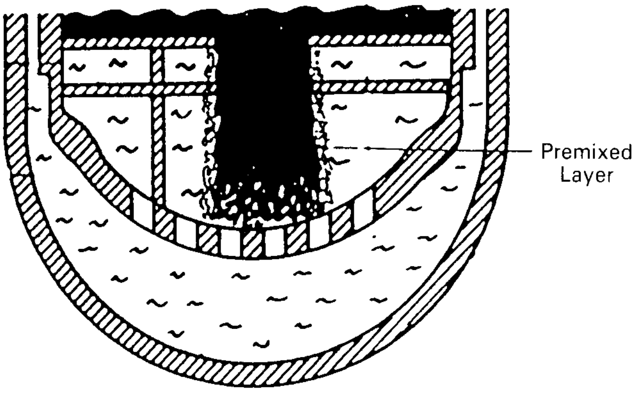

Fig. 7. The Theofanous-Saito premixing concept (from Ref. 1, Part II).

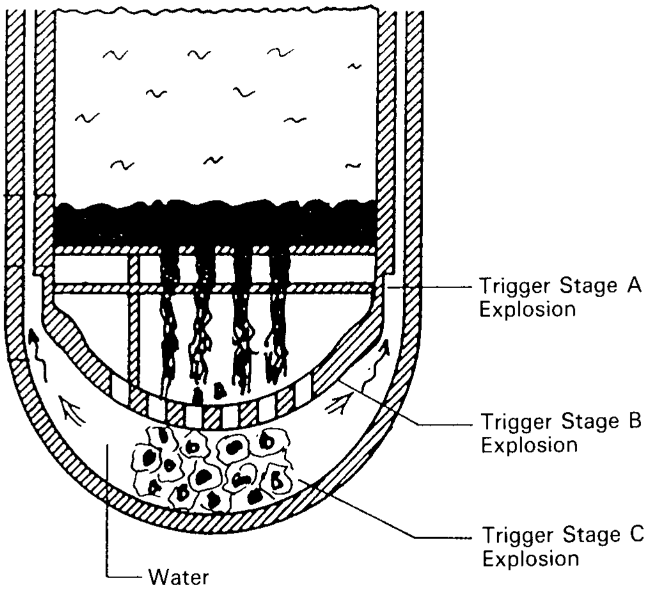

Fig. 5. Illustration of various potential explosion trigger locations as dictated by the lower plenum geometry (from Ref. 1, Part II).

through the holes in this plate. There is no reason to assume that either pure jets or pure particulate streams occur at any time during the accident, or at any stage or elevation. The most likely scenario may be that both partially and fully mixed jets will occur, with neither the hydrodynamic model nor the particulate-based K-FIX model being appropriate. In addition, layers of fuel may reside on the horizontal surfaces of the various plates and ultimately contribute to the total mass in the premixture. Splashing of the melt streams on contact with any plate could also significantly enhance the degree of mixing.

The speculation that a trigger occurs on impact is simply a speculation. Based on current experimental data, triggering is a complex phenomenon dependent on fuel and coolant thermophysical properties, geometry, pour velocity, contact mode, ambient pressure, and possibly other parameters such as melt viscosity, surface tension, wettability of plate surfaces, etc.[55]

No reliable models exist for predicting the occurrence or absence of a trigger or the nature of the premixing in prototypical geometries at large scale. In particular, there are no experimental data that quantitatively or even conservatively support the Theofanous-Saito mixing concept or triggering assumptions for reactor applications.

IV. THERMAL LIMITS FOR PREFRAGMENTED POURS

The authors review earlier mixing calculations by Bankoff and Han[20] and Bankoff and Hadid.[21] Theofanous et al. conclude that "these results indicate *no limitation to premixing*" (original emphasis). "In a subsequent communication,[56] an ideal thermodynamic conversion . . . yielded ~7000 MJ, indicating both a rather massive premixture and a potentially significant challenge to containment integrity."[1] Surprisingly, Theofanous et al. claim that these findings are "in strong contrast to all previous results." This is not true. The findings fall within the uncertainty range discussed in many studies[10,12,36]; they are not unreasonable based on qualitative extrapolations from many experiments conducted at SNL (Refs. 22, 57, and 58).

The authors question the applicability of the Bankoff-Han[20] and Bankoff-Hadid[21] calculations in three areas: initial pressure, degree of venting, and initial fuel fraction. I disagree with all these objections. The assumption of 10-bar initial pressure is very reasonable and should approximate some fraction of the anticipated accident scenarios. It is unreasonable to use the adjective "high" when referring to 10 bars compared to an operating pressure of ~160 bars. Indeed, the main calculations in Ref. 1 were conducted at a pressure of 1 bar (called "low"). Essentially *all* accidents will occur at pressures >1 bar; hence, this limitation could exclude all possible core melt accidents.

The question of venting depends again on the analysts' speculations concerning the accident conditions. In Figs. 5, 6, and 7 of Part II (Ref. 1) (reproduced here) and in all the calculations in Ref. 1, Theofanous et al. have assumed that the molten core above the explosion zone has spread out over the entire cross-sectional area of the core, thereby preventing any venting up through the core region; only downcomer venting is treated. Figures R-3 and R-4 have been taken from Ref. 59 and illustrate a recent picture of the way large masses of melt accumulated and poured into the lower plenum during the Three Mile Island (TMI) accident. Even more recently, it is believed that additional melt also poured through the bottom of the crust and down through the central region of the plenum. Clearly, venting up through the core is possible. Indeed, the TMI scenario, and the calculational models of both Bankoff and coworkers and Theofanous et al. are probably all possible and together represent some subset of all the possible melt and

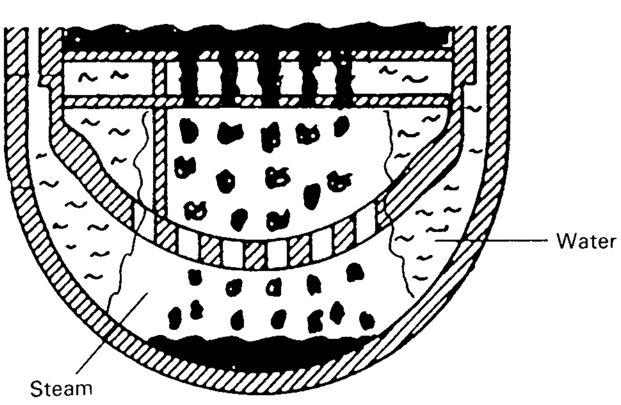

Fig. 6. Illustration of premixing regime after melt accumulation on the lower head (from Ref. 1, Part II).

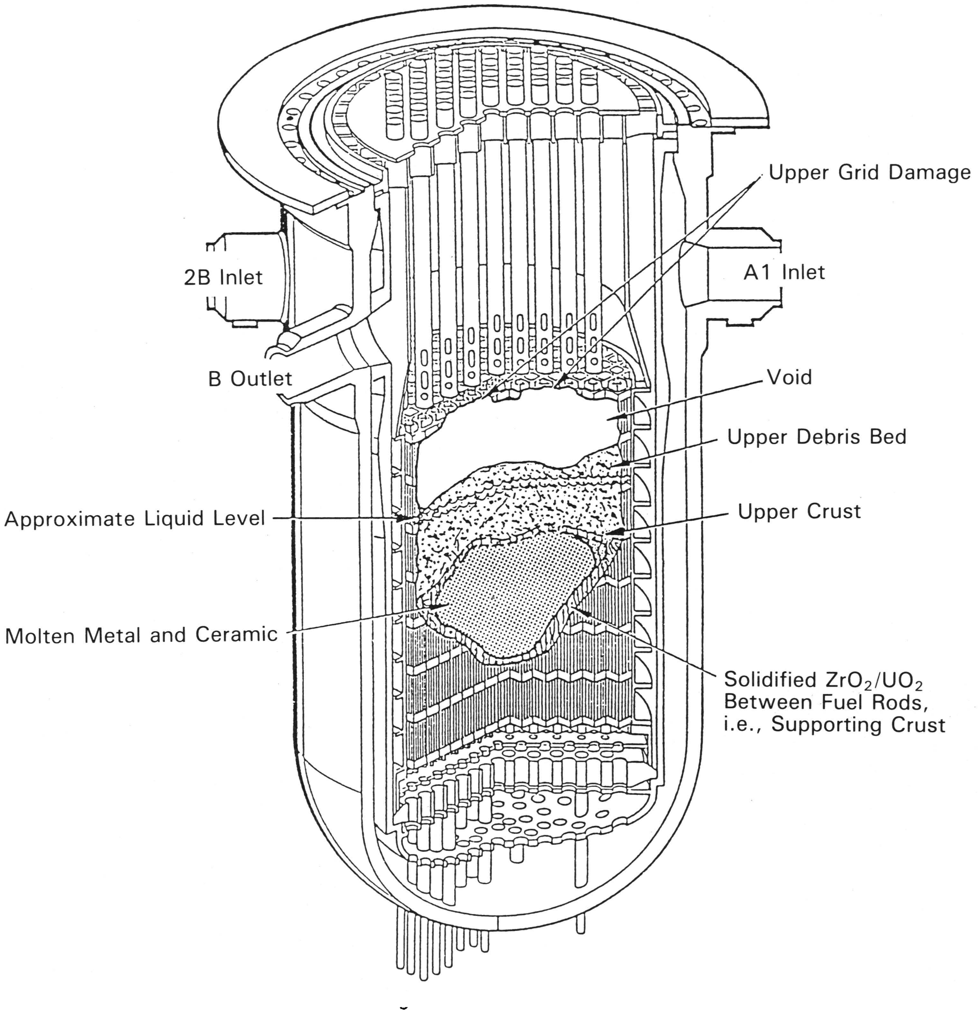

Fig. R-3. Hypothesized core damage configuration (224 min) (from Ref. 59).

pour configurations that could occur. I conclude that the objections concerning venting are not valid.

Finally, Theofanous et al. claim that the fuel fraction "is too low compared to the dense streaming envisioned in the present application." The assumed fuel fractions may indeed be *different* between the two studies, but the "present application"[1] may not represent a significant fraction of all possible scenarios, nor does it exclude the applications investigated in earlier studies.

The authors summarize their review of the calculations of Bankoff and coworkers with the statement that "we cannot reach any conclusions regarding premixing limitations in pressurized water reactor . . . geometries and low pressures." This is not correct, since Theofanous et al. *have already drawn* some conclusions themselves: For clearly plausible and perhaps likely assumptions concerning initial pressures and geometries, the model of Bankoff and coworkers predicts "no limits to mixing" and "a potentially significant challenge to containment integrity" (quotes taken directly from Ref. 1).

V. FURTHER CONSIDERATION OF THERMAL LIMITS

The description here of the mathematical model appears to be a statement of the K-FIX code equations,[48] which are not referenced until much later. Theofanous et al. should carefully distinguish between the existing K-FIX model and their own contributions to this model as applied to premixing calculations. Furthermore, some terms and subscripts are not defined prior to presenting the equations; the value of ρ is not given. The choice of various dimensions is not justified; for example, why was a depth of 1.7 m chosen (or 1 m in other parts of Ref. 1), rather than 2 or 3 m? Recall that a 10% change in missile energy can lead to a change of six or seven orders of magnitude in failure probability. Hence, a 20 to 80% change in mixing depth could have a very large and disproportionate effect. Similarly, why was a diameter of 4.4 m used rather than the 2 m used by Bankoff and coworkers or the 3.4-m diameter used by Berman[11] or the variety of core and lower plenum diameters of different reactors? A sensitivity study is required to analyze the effects and relative importance of these geometric assumptions. Similarly, other important parameters should be investigated, including initial pressure, particle sizes, etc.

The authors provide a caveat at the end of this section: "Clearly, at this stage, these are only analytical results in need of experimental verification." However, they have proposed experiments aimed primarily at verification of the calculation of the particular scenario addressed; i.e., the raining of a collection of hot spheres of fixed diameter into a water pool of a

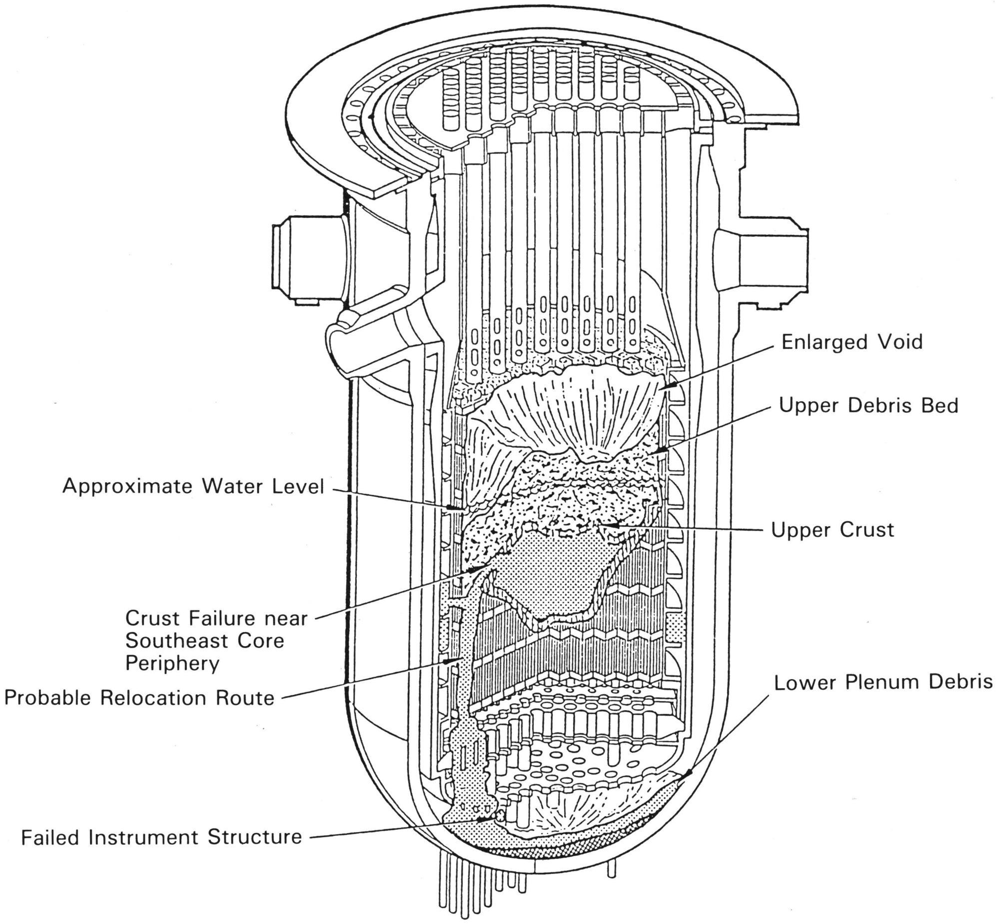

Fig. R-4. Hypothesized core damage configuration (226 min) (from Ref. 59).

single size, shape, and venting configuration.[60] In fact, experiments of this type have already been conducted at Brookhaven National Laboratory.[24–26] The area that actually needs experimentation far more critically concerns how well the calculational model represents accident environments that might actually occur. This question requires experimental confirmation of a much broader set of phenomena including jet fragmentation as well as mixing; the influence of structures, confinement, and scale; and the effects of ambient pressure, fuel temperature and composition, and water temperature. Experimental confirmation of a calculation that itself may be unrelated to any reactor accident scenario would be a waste of scarce research funds.

VI. CONCLUDING REMARKS

A great many assumptions and approximations are embedded in the two curves in Fig. 21 in Part II (Fig. 13 in Part I). I believe that Theofanous et al. have greatly underestimated both the complexity and the uncertainty in melt mass as a function of flow area. For example, Fig. R-4 shows that pours may resemble neither a shower of droplets nor a large central coherent pour. My previous discussion has shown the limited value of the K-FIX calculations and has indicated that they may not represent any reactor accident situation. I will now address the fundamental technical weaknesses of the smooth curves representing the 5 and 95% limits and show why these curves are nonconservative.

The essence of the Theofanous-Saito model[28] is the extremely simple (and unproven) concept that mass within a skin depth of 10 cm from the interface is fully in the premixture, and that deeper mass does not participate at all (see Part II, Sec. III). For example, cylinders of 10-cm or less radius are fully in the premixture; for larger cylinders, only the outer annulus to a depth of 10 cm participates in the explosion. Theofanous et al. now state that "the 5% line was taken to represent the trend of the [K-FIX] calculations while the 95% line was drawn to provide a margin for uncertainty, roughly by a factor of 2."[1] However, these same curves were developed in an earlier version of Ref. 1 based directly on the Theofanous-Saito model for an assumed water depth of 1.5 m (5% limit) and 3 m (95% limit). These depths are much more reasonable than the 1-m depth discussed in this section.

The original curves did not account for the leading edge effect, which would add an additional 10-cm-thick disk (mass = $\rho\delta A$) to the curves. (The current version of Fig. 21 includes this additional mass, but it is now added to the reduced calculation based on 1 m, rather than 1.5 or 3 m.) In the version of Fig. 13′ included here, I have plotted the sum of the leading edge mass and the 3-m-depth (95% limit) curve. However, even this curve does not represent a conservative approach for several reasons. Experiments (albeit at much less than reactor

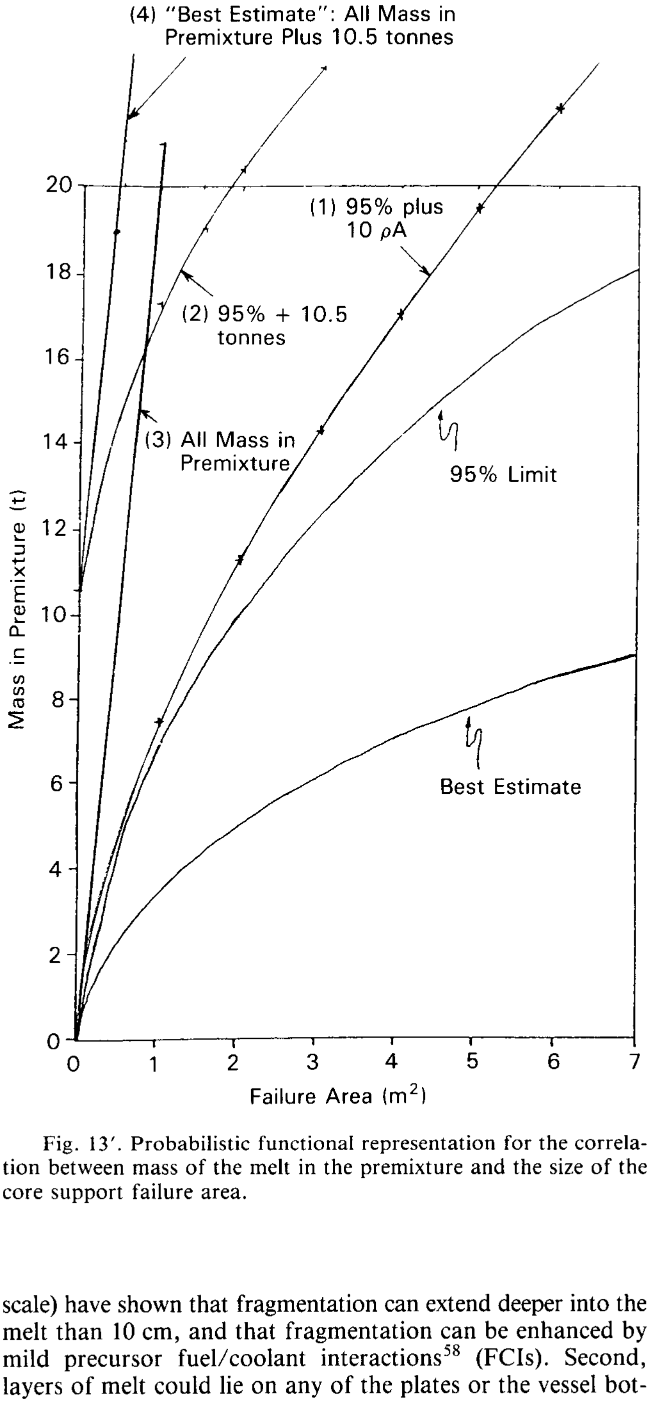

Fig. 13′. Probabilistic functional representation for the correlation between mass of the melt in the premixture and the size of the core support failure area.

scale) have shown that fragmentation can extend deeper into the melt than 10 cm, and that fragmentation can be enhanced by mild precursor fuel/coolant interactions[58] (FCIs). Second, layers of melt could lie on any of the plates or the vessel bottom in thicknesses $\geq$10 cm whenever the explosion occurs. If we nonconservatively ignore fragmentation deeper than 10 cm, but just add a 10-cm surface thickness from a melt pool assumed to be on the plenum floor, then curve 2 results.

Recent TMI findings indicate that lower plenum structures could remain intact as melt pours through.[59] Many experts now believe that the most probable flow regime in the lower plenum is composed of multiple melt streams of initial diameter comparable to the hole diameters of the various plates found in the lower plenum. Essentially *all* these holes are smaller than the critical diameter of 20 cm. Hence, the authors themselves must conclude that all these jets "should be considered fully in premixture." That means that *all* the melt falling through area A will be in the premixture, as shown in Fig. 13′, curve 3. Furthermore, if a layer 10 cm or deeper is on the bottom of the lower plenum at the time of the explosion, then the mass in the premixture must be increased by another 10.5 tonnes, as shown in curve 4. Hence, the "best estimate" for mass in the premixture as a function of initial break area, assuming triggering at the bottom of the lower plenum, may indeed be represented by curve 4.

The simple notion of being "in premixture" actually is not very informative with respect to explosion energetics. Although the melt could be fragmented, the explosion magnitude and efficiency depend on the premixture particle size distribution and the relative masses of fuel, water, and steam in the mixture region. My discussion here emphasizes that neither of the mixing analysis approaches in Ref. 1 is capable of demonstrating any significant limit to mixing. The K-FIX calculations can lead to predictions of either certain or impossible failure, depending on assumed initial conditions and other modeling assumptions. On the other hand, the concept of a 10-cm mixing depth can also lead to very energetic explosions.

To indicate the discrepancy between the authors' conservative limit (based on the mixing depth model from Theofanous and Saito[28] and my "best estimate" based on their model), we can compare a few predictions. For a failure area of 1 m^2, the authors' 5 and 95% masses are 3.7 and 7.4 tonnes, respectively. My "best estimates" are 21 and 31.5 tonnes for assumed water depths of 1.5 and 3 m, respectively. For a failure area of 5.5 m^2 (~50% of the total area according to the authors), the authors' masses are 8.7 and 17.5 tonnes. My "best estimates" are 68.3 and 126 tonnes (or all of the molten fuel and cladding). It is apparent that my estimates exceed the authors' by ~5 to 8 times. (Recall that a change of ~10% in missile energy changed the failure probability by almost seven orders of magnitude.) To my knowledge, there are no experimental data or validated models that can prove or disprove the various relationships I have drawn in Fig. 13′.

In letters to the reviewers, Theofanous et al. wrote: "we . . . invite the participants to provide their own input distributions (with their justification) as alternatives to our own. We agree to run them through the calculation."[61,62] I requested that several distributions and relationships similar to those shown in Fig. 13′ be calculated. Unfortunately, despite the authors' offer to do so, these calculations were not done, so that it is not possible to discuss the impact of different assumptions concerning mixing limitations.

To illustrate the extreme dependence of the authors' failure calculations on their assumed mixing relationship, I will perform a simple calculation. According to Theofanous et al., the most probable failure areas lie between 0 and 3.5 m^2, the most probable melt energy is 1.2 GJ/t, and the most probable conversion ratio is 15%. Using an area of 3.5 m^2 and curve 4 as an alternative best estimate of mixing by multiple jets with a diameter <20 cm, yields an explosion energy of 15.1 GJ, equivalent to more than 3000 kg of TNT; compared to such an explosion, the Chernobyl event was relatively mild. A failure area ten times smaller, i.e., 0.35 m^2, would still yield an explosion energy of 3.2 GJ, more than twice the energy estimated to pose a serious threat to containment.[36] Clearly, the single failure probability produced in this study is of no value without a consideration of the underlying uncertainties.

In the final paragraph in this section, the authors caution the readers concerning the judgment involved in creating their causal relationship (Fig. 21 or 13). They also discuss the need to continue the work and conduct experiments. This paragraph contradicts the incorrect statement presented in the Abstract (Part II) that "the issues of transient and two-dimensional effects on fuel-coolant mixing in the lower plenum of a pressurized water reactor are addressed and resolved."

PART III. Expansion and Energy Partition

This section discusses what I consider to be the second most important (and highly uncertain) assumption in the probabilistic model. It has been addressed before by other analysts, who have drawn different conclusions. In addition, some recent calculations have indicated that the lower head will be much colder [possibly more than 300 K (Ref. 29)] and therefore stronger than the upper head; this casts doubt on the assumption presented in the Abstract of Part III that "explosions that are energetic enough to be considered challenging to upper vessel head integrity would also fail the lower head, thus drastically reducing the upward directed mechanical energy release."

INTRODUCTION

As in other parts, this section begins with a critical review of two earlier works. The partition of slug energy in Berman et al.[10] is mentioned, but there is no subsequent comparison between that model and the predictions of Ref. 1, Part III. However, a comparison can be made rather easily. The simple energy partition model of Ref. 10 always leads to an explosion energy reduction of at least a factor of 2, but generally not much more than a factor of 3.

In Bohl and Butler,[18] and the subsequent study by Bohl,[19] lower head failure was treated in a more sophisticated manner using the two-dimensional SIMMER-II code. Interestingly, the calculated reduction of the force on the upper head due to lower plenum failure ranged from about −14 to −62%, quite close to the simpler calculation in Ref. 10. Theofanous et al. state that "comparisons to the prediction of [Ref. 10] or to the detailed results of the present study are not possible." On the contrary, comparisons can easily be made between the reduction factors of 1.2 to 2.6 determined by Bohl and Butler[18] and Bohl,[19] factors of 2 to 3 determined by Berman et al.,[10] and factors of 6 to 11 generated in Ref. 1.

II. PARAMETERIZATION OF EXPLOSION ENERGETICS

Many approximations are involved in the explosion calculation. Although the explosion zone diameters were reasonably varied from 1 to 3 m, the height was taken to be 3.06 m. This differs from the mixing heights used in the K-FIX calculations in Part II (1 or 1.7 m?). A parameterization of this height would be helpful in understanding its influence on the subsequent calculations. [It actually appears as though no dynamic calculations were performed in this section at all; i.e., only a Hicks-Menzies-type thermodynamic analysis was employed. If so, then neither the explosion zone diameters nor the height are relevant here (although they may be important for the subsequent dynamic fluid-structures calculation in Part III, Sec. III). Only the volume change assumptions would be needed. The authors should more clearly describe what initial and boundary conditions actually influence the results.]

The authors say that the "explosion was assumed to occur instantaneously."[1] Prior to Bohl's work,[19] this would have been considered a reasonable "opinion." Bohl primarily studied single large-scale coherent explosions. However, he also performed a calculation to "simulate an incoherent multiple explosion that could be more representative of the reactor situation."[19] In contradiction to his intuition, the calculation showed that "the most benign explosion produced the largest challenge to the head."[19] This enlightening calculation highlights our current lack of understanding of the explosion phenomenon, both analytically and experimentally. It also emphasizes the strong potential for failing the upper head without a prior failure of the vessel wall in the lower plenum region.

The authors[1] concede that "continued equilibrium between fuel and coolant until expansions of three and four times of initial volume [would increase] explosion yields [by] 14 and 25%. . . ." Surprisingly, they ignore this and conclude that "such effects were not deemed of sufficient importance to pursue further." Since a change in missile energy by ~7% could increase failure probabilities about a million times, effects of 14 or 25% could be very important.

IV. LONG-TERM EXPANSION

The major conclusion of this section is that lower head failure leads to a reduction of fuel slug energy by factors of ~6 to 11. This should be compared to the SIMMER-II reduction factors of 1.2 to 2.6 (Ref. 19) and factors of 2 to 3 reductions calculated by Berman et al.[10] In addition to these differences (and hence uncertainties), other aspects of the analysis deserve scrutiny.

The calculations are two-dimensional. Hence, lower plenum failure as treated in Ref. 1 involves a simultaneous azimuthal tear around the entire circumference of the vessel. This is a nonconservative and probably unlikely mode of failure. It greatly exaggerates the benefits in reducing slug energy both by venting and by the downward motion of the vessel bottom head. Steam explosion experiments almost always show a high degree of noncoherence and nonsymmetry, even for initially symmetrical experimental configurations. In addition, structural failure may be far more likely to occur as a local rip or puncture on the time scale of the explosion (milliseconds). In such a case, venting would be extremely reduced, and there would be no benefits from vessel motion since the vessel bottom head would not move downward. {Converting the upper head into a missile would also involve a circumferential failure of the head flange. This is more likely than a circumferential failure of the lower head because (a) upper head failure is due to slug impact rather than shock waves, and the gun-barrel vessel geometry would tend to load the head more symmetrically; (b) the upper head is connected by bolts at the flange, and circumferential failure could occur as a result of bolt failure [the bolts securing the upper head in the massive Winfrith test facility actually stretched in a steam explosion experiment SUW 09 (Ref. 63)]; and (c) the vessel walls in the region of the head bolts could be preferentially weakened due to heatup from natural convection (as shown, for example, by MELPROG calculations[29] and by RELAP/SCDAP and CORMLT/PSAAC calculations[30-33]).}

Given the extremely uncertain and perhaps absent benefits of lower plenum failure, the sensitivity calculation performed in Part I (Fig. 34) seems very appropriate.

The slug energies and loss terms presented in Table V in Part III (Ref. 1) do not sum up to the initial available energy in any of the cases. This discrepancy may imply a numerical or modeling error that could significantly affect the conclusions of this study.

FINAL COMMENTS

Some persons have hoped that the alpha-mode failure question could be resolved without conducting the hard-science research necessary to accomplish this. Studies and review panels have generated a plethora of subjective opinions concerning high or low probability numbers; but when analyzed carefully, the number of studies and calculations that predicts the potential for *high* failure probabilities now exceeds those that do not.

However, no popularity poll will suffice for resolution of the alpha-mode question, or any of the other important FCI-related issues, because the needed underlying data base and modeling capabilities are highly incomplete and uncertain.

The importance of the Ref. 1 study and previous subjective attempts to resolve the alpha-mode failure issue goes far beyond the particular question of alpha-mode failure. Two other important areas have already been negatively impacted by the unsubstantiated prophecies of low failure probability: the continuation of vital research related to FCIs for issues other than alpha-mode failure and the proposal of mitigation concepts to prevent direct containment heating.

As the authors would agree,[1] Ref. 1 was not intended to resolve the alpha-mode issue; rather, it was meant as a contribution to the continuing debate. However, this study has already been cited as evidence supporting the low priority of additional FCI research by various panels. The effect has already been to severely truncate or terminate the very research required to confirm or reject the models and assumptions in this policy-science study. Such a truncation of research guarantees that optimistic opinion will reign until and unless a subsequent accident provides indisputable contradictory evidence.

Another safety concern involves the potential acceptance of these questionable subjective probabilities and their actual application in decision making that could directly affect real risks. An important example concerns a recent proposal to depressurize the primary system to eliminate the threat from direct containment heating. That threat has also not been adequately studied or quantified, and its perceived magnitude is a result of the same kind of subjective judgment applied in Ref. 1. Experimental data for 20-kg fuel masses unequivocally support the increased probability of spontaneously triggered steam explosions as pressure is reduced. [Eighty-six percent (32 of 37) of fully instrumented test series (FITS) tests at ambient pressure and temperature spontaneously exploded; none (0 of 5) spontaneously exploded at pressures >5 bars.] Based on unverified, possibly inapplicable calculations, Ref. 1 would conclude that high pressure is more conducive to mixing (and hence larger steam explosions) than low. Hence, Theofanous et al. would argue that depressurizing the primary system would reduce risk from both alpha-mode failure and direct containment heating, despite the known increase in probability of *triggering* steam explosions. This may turn out to increase risk, rather than decrease it. The experimental data on steam explosion triggering should be given serious consideration before proceeding with mitigation schemes based on simplistic unvalidated models.

I do not know the "likelihood" of alpha-mode failure. Neither does anyone else. Reference 1 does not provide technically defensible support of a low failure probability. It is possible that additional research will ultimately show that such failures are impossible. It is also possible that research will indicate that such failures may be of concern during core melt accidents.

Marshall Berman

Sandia National Laboratories
Severe Accident Containment Response Division 6427
Albuquerque, New Mexico 87185

February 2, 1988

ACKNOWLEDGMENT

This work was supported by the NRC and performed at SNL, which is operated for the U.S. Department of Energy under contract DE-AC04-76DP-00789.

REFERENCES

1. T. G. THEOFANOUS, B. NAJAFI, and E. RUMBLE, *Nucl. Sci. Eng.*, **97**, 259 (1987); M. A. ABOLFADL and T. G. THEOFANOUS, *Nucl. Sci. Eng.*, **97**, 282 (1987); W. H. AMARASOORIYA and T. G. THEOFANOUS, *Nucl. Sci. Eng.*, **97**, 296 (1987); and G. E. LUCAS, W. H. AMARASOORIYA, and T. G. THEOFANOUS, *Nucl. Sci. Eng.*, **97**, 316 (1987).

2. R. P. FEYNMAN, "Personal Observations on the Reliability of the Shuttle," California Institute of Technology (June 10, 1986); also quoted in *Science*, **232**, 1596 (June 27, 1986).

3. F. KNOX, December 4, 1941, quoted in C. CERF and V. NAVASKY, *The Experts Speak, The Definitive Compendium of Authoritative Misinformation*, Pantheon Books, New York (1984).

4. M. BERMAN, "Vapor Explosions, Physics and Philosophy," presented at the Gordon Conf. High Temperature Chemistry, Wolfeboro, New Hampshire, July 23, 1986.

5. C. CERF and V. NAVASKY, *The Experts Speak, The Definitive Compendium of Authoritative Misinformation*, Pantheon Books, New York (1984).

6. H. F. MARTZ, "Quantification of Informed Opinion," Internal Report No. S-1/84-332, Los Alamos National Laboratory (May 1984).

7. H. F. MARTZ, M. C. BRYSON, and R. A. WALLER, "Eliciting and Aggregating Subjective Judgments—Some Experimental Results," *Proc. 1984 Statistical Symp. National Energy Issues*, Seattle, Washington, October 16-18, 1984, NUREG/CP-0063, U.S. Nuclear Regulatory Commission.

8. R. M. HOGARTH, *J. Am. Stat. Assoc.*, **70**, 271 (1975).

9. D. V. SWENSON and M. L. CORRADINI, "Monte Carlo Analysis of LWR Steam Explosions," NUREG/CR-2307, SAND81-1092, Sandia National Laboratories (Oct. 1981).

10. M. BERMAN, D. V. SWENSON, and A. J. WICKETT, "An Uncertainty Study of PWR Steam Explosions," NUREG/CR-3369, SAND83-1438, Sandia National Laboratories (May 1984).

11. M. BERMAN, *Nucl. Sci. Eng.*, **96**, 173 (1987).

12. J. B. RIVARD et al., "Identification of Severe Accident Uncertainties," NUREG/CR-3440, SAND83-1689, Sandia National Laboratories (Sep. 1984).

13. M. BERMAN, Sandia National Laboratories, Letter to C. ALLEN, U.S. Nuclear Regulatory Commission (Aug. 6, 1986).

14. M. BERMAN, Sandia National Laboratories, Letter to E. RUMBLE, Science Applications International Corporation (Sep. 16, 1986).

15. M. BERMAN, Sandia National Laboratories, Letter to T. G. THEOFANOUS, University of California, Santa Barbara (Nov. 10, 1986).

16. M. BERMAN, Sandia National Laboratories, Letter to T. G. THEOFANOUS, University of California, Santa Barbara (Mar. 23, 1987).

17. M. BERMAN, "The Probability of Alpha-Mode Failure," presented at the Steam Explosion Review Group Mtg., Harper's Ferry, West Virginia, November 27, 1984.

18. W. R. BOHL and T. A. BUTLER, "Comments on Proposed Research Contributing to the Resolution of Residual Steam Explosion

Issues," Letter Report, Steam Explosion Review Group Report, NUREG-1116, U.S. Nuclear Regulatory Commission (Feb. 1985).

19. W. R. BOHL, Los Alamos National Laboratory, "An Investigation of Steam Explosion Loadings with SIMMER-II," Draft Report (1986).

20. S. G. BANKOFF and S. H. HAN, "An Unsteady One-Dimensional Two-Fluid Model for Fuel-Coolant Mixing in an LWR Meltdown Accident," presented at the U.S.-Japan Seminar on Two-Phase Dynamics, Lake Placid, New York, July 29-August 3, 1984.

21. S. G. BANKOFF and A. HADID, "The Application of a User-Friendly Code to Nuclear Thermalhydraulic Safety Problems," presented at the Int. Nuclear Power Plant Thermal Hydraulics and Operations Topl. Mtg., Taipei, Taiwan, October 22-24, 1984.

22. B. W. MARSHALL and M. BERMAN, "Recent Results in FCI Research: Fuel-Coolant Jets," presented at the 14th Water Reactor Safety Research Information Mtg., Gaithersburg, Maryland, October 27-31, 1986.

23. B. W. MARSHALL, Jr., D. F. BECK, and M. BERMAN, "Mixing of Isothermal and Boiling Molten-Core Jets with Water; The Initial Conditions for Energetic FCI," to be presented at the Int. Conf. Thermal Reactor Safety, Avignon, France, October 2-7, 1988.

24. T. GINSBERG et al., "Core Debris Thermal-Hydraulic Phenomenology: Ex-Vessel Debris Quenching," *Safety Research Programs Sponsored by Office of Nuclear Regulatory Research, Quarterly Progress Report January-March 1984*, NUREG/CR-2231, BNL-NUREG-51454, Vol. 4, No. 1, p. 49, Brookhaven National Laboratory (June 1984).

25. G. A. GREENE et al., "BNL Severe Accident Sequence Experiments and Analysis Program," *Proc. 12th Water Reactor Safety Research Information Mtg.*, NUREG/CP-0058, Vol. 3., U.S. Nuclear Regulatory Commission (Jan. 1985).

26. T. GINSBERG, Brookhaven National Laboratory, Private Communication to M. BERMAN, Sandia National Laboratories (Dec. 1987).

27. B. W. SPENCER et al., "Corium Quench in Deep Pool Mixing Experiments," *Proc. 1985 Natl. Heat Transfer Conf.*, Denver, Colorado, August 4-7, 1985, p. 267, American Nuclear Society (1985).

28. T. C. THEOFANOUS and M. SAITO, *Nucl. Eng. Des.*, **66**, 301 (Sep. 1981).

29. J. E. KELLY et al., "MELPROG-PWR/MOD1 Analysis of a TMLB′ Accident Sequence," NUREG/CR-4742, SAND86-2175, Sandia National Laboratories (Jan. 1987).

30. V. E. DENNY, "The CORMLT Code for the Analysis of Degraded Core Accidents," EPRI-NP-3767 CCM, Electric Power Research Institute (1984); see also V. E. DENNY, "The Role of Natural Circulation in Severe Accident Analysis," Preprint, Science Applications International Corporation (1987).

31. P. D. BAYLESS, "Natural Circulation During a Severe Accident: Surry Station Blackout," EGG-SSRE-7858, EG&G Idaho, Inc. (1987).

32. B. R. SEHGAL et al., "Effects of Natural Convection Flows on PWR System Temperatures During Severe Accidents," *Proc. 1985 Natl. Heat Transfer Conf.*, Denver, Colorado, August 4-7, 1985, p. 223, American Nuclear Society (1985).

33. W. J. CAMP, Sandia National Laboratories, Private Communication to M. BERMAN, Sandia National Laboratories (Nov. 1987).

34. N. RASMUSSEN et al., "Reactor Safety Study—An Assessment of Accident Risks in US Commercial Reactor Plants," WASH-1400, NUREG-75/014, U.S. Nuclear Regulatory Commission (Oct. 1979).

35. "Germany, 1979, German Risk Study," Gesellschaft für Reaktorsicherheit (1979); English translation No. 729, U.S. Nuclear Regulatory Commission (May 1980).

36. J. H. GITTUS, Ed., "PWR Degraded Core Analysis," NDR-610(S), Springfields, U.K. (Apr. 1982).

37. Steam Explosion Review Group, "A Review of the Current Understanding of the Potential for Containment Failure Arising from In-Vessel Steam Explosions," NUREG-1116, U.S. Nuclear Regulatory Commission (Feb. 1985).

38. A. S. BENJAMIN et al., "Evaluation of Severe Accident Risks and the Potential for Risk Reduction: Surry Power Station, Unit 1," NUREG/CR-4551, SAND86-1309 (also in Draft NUREG-1150), Sandia National Laboratories (Feb. 1987).

39. A. S. BENJAMIN et al., *Nucl. Eng. Des.*, **104**, 249 (1987).

40. N. E. BUTTERY, "Fragmentation and Mixing Energy Requirements for Steam Explosions," Glos. GL13 9PB, Central Electricity Generating Board, Berkeley Nuclear Laboratories (1981).

41. A. T. CHAMBERLAIN and F. M. PAGE, "An Experimental Examination of the Henry-Fauske Voiding Hypothesis," University of Aston, Birmingham, U.K. (Jan. 1983).

42. A. J. WICKETT, "Review of IDCOR Technical Report 14.1A 'Key Phenomenological Models for Assessing Steam Generation Rates,'" Sandia National Laboratories (Nov. 1983).

43. M. BERMAN, "Comments on IDCOR Report 14.1A, 'Key Phenomenological Models for Assessing Steam Generation Rates,'" presented at the NRC/IDCOR Mtg. Accident Phenomenology and Containment Loading, Harper's Ferry, West Virginia, November 29, 1983.

44. A. T. D. BUTLAND et al., "Report on Phase 1 of the PWR Severe Accident Containment Study," AEEW-R 1842, U.K. Atomic Energy Establishment, Winfrith (Dec. 1984).

45. M. BERMAN, "Commentary on the Deliberations and Conclusions of the Steam Explosion Review Group (SERG)," Memo to J. L. TELFORD, U.S. Nuclear Regulatory Commission (Feb. 28, 1985).

46. A. T. D. BUTLAND et al., "Report on Phase 2 of the PWR Severe Accident Containment Study," Part 2, PWR/SAWG/P(85)183, AEEW-R1964, U.K. Atomic Energy Establishment, Winfrith (Sep. 24, 1985).

47. M. BERMAN, "An Evaluation of the Bases for Estimating Alpha-Mode Failure Probabilities," *Proc. Int. ANS/ENS Topl. Mtg. Thermal Reactor Safety*, San Diego, California, February 2-6, 1986, Vol. 2, p. XI.7, American Nuclear Society (1986).

48. W. C. RIVARD and M. D. TORREY, "K-FIX: A Computer Program for Transient, Two-Dimensional, Two-Fluid Flow," LA-NUREG-6623, Los Alamos National Laboratory (Apr. 1977).

49. L. L. SMITH, "SIMMER-II: A Computer Program for LMFBR Disrupted Core Analysis," NUREG/CR-0453, LA-7515-M, Los Alamos National Laboratory (Sep. 1978).

50. M. F. YOUNG, "IFCI: An Integrated Code for Calculation of All Phases of Fuel-Coolant Interactions," NUREG/CR-4986, SAND87-1048, Sandia National Laboratories (Sep. 1987).

51. G. A. GREENE et al., "Some Observations on Simulated Molten Debris-Coolant Layer Dynamics," *Proc. Int. Mtg. Light Water*

Reactor Severe Accident Evaluation, Cambridge, Massachusetts, August 28-September 1, 1983, Vol. 2, p. 12.2, American Nuclear Society (1983).

52. B. W. MARSHALL, Jr., M. BERMAN, and M. S. KREIN, "Recent Intermediate-Scale Experiments on Fuel-Coolant Interactions in an Open Geometry (EXO-FITS)," *ANS/ENS Int. Topl. Mtg. Thermal Reactor Safety*, San Diego, California, February 2-6, 1986, Vol. 1, p. II.5-1, American Nuclear Society (1986).

53. "The Explosion at the Appleby-Frodingham Steelworks, Scunthorpe 4 November 1975," Health & Safety Executive, HM Factory Inspectorate, U.K. (1976).

54. M. PILCH, "Scaling of Isothermal, Coarse-Mixing Experiments Using Simulant Fluids," Memorandum to M. BERMAN, Sandia National Laboratories (Mar. 7, 1985).

55. L. S. NELSON et al., "Explosive Interactions Between Molten Aluminum and Aqueous Coolants," Quarterly Report prepared for the Aluminum Association, January-March 1987 (May 1987).

56. S. G. BANKOFF, Northwestern University, Personal Communication to T. G. THEOFANOUS et al., University of California, Santa Barbara (Jan. 1986).

57. D. E. MITCHELL, M. L. CORRADINI, and W. W. TARBELL, "Intermediate Scale Steam Explosion Phenomena: Experiments and Analysis," NUREG/CR-2145, SAND81-0124, Sandia National Laboratories (Sep. 1981).

58. D. E. MITCHELL and N. A. EVANS, "Steam Explosion Experiments at Intermediate Scale: FITSB Series," NUREG/CR-3983, SAND83-1057, Sandia National Laboratories (Feb. 1986).

59. J. M. BROUGHTON and E. L. TOLMAN, "Core Boring Observations and Updated TMI-2 Accident Scenario," presented at the Severe Fuel Damage Mtg., Rockville, Maryland, October 21, 1986.

60. W. H. AMARASOORIYA and T. G. THEOFANOUS, "Scaling Considerations in Steam Explosions," *Proc. 1987 Natl. Heat Transfer Conf.*, Pittsburgh, Pennsylvania, August 9-12, 1987, p. 58, American Nuclear Society (1987).

61. T. G. THEOFANOUS, University of California, Santa Barbara, Letter to M. BERMAN, Sandia National Laboratories (Oct. 21, 1986).

62. T. G. THEOFANOUS, University of California, Santa Barbara, Letter to M. BERMAN, Sandia National Laboratories (Oct. 23, 1986).

63. M. J. BIRD, "An Experimental Study of Scaling in Core Melt/Water Interactions," *Proc. ASME 22nd Natl. Heat Transfer Conf.*, Niagara Falls, New York, August 5-8, 1984, No. 84-HT-7, American Society of Mechanical Engineers (1984).

Response to "Comments on 'An Assessment of Steam-Explosion-Induced Containment Failure. Parts I–IV'" by M. Berman

If the size of a letter to the editor is a measure of what is needed to dispute a study, we are flattered by Berman's offering. We, however, will not need nearly as much space to respond.

First, the methodological and philosophical aspects are discussed. As developed in Part I (Ref. 1), the key idea is to establish a successive approximation scheme, whereby the basis for making the necessary technical judgments is continuously enriched as the specialists in various parts of the problem tackle respective issues. Our "causal relation" approach provides the necessary common basis for that; it makes possible a continuously better focusing on the key technical issues, and through this process it allows a continuous refinement of the quality of judgments and gradual reduction of what we call "intangible uncertainty." Of course, this process does not have to be monotonic, nor was it ever intended to be. It does challenge people to lay their technical expertise on the line, and once in the open domain, time can prove rather unforgiving for those who make mistakes. A pure scientist can play agnostic forever; however, a good engineer needs to know when he has reached an adequate basis for a decision, otherwise opportunities for society are lost, and such losses entail their own risks!

Turning next to the phenomenological aspects, the question of premixing (Part II) is the crucial one. Berman has difficulty (item 12 under his summary and conclusion) with our treatment because (a) it ignores transient fragmentation processes, and (b) it has not been experimentally validated. Furthermore, he claims (item 13) that our thermal limits analysis is a modest extension of Bankoff's work and that such a minor modification is not capable of changing "any of the conclusions in previous studies demonstrating large uncertainties..."

Taking on the last point first, what Bankoff's work has done and has not done is discussed in detail in Part II. Suffice to say that our calculations are the first and, to this day, the only ones available for large pours into the lower plenum of a pressurized water reactor (PWR) at low pressures. Indeed, if Berman could produce or cite a calculation that contradicts our results, he should have done so. That, by itself, would have been quite effective in raising questions about our results and would have saved him the considerable time devoted to preparing his extensive comments and their four revisions over a period of over 2 months!

Second, transient fragmentation was indeed ignored. This was not so much a computational difficulty as one of unavailability for reliable physics on the fragmentation (breakup) process. In the general perspective of the accident scenario, we believe that we can provide, for the time being, a useful perspective on the mixing process by varying fuel particle sizes and other aspects of the pour process (i.e., fuel velocity and volume fraction at the inlet) parametrically. From what we have seen in these calculations, we believe that, for a given initial fuel particle size, taking into account transient fragmentation will further reduce the calculated quantities of premixtures (i.e., we are being conservative in ignoring it). This, of course, is subject to confirmation when the breakup process itself is understood and modeled.

Finally, experimental validation was not possible due to the lack of appropriate data (see also the response to Marshall[2]). Even worse, we did not even have the benefit of an independent numerical calculation to compare it with. So, we produced an independent numerical model ourselves.[2] This model treats three fluid fields, thus removing the assumption of a homogeneous steam-water mixture made in the paper being discussed here. By increasing the steam-water drag, we have produced with this model a comparable calculation to the old one, with excellent agreement. Furthermore, in the steam-water slip mode, this model produced somewhat lower premixtures as shown in Fig. 1. This, of course, considerably increases our confidence in our previous estimates of premixing and, following up with our methodology, we are currently preparing a