Response to "Comments on 'An Assessment of Steam-Explosion-Induced Containment Failure. Parts I-IV'" by B. W. Marshall, Jr.

To a large extent Marshall's letter contains a repeat and embellishment of Berman's points[1] on fixed-diameter particles (comment 1), single-field representation of the coolant (comment 2), and validation with experiments (comment 3). These points have been fully addressed in our response[2] to Berman (especially in our third paragraph) and there is no need for further repetitions. It is sufficient, therefore, to focus the discussion here on the available experimental base (Sec. I.A of the letter).

Marshall cites work at Argonne National Laboratory (ANL) (Spencer et al.[3,4] and Gabor et al.[5]) and Sandia National Laboratories (SNL) (Marshall et al.[6]) as the pertinent experiments on premixing, and he suggests that we have missed an important opportunity to test our computational tools. The simple answer is that these experiments provide no information of premixing and, therefore, cannot be used for the suggested purpose. To our knowledge (confirmed by personal communication with Spencer,[7] who also indicated that some data analysis currently under way *might* provide some information on premixing) the ANL experiments were not intended for premixing; the SNL experiments ostensibly were, but they were so poorly instrumented that we have to wonder why they were ever run!

As Figs. 1 through 4 of the letter indicate, only the outer mixture diameter (single jets) is given, and this information is a far cry from what one would call premixture information. The key parameters of a premixture are its water and melt contents; none are available in these experiments! Furthermore, as evidenced in the discussion, Marshall seems to associate expansion of the mixture region (externally observed) with "extensive fragmentation" of the jet. This *may* be so, but we will not know for sure until these experiments are run properly and with the proper instrumentation. Finally, the cited references provide no information on the details of the jet entry configuration, nor indeed of the melt entry velocity.

Regarding the "implication for reactor safety" section, we would like to briefly note the following:

1. Marshall commits the same error as Berman in applying the Theofanous-Saito[8] ideas to a multiple-jet geometry. This is where steaming limitations come into play, and this is the whole point of Part II (Ref. 9).

2. The Theofanous-Saito ideas were confirmed with a detailed analysis by Epstein and Fauske.[10] Marshall must take another look at Theofanous-Saito; far from ignoring steam generation, it is an essential aspect (as in the Epstein-Fauske analysis) of the argument. Marshall's "data" are utterly inadequate (as elaborated above) to dispute the conclusions of these two studies.

3. Marshall states that "it is obvious from our experiments at SNL ... that the characteristic diameter of the fuel *changes with time*." In light of what he measured (or could see!) in these experiments (i.e., Figs. 1 through 4), this is simply an incredible assertion.

To conclude, we would like to reiterate that we have put forth a fully documented one-of-a-kind calculation to predict *upper bounds* on premixing. Subsequently, we have shown that steam clip results in lower premixtures (see Figs. 1 and 3 of our response to Berman[2]) and we claim that ignoring fragmentation is conservative. We have also developed a scaling approach to test this prediction in the relevant regimes.[11] We are comfortable with our positions, and Marshall will need much more than vague references to vague experimental results to dispute these positions convincingly. Indeed, it would be so much more constructive if he could offer a positive contribution himself.

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

INTRODUCTION

My comments are a distillation of this report[1] as a member and vice-chairman of the U.S. Nuclear Regulatory Commission (NRC) Steam Explosion Review Group (SERG) (Refs. 2

through 7). The collection of papers published by Theofanous and coworkers in *Nuclear Science and Engineering*[8] comprise NUREG-5030 (Ref. 1) and address the probability that the containment of a pressurized water reactor (PWR) fails due to the impact of a missile generated by an in-vessel steam explosion. The WASH-1400 reactor safety study[9] termed this containment failure mechanism "alpha-mode failure." Although this containment mechanism has been subjectively considered to be of low probability, it remains important because of its possible consequences for relatively early and prompt containment failure. It is my overall impression that aspects of the study provide significant technical advances to previous research work on this topic.[10–12] However, I also feel this work has some flaws and limitations that should be pointed out. Normally one does not take the time to write a letter to the editor to indicate particular points of disagreement or limitations in an analysis. However, because of my past involvement in this review, my own area of research, and the policy implications of this work, I felt this was a unique case.

I divide my comments into four general categories: (a) the subjective nature of this work, (b) limitations of the study, (c) documents, and (d) technical comments on the remainder of the study (Parts II, III, and IV).

SUBJECTIVITY

One should realize that this work is quite extensive and involves subjective judgments by the authors as well as technical analysis in support of these subjective judgments. This combination of subjective "engineering judgment" and technical analysis is not new in the area of engineering and science, but must be recognized for what it truly represents. Some might characterize this combination as "technical policy analysis" (other names given have been technological decision analysis, technology management, or policy science). Technical policy analysis must always contend with making a policy decision in the face of uncertainties in the technical "facts." This is particularly true when the policy involves technical facts or data on low-probability-rate events, e.g., earthquake prediction and protection. This has always been the case in reactor safety issues involving severe accidents because of the limited data base. The NRC performs research on the basis that the technical foundation of a particular safety policy (e.g., severe accident policy) will ultimately be confirmed by the "hard technical science" of experiments and associated analysis (mechanistic models or correlations). The reader must be aware of this unique characteristic of this work.

Part I of the work utilizes the subjective input for a set of needed initial conditions (e.g., fuel mass "mixed" with water) to estimate the alpha-mode failure probability. Parts II, III, and IV present the technical analysis on which the subjective estimates for initial conditions are based. In our past work, when estimates of alpha-mode failure were published,[10] we were careful to note that the final estimate for failure probability was subjective and based solely on our judgment. In our own work, we had technical reasons for our judgment, but we clearly identified the subjectivity of our input for estimating alpha-mode failure. I think it is imperative that the reader of the current work realize that the final quantitative values are subjective and critically evaluate whether the technical analyses presented in Parts II, III, and IV support the subjective estimates used in the Part I analysis. Based on my own personal experience, readers will tend to use these numerical estimates as "technical fact," instead of a "quantitative" estimate of an individual's subjective judgment. This is unfortunately but pragmatically the case. The discussion in Part I of this work does not emphasize this point enough, in my opinion. The reader is left with the impression that the probability of alpha-mode containment failure is a technical fact when it is truly a quantitative opinion.

Another way to view this probability is to realize that if we knew the initial conditions of a core melt and understood the fundamentals of the explosion process, the actual alpha-mode failure probability would be a particular value, i.e., near 0 or 1. It is the uncertainty in the initial conditions and the lack of fundamental understanding of the physical process that result in a subjective value other than the two possible bounds. It is then obvious that the uncertainty lies between 0 and 1 and that the subjective value calculated is a systematic way of expressing that.

LIMITATIONS

Because the steam explosion process might occur in a number of situations in a severe accident, this current work should be appreciated in light of what it does not address. These limitations should not detract from the breadth and technical detail of Theofanous and coworkers' efforts, but provide a framework for the other steam explosion issues. In brief, one could list what this study does not address:

1. The alpha-mode failure probability for a boiling water reactor (BWR) cannot be determined from the current analysis, although this framework could be used for a subjective estimate.

2. Possible containment failure from ex-vessel steam explosion is not addressed (e.g., Mark-II or -III boiling water reactors).

3. Contribution of steam explosion to other containment hazards is not covered (e.g., hydrogen generation).

DOCUMENTATION

The SERG committee reviewed this work[1] over three separate meetings with written correspondence to document the reviewers' questions and suggestions for improvements, as well as Theofanous' written responses. These review comments and written responses are part of Ref. 1. I think that the reader should be aware of this document and the reviewers' comments that form its appendix. The SERG review process focused on open discussion of issues raised by the study and not necessarily on complete resolution of every technical issue and improved analysis.

TECHNICAL ANALYSIS

The probability estimate of this work by Theofanous and coworkers is based on an extensive set of technical analyses (Parts II, III, and IV). Within these technical analyses, models are developed and assumptions made that form the basis of the subjective input used in the probabilistic treatment of Part I. Even after the SERG review, there are a few technical points that need to be discussed. The intent is not to detail all the issues. This has been done in the past letter correspondence[2–7] and is documented in the appendix of Ref. 1. I only briefly cover some key points below.

Sensitivity Calculations in Part I

In Part I of the work,[1] two sensitivities were included based on past suggestions and comments from SERG members.

One sensitivity considered larger masses of fuel mixed with water for small pour areas (5 to 10 tonnes), based on stratified explosion conditions. It should be pointed out that these masses were arrived at using a fuel penetration depth of 10 cm (see Part II) based on assertion, not data. Also, this 5 to 10 tonnes of additional fuel mixed does not include the fuel mixed as it pours from the core to the plenum. This could add 2 to 3 tonnes of fuel, based on the analysis of Part II. Interestingly, changing these values had an almost negligible effect on the overall result. This implies that small variations in the fuel mass mixed have a small effect, because we are below some threshold. I think it would be useful to know this threshold, because it then points to the margin of uncertainty allowable in this estimate. A fuel mass of 10 tonnes is ~8% of the core (including clad). In our first work,[13] we found that the threshold from no containment failure to assured containment failure for the Zion PWR was between 10 and 40% of the core for nearly thermodynamic explosion conversion ratios. It would be useful to see if this analysis verifies this past result. There is no way to tell here. I would recommend that such an analysis be done since the fuel mixed before the explosion is the key uncertainty in all of these analyses. I also would recommend that this bounding calculation be considered including the second sensitivity of lower plenum failure to see how this affects the resulting fuel mass required. The synergism of these two sensitivities was never considered.

The second sensitivity is the exclusion of lower plenum failure and recalculating the alpha-mode containment failure probability. The preclusion of lower plenum failure increases the failure probability by a factor of 100. That was quite surprising and the reasons for this ought to be examined in some detail. No discussion of this point was provided in Part I. This result indicates to me that the structural analysis and the accompanying fluid/structure interaction analyses ought to be carefully reviewed and compared to past structural analyses as done in past work.[3,10,11,13] Once again, the structural threshold should be found. This was not done in the three review meetings, even though this was suggested early in the review. All of us from the SERG group focused to a large extent on the steam explosion process and not on the structural analysis, given that it looked reasonable. In fact, none of us were capable of critically assessing the structural analysis. This should be evaluated and compared to past structural analyses.

Let me suggest some possible reasons for the resultant large probability change for the second sensitivity. It appears that the way in which lower plenum venting is modeled grossly overexaggerates the downward kinetic energy and the loss of kinetic energy in the upward-directed missiles. Theofanous and coworkers indicate in Parts III and IV that the upward-directed slug kinetic energy is reduced by factors of 5 to 10. Past analyses (e.g., Refs. 10 and 11) estimated much smaller kinetic energy reductions (factor of 2). Perhaps this overestimation may be due to the assumed complete "unzipping" of the entire lower plenum. Such a failure would overestimate the plenum failure benefit compared to more realistic asymmetric wall failure and local venting of the high-pressure vapor. Such concerns were raised in the SERG review of Ref. 1.

The final point about sensitivities is that many of these parameter studies are needed when one uses subjective input. This is particularly true about fuel-coolant mixing because this input is obviously important. The concept of a threshold of missile kinetic energy for containment failure is quite important and can be determined by such a sensitivity study.

TABLE I

Advantages and Disadvantages of Current Mixing Models

Model	Advantage	Disadvantage
Phoenics (Bankoff and Habib[14])	Two-dimensional	No dynamic liquid breakup Equal velocities and temperatures for coolant liquid and vapor
K-FIX (Theofanous)	Two-dimensional	No dynamic liquid breakup Equal velocities and temperatures for coolant liquid and vapor
SIMMER (Bohl[15])	Two-dimensional	No dynamic liquid breakup Equal liquid velocities (fuel and coolant liquid)
TEXAS (Chu and Corradini[16])	Dynamic liquid breakup Three-field model with fuel Lagrangian and unequal velocities	One-dimensional

Mixing Calculations in Part II

A major portion of the theoretical work in this set of papers was in the area of fuel-coolant mixing. The analysis done has some important limitations that may not be immediately apparent and are not discussed by the authors. I have discussed these limitations in detail in past presentations and correspondence,[2–7] so I will be brief. Let me summarize and state that fuel/coolant mixing when fuel pours into a coolant pool is a complex phenomenon with a very limited data base (some may feel no data base). The mixing data available are primarily qualitative and one must be careful not to put too much faith in theoretical predictions without data to validate the models. This is particularly true when all the current mixing models have distinct limitations as shown in Table I.

The authors criticize the other mixing models (and rightly so) for their limitations, but are somewhat reticent to point out that their analysis has similar limitations, e.g., as in the Bohl and Bankoff models. The qualitative model discussed in Part II for mixing does not have a rigorous basis, e.g., the relation that results in a 10-cm depth of penetration for fuel mixing. Therefore, the authors should point out the uncertainties of the final values subjectively chosen and used in Part I. Recently, researchers at Sandia National Laboratories (SNL) (Young[17]) have developed a two-dimensional model for fuel-coolant mixing that incorporates a dynamic fuel breakup model (developed by Pilch) and allows for separate fluid treatment of the fuel, coolant vapor, and liquid (three fields) in an Eulerian treatment. Preliminary calculations with this integrated fuel/coolant interactions model are promising and may be an advancement over past work.

Note one final point about mixing. Experiments in this area are difficult to perform and even more difficult to interpret.

Therefore, I would consider the current results to be semiquantitative. The air-water mixing experiments quoted in Part II must be analyzed carefully, especially with regard to scaling, before they are of use in explosion analysis. More new data are available from SNL that should also be analyzed. The important limitation about these current data is that they are at 1-atm pressure. All the mixing models suggest a significant increase in fuel-coolant mixing as ambient pressure increases, and this needs to be examined.

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

Corradini's comments[1] are more along the lines of the kind of technical exchange we envision in our methodology (see second paragraph of our response to Berman).[2] We welcome them, and we welcome the opportunity to respond.

Referring to his main headings:

1. *Subjectivity:* The issue raised here is that Part I does not emphasize enough the subjective component of the quantification. It is not clear what would have been enough, and we have no problem with the additional emphasis added here. Let me reiterate, however, the two important ideas that exemplify our own note of emphasis in this area. The one refers to the overall methodology that seeks to establish a successive approximation scheme with many independent investigators contributing toward enriching the basis and refining these judgments. The other refers to what we call "intangible uncertainty"; it is impossible to quantify but we expect it to diminish gradually as a result of the synergistic effect of multiple independent contributions to this process (see also Ref. 3).

2. *Limitations:* We certainly agree.

3. *Documentation:* We found the Steam Explosion Review Group (SERG) experience very useful and took advantage of it to appropriately revise the manuscript. As an important aspect of our methodology, we invited the members to document any remaining reservations in letters included in the appendix of the U.S. Nuclear Regulatory Commission report that contains our papers.[4] In addition to the three letters discussed here, a letter from R. Anderson (Argonne National Laboratory) has been received and included.

4. *Technical Analysis, Sensitivity:* The energy (and fuel mass in premixture) threshold can be easily obtained from information provided in Part I. It is estimated as something over 25 t. Still, though, the whole point here is to get away from bounding analyses, which are generally agreed to be not a very fruitful approach for severe accidents.

The second sensitivity indicates that with the generous uncertainties adopted for premixing, the lower plenum failure