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

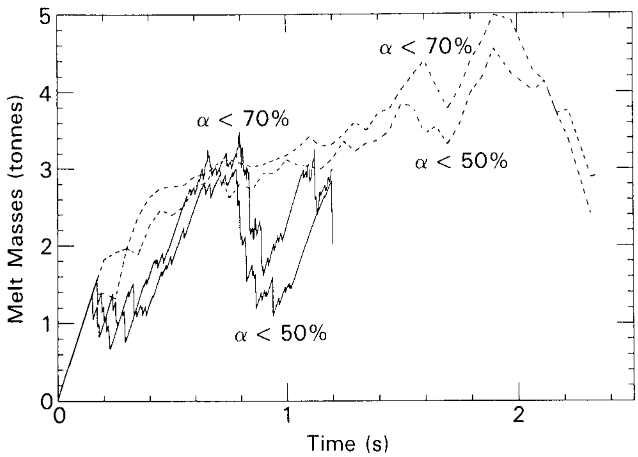

Fig. 1. Comparison of fuel mass transients in premixture predicted by the three-fluid model[3] (——) with that of Part II (----).

whole set of such calculations that will allow us to considerably reduce the upper bound uncertainty limits proposed in Fig. 21 of Part II. Going back to experimental validation, we have documented[4] a scaling study that provides an experimental approach to premixing, and we are currently working toward implementing it.

Furthermore, some brief, point-to-point notes are in order. The numbers refer to the summary and conclusion of Berman's letter.

1. Figure 32 of Part I and its bases are available for everyone to see.

2. Our premixing calculations show that for large pours, by the time there is stratification in the lower plenum, the steam coverage is so extensive that such stratified melt cannot be considered part of the premixture (see also Ref. 4).

3. This is only a sensitivity calculation and must not be viewed as part of the uncertainty analysis. It is provided for additional perspective on the problem.

4. This point was already noted in our paper.

5. This point was not substantiated (see Sec. IV of Part I of Berman's letter). The calculation with flat distributions was indeed done, but conclusions were not based on it! The result of Berman, Swenson, and Wicket (B-S-W) is that the probability of alpha failure is 0 to 1, clearly written in that study and clearly reflected in Berman's beliefs as expressed in his present letter. What B-S-W assumed is succinctly contained in our Table V, Part I (Ref. 1). If this indicates that something is known, we too would surely like to know what it is! Finally, if the uncertainty results in our work are in substantial agreement with that of B-S-W as claimed, then what is all this fuss about? We can only suggest that Berman read Part I over again carefully.

6. Bohl,[5] using SIMMER, had no option but to combine water and fuel. If Berman does not understand the difference in the implied physics and degree of approximation between this and our use of a homogeneous steam-water mixture, he needs considerably more help than we can provide here. Furthermore, if Berman feels he can support the calculations by Bohl and if he has access to the results (we do not), why does he not use them directly?

7. The context for our air-water experiments is carefully explained in Part II.

8. As clearly pointed out in Part I, our work addressed only low-pressure scenarios (see also above comment on premixing and item 16 below). We like to take on problems one at a time.

9. We have provided in the paper the basis of our contention that all our inputs are intended to be conservative, and we do not see the possibility of reversals because of "unanticipated couplings" of different phenomena. Berman will have to be specific.

10. All points are made very clear in our papers. Such accidents have been of major concern for many years and need to be fully clarified in their own right. If Berman has other scenarios in mind, he will have to provide a *consistent specification* for such so that they can be addressed on their own merits.

11. The premixture estimates based on hydrodynamic breakup limitations are based on the idea that a large coherent fuel jet will not breakup in the small L/D involved in the lower plenum geometry. Epstein and Fauske[6] have confirmed this basic idea with detailed analysis.

12. See the above special discussion on premixing.

13. See the above special discussion on premixing.

14. All earlier calculations were cited and discussed in our papers (Part III).

15. The symmetry is imposed by the highly constrained geometry and the vessel wall structure. We believe two-dimensional calculations are adequate. If Berman finds it necessary, he can do the three-dimensional calculations to confirm it.

16. We were the first to suggest weakening of the upper head structure, indeed failure of the primary system pressure boundary, in a high-pressure scenario.[7] Because of this and other reasons,[8] we have recommended that PWRs adopt depressurization procedures (and install systems for such as necessary). This suggestion was given a favorable response by a senior U.S. Nuclear Regulatory Commission (NRC) panel,[9] and currently both the NRC and utilities are pursuing the details. Furthermore, such decisions have already been made in other countries [i.e., Federal Republic of Germany (FRG)]. For all these reasons, we have focused our efforts on the low-pressure scenario.

17. This is acknowledged in Part III, where we conclude with the statement, "explicit consideration of explosion propagation in the [short-term expansion] and of multifluid momentum exchange and inertial effects in the [long-term expansion] should allow a better definition of uncertainties." We have just developed the tool that makes this possible.[10]

18. Here again Berman is jumping to conclusions. Quite obviously, the expansion condition between that named "ideal" and the "mechanistic" ones are quite different. In particular, the mechanistic ones involve significant changes in control volume and fluid release. Taking these into account plus the thermal energy component produced adequate accounting of energy conservation in all cases. These types of tests are standard quality assurance procedures and normally need not be reported explicitly.

19. The importance of developing experimental evidence is amply explained in our papers. Surely Berman could not expect us to delay publication until we have a full panoply of experiments to go with it. Furthermore, having the prediction first makes it possible to optimize experiments to the intended purpose (i.e., see Ref. 4).

20. As emphasized in Part I, this is a probabilistic study. It is not a sensitivity study. One can run sensitivity studies forever; however, if one does not know how to distinguish the various results, it becomes no more than an exercise in futility.

Finally, with reference to detailed sections of Berman's letter, we note the following:

1. Berman's discussion of Fig. 32 (Sec. X of Part I in his letter) betrays his lack of understanding of the discrete probability distribution method. This is not a place for a tutorial, of course. Suffice it to say that his conclusion that the total probability to a frequency >0.1 is 1000 times (!) greater than is stated in the figure is completely erroneous. Depending on how the calculation is done and on how many points are used to discretize the distributions, the final result may appear more or less uniformly distributed. The original calculations were done with DQUAN. Meanwhile, we developed our own code (P-ALPHA), which allows better attention to such details. Using the same distributions as Part I, the bottom line result[11] is shown in Fig. 2, and demonstrates that Berman's attempted interpretations (his Table I) of Fig. 32 (Part I) are simply erroneous. It is emphasized again, as clearly stated in the paper, that Fig. 34 (Part I) is offered for the sole purpose of providing some "additional perspectives" and that it represents only a parametric study. It is not, repeat *not*, part of the probabilistic study.

2. Going back to premixing, the reader will have to draw his preference between our Fig. 21 and Fig. 6 of Ref. 3 (reproduced as Fig. 3 here), and Berman's modifications of them shown as his Fig. 13′. Our figures are based on *two fully documented, published models* including both a two-fluid and a fully three-fluid treatment. His modifications are based on the handwaving of his letter. Note also that Berman, in this handwaving, misapplies the Theofanous-Saito[13] ideas to a multiple jet stream configuration. As clearly discussed in Part II, this is precisely the regime we call prefragmented and for which our thermal limits model applies.

3. Berman complains that we have not run for him the sensitivity studies he suggested. We did not because he had not provided a good basis for considering them as plausible. In looking over how badly he mistreated our two sensitivity studies by attempting to make them part of the probabilistic model (see item 3 above), we are glad we did not oblige.

4. In his Sec. I of Part III, Berman disputes our statement that we could not carry out detailed comparisons with the results of Bohl because their initial conditions were not available. Again, if they were, it would be so much simpler to let us know where we can find them. More importantly, since he did have access to the draft report[5] of this work (we only know and reference the 1985 letter[13]), why did he not provide concrete quantitative data from it to dispute our results? Only such an approach would have generated the fruitful technical interchange envisioned by our methodology.

5. Referring to his section "Final Comments," we find most comments emotionally charged and inappropriate for a scholarly journal. We will not respond in kind. As mentioned above, the depressurization of PWRs has already been ordered in the FRG, it is imminent in many other countries, and not withstanding Berman's acrobatics (only just recently he was arguing against the concept of the so-called pressure cutoff in steam explosions), we strongly hope (and believe) the U.S. reactors will follow suit.

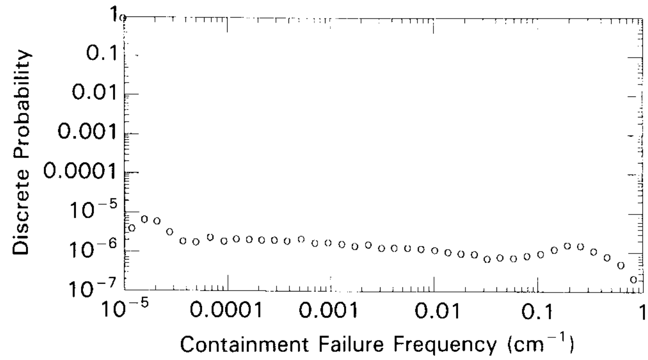

Fig. 2. Probability distribution of containment failure frequency (conditional on major core melt).[12]

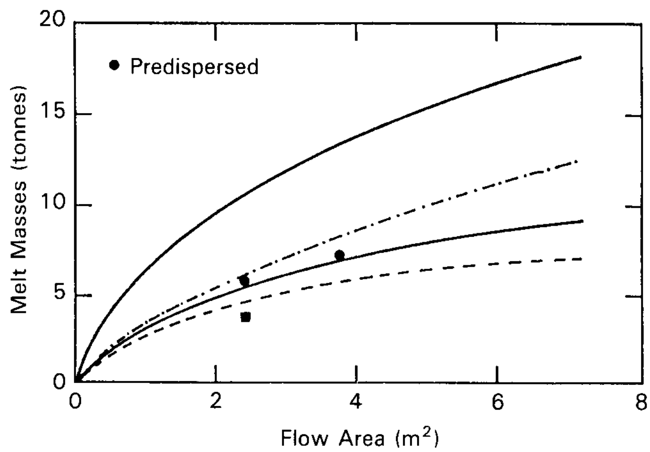

Fig. 3. Qualification of melt mass in premixture from the three-fluid model calculation[3] (■) and as given by Part II.

T. G. Theofanous

University of California at Santa Barbara
Department of Chemical and Nuclear Engineering
Center for Risk Studies and Safety
Santa Barbara, California 93106

May 6, 1988

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. T. G. THEOFANOUS, *Nucl. Sci. Eng.*, **100**, 171 (1988).

3. W. H. AMARASOORIYA and T. G. THEOFANOUS, "Premixing of Steam Explosions: A Three-Fluid Model," *Proc. 1988 Natl. Heat Transfer Conf.*, Houston, Texas, July 24–27, 1988, Vol. 3, p. 191, American Nuclear Society (1988).

4. 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).

5. W. R. BOHL, "An Investigation of Steam Explosion Loadings with SIMMER-II," Draft Report (1986).

6. M. EPSTEIN and H. K. FAUSKE, "Steam Film Instability and the Mixing of Core-Melt Jets and Water," *Proc. 1985 Natl. Heat Transfer Conf.*, Denver, Colorado, August 4–7, 1985, p. 277, American Nuclear Society (1985).

7. H. P. NOURBAKHSH, C-G. LEE, and T. G. THEOFANOUS, "Natural Circulation Phenomena and Primary System Failure in Station Blackout Accidents," *Proc. 6th Information Exchange Mtg. Debris Coolability*, Los Angeles, California, November 7–9, 1984, EPRI-NP-4455, Project 1931-1, p. 24-1, Electric Power Research Institute (Mar. 1986).

8. T. G. THEOFANOUS, "Integrated α-Mode Failure Analysis," and "Energetic Aspects of Severe Accidents at High Pressure," presented to the U.S. Nuclear Regulatory Commission Expert Panels on Severe Accident Research, Bethesda, Maryland, January 28, 1987.

9. H. KOUTS, "Review of Research on Uncertainties in Estimates of Source Terms from Severe Accidents in Nuclear Power Plants," NUREG/CR-4883, U.S. Nuclear Regulatory Commission; see also BNL-NUREG-52061, Brookhaven National Laboratory (Apr. 1987).

10. S. MEDHEKAR, M. ABOLFADL, and T. G. THEOFANOUS, "Triggering and Propagation of Steam Explosions," *Proc. 1988 Natl. Heat Transfer Conf.*, Houston, Texas, July 24–27, 1988, Vol. 3, p. 244, American Nuclear Society (1988).

11. A. MOGHADAM and T. G. THEOFANOUS, *Reliab. Eng. Saf. Sys.* [to be published in Vol. 23 (1988)].

12. T. G. THEOFANOUS and M. SAITO, *Nucl. Eng. Des.*, **66**, 301 (1981).

13. W. R. BOHL and T. A. BUTLER, "Some Comments on the Probability of Containment Failure from Steam Explosions," in "A Review of Current Understanding of the Potential for Containment Failure Arising from In-Vessel Steam Explosions," NUREG-1116, U.S. Nuclear Regulatory Commission (Feb. 1985).

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

During the past few years and as a result of the Three Mile Island Unit 2 (TMI-2) and Chernobyl accidents, the reactor safety community has renewed its efforts to assess and understand the consequences of severe core melt accidents inside nuclear power plants. Of the many issues associated with severe accidents that have been reviewed over the past few years, few have been as highly debated as the alpha-mode failure question. As described in Refs. 1 through 4, alpha-mode failure could occur if the interaction between molten core material and water were energetic enough to fail the upper head of the reactor primary vessel and create a "missile" having sufficient kinetic energy to threaten the structural integrity of the containment building on impact.

The authors of the four papers[1–4] have attempted to define the probability of alpha-mode failure based on their models and experiments and to narrow the uncertainty associated with these results. However, the technical basis in many areas cannot support many of their assumptions.

I will focus here on the underlying technical assumptions that are important and unique to this study. Although there are many issues that could be discussed, I believe that the results and analyses presented for the coarse mixing phase represent the most crucial link determining the final probability of alpha-mode failure. This emphasis is chosen for three reasons. First, the authors have assumed distribution functions for many of the mechanisms that appear to be similar to those presented in previous probabilistic studies of alpha-mode failure[5,6] and, therefore, the same uncertainties that applied to those studies also apply to this one. Second, the assumptions and code calculations performed for the coarse mixing phase represent a novel probabilistic approach. Therefore, a more extensive investigation of the coarse mixing arguments and results appears warranted. Third, I have limited these comments to coarse mixing, an area in which I have conducted pertinent research, and, therefore, can assess the results and assumptions presented in Parts I and II (Refs. 1 and 2). I assume that my colleagues in the field of reactor safety research will comment on other important areas pertaining to this issue.

I. REVIEW OF THE COARSE MIXING MODELS AND EXPERIMENTS PRESENTED

In the abstract of Part II of the report,[2] the authors have stated 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." Surely the authors do not mean this as written. The implication here is that they have completely analyzed the coarse mixing phase using models and analyses that have been well validated against appropriate experimental data. As written, I believe the statement is false. The issue of coarse mixing has not been "addressed and resolved" within the framework of this paper, or for that matter, within the technical community. In the following paragraphs, I will discuss why this issue remains *unresolved*.

The coarse mixing phase is a highly transient, multiphase, multidimensional process that is dependent on the initial and boundary conditions of the system considered. As melt flows through the lower plenum during a core melt accident, the flow distributor plates create multiple jets (or streams) of fuel. As an example, ~3.8 h into the accident at TMI-2, ~20 t of core material relocated from the core region into the lower plenum. Postaccident inspections have shown that the structural and flow distributor plates in the lower plenum were relatively undamaged.[7,8] Therefore, molten core materials will have flowed through these structural plates, creating streams or jets of molten fuel surrounded by water. The authors correctly summarize these observations: "Inevitably, we are lead [sic] to the consideration of fuel entering the lower plenum in the form of multiple relatively small-diameter streams." Hence, the coarse