PALL A

NUREG-1116

A Review of the Current Understanding of the Potential for Containment Failure From In-Vessel Steam Explosions

U.S. Nuclear Regulatory Commission

Office of Nuclear Regulatory Research
Office of Nuclear Reactor Regulation

Steam Explosion Review Group (SERG)

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EXECUTIVE SUMMARY

A group of experts was convened at the request of Dr. Denwood Ross, Deputy Director of the Office of Nuclear Regulatory Research of the U.S. Nuclear Regulatory Commission, to review the current understanding of the potential for containment failure arising from in-vessel steam explosions during core meltdown accidents in a typical PWR and BWR. The Steam Explosion Review Group (SERG) was requested to provide assessments of: (i) the conditional probability of containment failure due to a steam explosion, (ii) the Sandia National Laboratory (SNL) report entitled "An Uncertainty Study of PWR Steam Explosions," NUREG/CR-3369, (iii) a SNL proposed steam explosion research program. This report summarizes the results of the deliberations of the review group. It also presents the detailed analyses of each individual member to each of the issues raised by Dr. Ross.

Fundamental mechanisms involved in steam explosion accident sequences were discussed at a meeting of the SERG. Initial conditions, melt-water mixing, thermal-to-mechanical energy conversion, multiple explosion, slug dynamics and mechanical damage stages of steam explosion sequences were discussed. In each area the SERG came to some agreements and identified some areas of disagreement. The agreements were conceptual in nature and a consensus on the validity of specific models of the phenomena was not reached by the group. In the important area of fuel-coolant mixing, the SERG agreed in principle on the validity of transient jet breakup, flooding and/or fluidization limits on the extent of fuel-cooling mixing. Consensus agreement on the validity of specific analytical models, however, was not reached by the group. The SERG discussions are summarized in Appendix D to this report.

Each member of the SERG provided subjective quantitative estimates of the conditional probability of containment failure based upon combinations of mechanistic and probabilistic arguments. The SERG members provided "best estimate" values of the subjective conditional probability of containment failure that range from 0 to 10^{-2}. Of 10 numerical best estimates provided, six of them have values which are less than or equal to 10^{-4} and seven are less than or equal to 10^{-3}. Several SERG members also provided "upper limits" to the subjective probability of containment failure which ranged from 0 to 0.1. All but one of these (the 0.1 value) are characterized as being upper limits of the individual's best estimates. Of seven such numerical estimates provided, six of them are less than or equal to 10^{-2} and three are less than or equal to 10^{-4}. Of the two members who did not provide numerical estimates, one believes that WASH-1400 is "very conservative" in its treatment of steam explosions, and the other believes that steam explosions pose no threat to the Federal Republic of Germany's PWR containment.

Based upon the probability estimates summarized above, the consensus of the SERG is that the occurrence of a steam explosion of sufficient energetics which could lead to alpha-mode containment failure has a low probability. This conclusion is reached despite the expression of differing opinions on modeling of basic steam explosion sequence phenomenology. An opinion supported by most members of the group is that the probability of containment failure is reduced due to the expectation of limited melt mass involvement in the explosion and/or low thermal-to-mechanical energy conversion. Other members placed more emphasis on reduction by other physical mechanisms. Individual group member quantitative estimates of the subjective conditional probability of containment failure and supporting arguments are presented in the report. These estimates are contrasted with results from WASH-1400.

With broad consensus the SERG members disagreed with the methodology as used in NUREG/CR-3369 for the purpose of establishing the uncertainty in the probability of containment failure by a steam explosion. Nearly all of the members also disagreed with the Sandia conclusion that "Indeed the results (for the conditional probability of containment failure) span the range from 0 to 1." The major factor in the review group's disagreement with the Sandia work is that, in the choice of probability density functions, Sandia does not make direct use of physical models which are representative of current thinking of most of the SERG members.

A consensus was reached among SERG members on the need for a continuing steam explosion research program which would improve our understanding of certain aspects of steam explosion phenomenology and which would be directed toward providing confirmation of the SERG conclusion that the probability of containment failure by steam explosion is low. Most members agreed with a continuation of the Sandia FITSX test program involving melt masses of up to 50-100 kg. Most agreed with "Alternate Contact Mode" experiments, involving explosions in stratified geometry. Consensus could not be reached on the advisability of pursuing the large-scale SEALS experiments (up to 2000 kg melt mass) proposed by Sandia at this time.

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1. INTRODUCTION

1.1 Background

This report presents the results of a review of the present understanding of the potential for containment failure due to in-vessel steam explosions during core melt accidents in light water reactors. The review includes independent estimates of the probability of such events by several experts in this area. It also includes their comments regarding a recent proposal by the Sandia National Laboratory for a research program to reduce the uncertainties in these estimates. Before proceeding we will first qualitatively define the term "steam explosion" and explain its significance to reactor safety.

The term "steam explosion" refers to a phenomenon in which molten fuel rapidly fragments and transfers its energy to the coolant resulting in steam generation, shock waves and possible mechanical damage. To result in a significant safety concern the interaction must be very rapid and must involve a large fraction of the core mass. If such events were to take place within the reactor pressure vessel missiles could be generated which might penetrate the containment and allow early release of fission products. In the Reactor Safety Study (WASH-1400) this mode of containment failure was denoted by the symbol alpha (α) and is often referred to α-mode containment failure or simply α-mode failure.

The need for the present review arose from the conclusions given in a report issued by the Sandia National Laboratory ("An Uncertainty Study of PWR Steam Explosions," NUREG/CR-3369, May 1984) of uncertainties associated with estimates of the probability of α-mode failure from in-vessel steam explosions. That document reflected the author's opinions that such probabilities must be considered to range from 0 to 1. The implication from the report is that it is not possible to provide a meaningful best-estimate of the probability of such events. This conclusion was considered to be at variance with conclusions from a number of other studies including those of the NRC staff and its consultants. These views have been expressed by the staff during hearings on the Indian Point reactor. 1/ (See also Meyer, J. F., et. al, "Preliminary Assess-ment of Core-Melt Accidents at the Zion and Indian Point Nuclear Power Plants and Strategies for Mitigating Their Effects, Appendix B," NUREG-0850, Vol. 1.) It was, therefore, deemed advisable to convene a group of experts to review NUREG/CR-3369 and to obtain from them their own independent assessment of the likelihood of such events as well as their comments on that document. In addition, since SNL had recently submitted a related research proposal, it was appropriate to request comments from these experts on the merits of that proposal.

1.2 Objectives of the Review

The review was organized in response to a letter, dated August 24, 1984, from Dr. D. Ross, Deputy Director of NRC's Office of Nuclear Regulatory Research, to several experts on the subject of steam explosions. In that letter Dr. Ross requested the experts' input in three areas:

1/ A Board Notification was issued to the Indian Point Licensing Board on August 24, 1984 in which that Board was notified that a review of the conclusions reached by the SNL in its report NUREG/CR-3369 regarding uncertainties in the probability of an in-vessel steam explosion induced containment failure was being conducted.

(i) "What in your judgment, is the best estimate value for the probability of containment failure arising from a steam explosion, given a core melt, that we should be using in the severe accident program? As part of your response please provide the bases (experimental and/or analytical) and the approach used in reaching your conclusions."

(ii) "Please provide written comments on the Sandia study described in NUREG/CR-3369 and any additional inputs and analyses which you feel are relevart to this study. As pointed out above, the SNL report is based on assumed uniform distributions and some additional calculations, assuming other distributions, may be required."

(iii) "You should plan to attend a two-day meeting in the Washington, D.C. area during the week of October 15, 1984 to discuss your response to items 1 and 2, to review the NRC research program, and to assess the comments on the Sandia study and the various inputs from the experts on containment failure probability."

These requests are referred to in this report as Request Numbers 1, 2 and 3. Note that the focus of Request Number 3 is a review of the Sandia proposed research program. Further, the October meeting referred to in that request was deferred to November 27 and 28 of 1984. These requests were further defined in a letter issued to SERG members dated October 2, 1984.

A copy of Dr. Ross' letter is included in Appendix A together with a list of the experts contacted (referred to as the Steam Explosion Review Group or SERG) and other correspondence related to this review (including the October 2, 1984 letter referred to above).

1.3 The Review Process

A meeting was held at Harpers Ferry, West Virginia on November 27-28, 1984 to discuss the results from these assessments and reviews. Correspondence describing the organization and purpose of the meeting together with the agenda, a list of attendees and schedule for the entire review are provided in Appendix A. Members of the SERG were provided background material prior to the meeting at Harpers Ferry. 2/

To facilitate discussion at the meeting the participants were asked to provide their written responses to the questions posed in Dr. Ross' letter well before the meeting. Their written responses were submitted in two parts. The first submittal contained responses to Request Numbers 1 and 2 (containment failure probability estimates and comments on NUREG/CR-3369 respectively.) The second set of responses contained comments on the SNL proposed research program and was provided following their review of an updated and complete research proposal submitted by Dr. Berman on October 29, 1984. Dr. Berman's research proposal is provided in Appendix B. The responses from members of the review group regarding both the first two requests and comments on the proposed research program are provided in Appendix C.

The Harpers Ferry meeting was co-chaired by Dr. T. Ginsberg (Brookhaven National Laboratory-BNL) and Dr. M. Corradini (University of Wisconsin). Dr. Ginsberg opened the technical sessions by describing the meeting objectives, format and ground rules. As noted in the agenda each participant was allotted approximately 20 minutes (including questions) to highlight key points in his approach to estimating containment failure probabilities. Dr. Marshall Berman of SNL was provided the opportunity to present his views on NUREG/CR-3369 and to discuss the SNL research proposal.

2/ Dr. Corradini and Dr. Berman provided a collection of reports and articles relevant to this subject (see letters from them to the SERG members provided in Appendix A).

Dr. Ginsberg explained that, following these presentations, Dr. Corradini would summarize the current state of knowledge of steam explosion phenomena as developed from the presentations. A preliminary summary was presented to the group and discussed with them on the second day of the meeting. An attempt has been made to identify areas of agreement regarding the state of uncertainty of the important phenomena associated with the steam explosions. Dr. Corradini's revised summary is included in Appendix D.

As noted in the schedule in Appendix A the SERG members were provided an opportunity, following the Harpers Ferry meeting, to submit revisions and/or additional comments. The SERG members have also been given the opportunity to review and comment on an initial draft of this report. Their opinions have been incorporated in this final version and their written comments are included in Appendix F. Comments by the authors of NUREG/CR-3369 are included in Appendix E. Note that the page numbers cited in both sets of comments refer to an earlier draft of this report and will not, in general, correlate with the pagination of this final report.

1.4 Organization of the Report

In the remainder of this report we present the responses of the SERG to the requests by Dr. Ross described earlier.

At the Harpers Ferry meeting described in Section 1.1.2 above, Dr. Ginsberg agreed to summarize the conclusions regarding the individual estimates of a-mode failure probabilities. A preliminary summary in this area was presented to the group and discussed on the second day of the meeting. Dr. Ginsberg's revised summary is included in Section 2.1 below which summarizes the group's response to Request Number 1 of the Ross letter.

In response to Request Number 2 of the Ross letter, the group also discussed the various comments on NUREG/CR-3369 and a summary of these views is presented in Section 2.2 below.

Finally, in response to Request Number 3 of the Ross letter, following presentations from SNL regarding their research proposal, the group discussed the merits of the proposed program. A summary of the views expressed in this area is given in Section 2.3 below.

The reader is referred to Appendix C for the written responses from each SERG member. Appendix D contains a summary of the group's discussions on steam explosion phenomena. Appendix E presents comments by the authors of NUREG/CR-3369 on the draft SERG report and Appendix F contains comments from SERG members.

2. MAJOR RESULTS OF STEAM EXPLOSION REVIEW GROUP DELIBERATIONS

2.1 Response to Request Number 1: Probability of Containment Failure by Steam Explosions

"What, in your judgment, is the best estimate value for the probability of containment failure arising from a steam explosion, given a core melt, that we should be using in the severe accident program? As part of your response please provide the bases (experimental and/or analytical) and the approach used in reaching your conclusions."

In a subsequent letter (October 2, 1984) the above question was further defined. It was then recommended that responses be addressed to both PWR's and BWR's and that both high- and low-pressure accident sequences be considered. The Indian Point 2 and Peach Bottom systems were recommended as representative models of the two plant types.

2.1.1 Approaches

The consensus of the members of the SERG is that the occurrence of a steam explosion of sufficient energetics which could lead to containment failure has a low probability. The above perception was reached independently by the various members of the group using several approaches.

The methodology commonly employed to assess the probability of containment failure by steam explosion in quantitative terms involved combinations of:

(i) mechanistic arguments based upon physical models and available data,

(ii) engineering judgment in extrapolation beyond available experimental data,

(iii) probabilistic arguments using subjective assessment of the probabilities of uncertain phenomena.

Individual assessments of steam explosion phenomenology generally involved segmenting the explosion sequence into a series of steps. Arguments raised by individuals stressed aspects of three major areas: (i) initial conditions, (ii) melt-water mixing and conversion, and (iii) slug dynamics and missile generation.

The reader is referred to the individual SERG member responses for the details of the methodologies employed and for the justification for their use provided by the members. Since different methodologies were used it is not suprising, despite the general agreement in qualitative terms cited above, that a range of quantitative estimates of containment failure probability was provided by the members of the group. The objective of this summary is to provide a concise representation of the range of estimates provided by the group. Section 2.1.2 provides a summary of the mechanistic calculation results provided by the members, which focuses on the mass of melt involved in the explosion and on the energy yield of the interaction. Section 2.1.3 summarizes the individual estimates of the probability of containment failure by steam explosions. Section 2.1.4 presents a direct quotation from each SERG member which represents his direct response to Request Number 1 as defined above.

2.1.2 Summary of Mechanistic Calculations

A central feature of many of the individual responses was a mechanistic treatment of various stages of the steam explosion sequence. The focus of these treatments was the estimation of the mass of melt which participates in the explosion and the energy released in the explosion.

These calculations formed one of the bases for the individual estimates of containment failure probability. The reader is referred to the individual responses for the details of the physical models and assumptions used for assessment of the melt mass participating and the energy release. Table 1 summarizes the mechanistic calculations, presents several of the major parameters which enter into the calculation and lists the individual's best estimate of the probability of containment failure by a steam explosion. The results apply to the low-pressure PWR sequence. It is noted that the recorded melt masses and energy yields are not necessarily "best estimate" numbers, in some cases they may be characterized by their authors as conservative. The reader should refer to the individual responses for the precise framework of the calculations and results.

In order to put the information presented in Table 1 into proper perspective, the energy releases should be compared with an estimate of the energy release required to fail containment. It is generally acknowledged, however, that containment failure limits are not simply characterized in terms of energy release or slug kinetic energy. Rather it is a complex function of slug characteristics (mass, shape, void fraction, etc.), dissipation in the upper region of the vessel, dynamics of interaction with the vessel head, etc. A number of the SERG members, however, refer to containment failure limits in terms of required energy release. The containment failure limits referred to by members of the group lie in the range of 1-3 GJ of energy released in the steam explosion.

There is a basic notion that is conveyed by the results shown in Table 1 as compared with the above failure range: the members represented in Table 1 argue that a combination of small melt mass participating in the explosion, and/or a low conversion of thermal into mechanical energy, limits the explosion yield to values which are below the estimates of containment failure energetics.

Table 1 Summary of Assumptions and Mechanistic Calculation Results: Low Pressure PWR*

Investigator	Melt Pour Rate (kg/s)	Pour Diameter (m)	Mixing Depth (m)	Specific Energy Melt (MJ/kg)	Conversion Ratio	Melt Mass Participating (kg)[1]	Explosion Energy Yield (MJ)		Best Estimate Containment Failure Probability
Bankoff[2]	24,000			1.2		24,000[2]	<500		$<10^{-4}$
Corradini[3] (no mixing) (mixing)		4.4	.01 .01 - .10	1.6	.10 - .15 .02 - .05	1,000 1,000 - 10,000	<250 <500		$10^{-4} - 10^{-2}$
Fauske[4]		1.0	.01			700 (Mixing rate 70 kg/ms)	<<Required for Failure		Vanishingly Small (≈0)
Ginsberg[4]		<1.0	.10	1.6	.03	<3,700	<180		4×10^{-3}
Mayinger[4]	1,700	2 subassemblies				2,000			No endangerment of FRG/PWR containment
Theofanous[4]		1.0 1.0	.10 Entire jet[5]	1.32 1.32	.15 .15	1,905 6,350	380 1,260[5]		$<10^{-4}$

*Refer to papers in Appendix C for breakdown of assumptions and calculations.
[1]The total core mass (UO_2+zircaloy) is 130,000 kg for the case considered.
[2]Result shown here is for upper limit of melt mass presented in paper. See Bankoff's Figs 1 and 2 for complete results.
[3]Calculations referred to by Corradini as "example cases for illustrative purposes."
[4]See investigator's paper for precise definition of calculations.
[5]This case referred to by Theofanous as "extremely non-mechanistic."

2.1.3 Summary of Estimates of Probability of Containment Failure by a Steam Explosion

The probability of containment failure by a steam explosion, given a core melt, was generally assessed by dividing the explosion sequence into a number of steps. Each step was assigned a probability of occurrence. The individual probabilities were combined to yield a best estimate and, in some cases, a range of containment failure probabilities. The reader is referred to the individual responses for the details of the models, methodologies and results. Note that the probabilities discussed above are all conditional probabilities.

The overall conditional containment failure probabilities are presented for each respondent in Table 2. Also presented, for comparison, is the result obtained in the Reactor Safety Study, WASH-1400. It is noted that, with two exceptions, those respondents who addressed the BWR issue directly felt that the BWR failure probability would be less than that for a PWR. This was generally associated with the more constrained geometry of the lower plenum in the case of a BWR. In addition, the members felt that the failure probability for high-pressure sequences would be no higher than for the low-pressure sequences. This was generally associated with the increased difficulty in triggering the explosion at higher system pressures.

Tables 3 and 4 show how the SERG members broke down the explosion sequence into stages and assigned probabilities to each stage in the progression. Table 3 presents the information for three broad stages of the explosion sequence:

(i) Initial Conditions - This refers to core meltdown phenomena, including melt coherence, molten pool development, pool temperature, path development for melt transport to the plenum, coherent pour conditions, etc.

Table 2 Summary of Subjective Containment Failure Conditional Probability Estimates

	PWR LOW PRESSURE		OTHER CASES	
INVESTIGATOR	BEST ESTIMATE	UPPER LIMIT	BWR	HIGH-PRESSURE COMMENTS
Bankoff	$<10^{-4}$			
Bohl/Butler	3×10^{-4}	.10		≈Low-P
Briggs	$<10^{-2}$			≈Low-P
Catton	5×10^{-5}		1/10 x PWR	10^{-5}
Cho	WASH-1400 very conservative. Failure very unlikely.			
Corradini	10^{-4} - 10^{-2}		<1/10 x PWR	
Cybulskis	10^{-4}	10^{-2}	10^{-3} (Best)	
Fauske	Vanishingly small (≈0)			
Ginsberg	4×10^{-3}	4×10^{-2}	<PWR	<Low-P
Mayinger	No endangerment of FRG/PWR Containment			
Squarer	10^{-5} - 10^{-4}		Perhaps >PWR	
Theofanous	$<10^{-4}$	$<10^{-4}$		See footnote 2
WASH-1400	10^{-2}	10^{-1}		

[1]See individual contributions for definitions of "upper limit." In general the values in the column represent upper limits to best estimates. The value given by Bohl represents an absolute upper limit, although an unconventional probability definition is employed.

[2]Argues that there will be no high-pressure scenario based upon analysis.

Table 3 PWR Low-Pressure Sequence Subjective Conditional Probability Summary - I

INVESTIGATOR	INITIAL CONDITIONS	MIXING & CONVERSION	SLUG DYNAMICS	FAILURE PROBABILITY BEST ESTIMATE	FAILURE PROBABILITY UPPER LIMIT
Bankoff	————	/////////////	————	$< 10^{-4}$	
Bohl/Butler	.1 - 1.0	.01 - .10	10^{-4} - 1	$3x10^{-4}$	10^{-1}
Briggs	0.8	0.05	0.3	$<10^{-2}$	
Catton	$3x10^{-3}$	$3x10^{-2}$	0.6	$5x10^{-5}$	
Cho	————	/////////////	————	WASH-1400 very conservative. Failure extremely unlikely.	
Corradini	1.0	10^{-4} - 10^{-2}	1.0	————10^{-4} - 10^{-2}————	
Cybulskis	0.9	$9x10^{-3}$	10^{-2}	10^{-4}	10^{-2}
Fauske	————	///////———	///////////	Vanishingly small (≈0)	
Ginsberg	.75	$5x10^{-3}$		$4x10^{-3}$	$4x10^{-2}$
Mayinger	//////////////	————	////////////	No endangerment of FRG/PWR containment	
Squarer	10^{-2} - 10^{-1}	10^{-1}	10^{-2}	————10^{-5} - 10^{-4}————	
Theofanous	————	///////———	///////////	$<10^{-4}$	$<10^{-4}$
WASH-1400	∿1.0	$10^{-1}(+.5, -1)$	$10^{-1}(+.5, -1)$	10^{-2}	10^{-1}

Note: "Hash marks" indicate strong emphasis in investigator arguments.
Solid lines indicate that investigator considered phenomena in this state of sequence.

Table 4 PWR Low-Pressure Sequence Subjective Conditional Probability Summary - II

	Initial Conditions			**Mixing and Conversion**			**Slug**	**Containment Failure Probability**			
Investigator	Establish Pool Conditions	Coherent Pour Conditions	Water Available	Large-Scale Intimate Mixing	Trigger Available	Significant Conversion	Slug Dynamics, Missile, Failure	Best Estimate	Upper Limit	BWR Failure Probability	High-Pressure Comments
Bankoff	————	————	————	////////////	////////////	////////////	————	$<10^{-4}$			
Bohl/Butler	1.0	——0.1 - 1.0——	————	——0.01 - 0.10——	————	————	——10^{-4} - 1——	3×10^{-4}	0.10		≈Low-P
Briggs	——0.8——	————		——0.05——	————	————	0.3	$<10^{-2}$			≈Low-P
Catton	——0.004——	————	.75	.3	1.0	0.10	0.6	5×10^{-5}	---	0.1 x PWR	10^{-5}
Cho	————	————	————		————	————	————	WASH-1400 very conser. Failure very unlikely.			
Corradini	——1.0——	————	————	——10^{-4} - 10^{-2}——	————	————	1.0	——10^{-4} - 10^{-2}——	————	<0.1 x PWR	
Cybulskis	——0.9——	————	————	——9 x 10^{-3}——	————	————	10^{-2}	10^{-4}	10^{-2}	10^{-3} (best)	
Fauske	————	————	————	////////////	————	————	//////////////////	Vanishingly small (≈0)			
Ginsberg	——0.75——	————	————	0.05	1.0	0.10		4×10^{-3}	4×10^{-2}	<PWR	<Low
Mayinger				————	————	————		No endangerment of FRG/PWR Containment			
Squarer	——10^{-1}——	————	10^{-1}*	——10^{-1}——	————	————	10^{-2}	——10^{-5} - 10^{-4}——	————	Perhaps >PWR	
Theofanous	————	————	————	////////////	1.0	————	//////////////////	$<10^{-4}$	$<10^{-4}$		**
WASH-1400	——~1.0——	————	————	——$10^{-1}(+.5,-1)$——	————	——$10^{-1}(+.5,-1)$——	————	10^{-2}	10^{-1}		

Note: "Hash marks" indicate strong emphasis in investigator arguments.
Solid lines indicate that investigator considered phenomena in this stage of sequence.

*For subcooled water.

**Argues that there will be no high-pressure scenario based upon analysis.

(ii) Mixing and Conversion - This refers to the extent and scale of mixing of water and melt in the lower plenum prior to the explosion, and to the process of conversion of thermal energy to mechanical energy imparted to an overlying slug of liquid,

(iii) Slug Dynamics and Missile Generation - This refers to the potential for slug breakup, dissipation in the upper plenum, impact with the head, head failure dynamics, etc.

The reader should refer to the individual responses for a precise definition of each of the probabilities. As a rule, however, the probabilities should be read as an indicator of the likelihood that the individual stage in the sequence would lead to conditions favorable to a large-scale steam explosion. Note that the individual probabilities are multiplied to give the failure probabilities. WASH-1400 results are also presented for comparison.

Table 4 presents a more detailed breakdown of the individual stage probabilities and shows where the individuals placed most heavy emphasis on reduction of the likelihood of conditions favorable to a large-scale steam explosion. Note that there is a general perception, despite individual differences in analysis of the physical mechanisms involved, that mixing and/or conversion ratio limitations significantly reduce the explosion yield and, therefore, the likelihood of large-scale steam explosions. There is, on the other hand, considerable variation in the perception of phenomena involved in the "initial conditions" and "slug" stages of the explosion sequence. These variations are attributed to: (i) time constraints (some subjects could not be addressed in sufficient depth in the available time), (ii) expertise of individuals in some areas and (iii) differences in interpretation of physical phenomena and assignment of the probabilities. The reader is encouraged to read Appendix D for a summary of the range of SERG member views on steam explosion phenomena.

The sense of the SERG members as represented by the numbers shown in Tables 2-4 is that the probability of containment failure by steam explosion is low. Each individual's sense of the term "low" is shown in the tables. An opinion which runs through most of the responses is that the probability of containment failure is reduced due to a limited melt mass participating in the explosion and/or a low thermal to mechanical energy conversion. In addition, however, members assigned various amounts of weight in their arguments to additional phenomena such as the possibility of a large, coherent pour of melt into the lower plenum, and such as the likelihood of a coherent slug impacting the vessel head.

2.1.4 Direct Individual Quotations in Response to Request 1

"What, in your judgment, is the best estimate value for the probability of containment failure arising out of a steam explosion...?"

Bankoff:

> "All these results lead to an overall probability of 10^{-4} or less, purely from the Sandia Monte Carlo calculations, and probably much less. Independently, however, it is seen that the work calculation gives a total work well below that necessary to fail the containment. The other points simply add to the incredibility of such an event."

Bohl/Butler:

> "For a Zion-type PWR the conditional probability of direct containment failure by a missile resulting from a steam explosion, given core melt, is estimated to range from effectively zero to about 0.1. ...A value of $10^{-3.5}$ or $3x10^{-4}$ is our guess at a mean best-estimate value."

Briggs:

"Quantification of the conditional probability of each stage on a subjective but conservative scale, which uses only the range from 0.1 to 0.9 results in an overall probability of containment failure, given core melt, of order 10^{-2}."

Catton:

"Rather, engineering judgment (mine) has been invoked to choose a set of numbers that will give the fraction of core melts that might result in a serious steam explosion. The resulting "chance" of a serious steam explosion is

Low pressure	$P = 5 \times 10^{-5}$
High pressure	$P = 10^{-4}$

The two sets are close enough to one another that I would use 10^{-4} as the conditional probability of containment failure by steam explosions."

Cho:

"I am reluctant to give a probability number since I do not know how to derive one in a satisfactory manner. However, I do feel that the WASH-1400 estimates (10^{-2} for PWRs) are very conservative."

Corradini:

"Based on all the above discussion I would estimate that the likelihood of a steam explosion with fuel-coolant mixing and a conversion ratio that would produce an energy exceeding 1-2 GJ to be very small. I would assign a probability for this event of 0.01 to 0.0001."

Cybulskis:

"My estimate of the probability of direct containment failure due to in-vessel steam explosions in large dry containment PWR's is 1E-4 with a range of 1E-6 to 1E-2. The corresponding probability for Mark I BWR containments is 1E-3, with a range of 1E-4 to 1E-2."

Fauske:

"With the large margin between the required mass of core debris and the calculated masses which could interact, the probability of early containment breach due to a steam explosion occurrence is assessed to be vanishingly small."

Ginsberg:

"A subjective estimate of the (conditional) probability of containment failure is arrived at by dividing the explosion sequence into several stages. A subjective judgment is defined for each stage, and a judgment of the probability is made on the basis of physical and intuitive arguments. A subjective probability of 0.0038 is computed as the 'best guess' for the low-pressure PWR case considered here."

Mayinger:

"A detailed survey of the international literature and also studies performed in our country showed that the integrity of the pressure vessel or of the containment structure of a modern German pressurized water reactor would not be endangered due to a steam explosion. This statement is valid without raising a loan, from probabilistic studies or from deliberations, with what probability which course of any severe accident may occur. So in risk studies steam explosions leading to an early contamination of the environment should not play any role."

Squarer:

"In response to your first question, my judgment for the best estimate value for the probability of containment failure arising from a steam explosion, given a core melt, is of the order of 10^{-4} per core melt."

Theofanous:

"We develop the position that the alpha-mode of containment failure in LWR's postulated to suffer any core melt scenario is physically unreasonable. Quantitatively, this wording implies a probability range below 10^{-6}. However, with due regard to the extreme nature of the phenomena considered, we believe that on a scale commensurate with quantification of the so-called front-end of a PRA, a probability range below 10^{-4} would be more appropriate."

2.2 Response to Request Number 2: Critique of Sandia Steam Explosion Uncertainty Study, NUREG/CR-3369

"Please provide written comments on the Sandia study described in NUREG/CR-3369 and any additional inputs and analyses which you feel are relevant to this study. As pointed out above the SNL report is based on assumed uniform distributions and some additional calculations, assuming other distributions may be required."

The reader is referred to the individual SERG responses for the detailed comments by each member of the group. The summary below highlights the major criticisms of the Sandia report which were common to the responses by nearly all members of the group and for which there was broad agreement.

2.2.1 Methodology

The purpose of the Sandia report is to provide a measure of the uncertainty of the conditional probability of containment failure by steam explosion. The methodology employed consists of:

(i) a physical model of the steam explosion sequence of events,

(ii) the Monte-Carlo technique for estimation of the uncertainty in probability of containment failure by steam explosion by combination of uncertainties in steam explosion parameters chosen to represent the physical model.

With broad consensus the SERG members agreed that the methodology as used in NUREG/CR-3369 was inadequate for the purpose of establishing the uncertainty in the probability of containment failure by a steam explosion. The reasons for this finding are described below.

The most commonly expressed attitude of the members was that the heart of the uncertainty estimation procedure was specification of the probability density functions (PDF) for the uncertain parameters which were chosen to represent the physical model of the explosion sequence. The major criticism of the methodology is that the authors did not make direct use of physical models of aspects of steam explosion phenomenology which are representative of current thinking of most of the SERG members. This, led the authors to large uncertainties in the phenomenology and thus in the values of the parameters.

The rectangular distributions within extremely broad limits reflects the notion that little is known about a particular parameter and its underlying physical process. The feeling of the SERG is that the use of such distributions does not adequately represent the current state of knowledge regarding steam explosion phenomenology. It is felt that we do indeed know more about many of the various processes and parameters than is implied in the report by the choice of the rectangular distributions.

The most commonly-cited example of the above criticism is the authors' choice of parameters which govern the mass of melt involved in the steam explosion. Here the authors use a range of parameters which lead to the possibility of up to 94,000 kg of melt involved in an explosion. While mixing limitations are discussed in the report, it is concluded that "...at present, evidence does not exist to allow an upper limit to be imposed on the mass of melt mixed that is less than all the available melt." With broad agreement the SERG members believe that this approach overstates the uncertainty and that information exists in the literature to support limits-to-mixing arguments which would lead to a significantly reduced range of melt mass participating in a steam explosion.

There was considerable support among SERG members for the use of the Monte-Carlo method as a method of combining uncertainties, provided that the probability density functions are chosen to adequately represent the physical phenomena.

2.2.2 On the Conclusions and Implications of the Sandia Study

The major conclusion of the Sandia study which will be addressed here is that:

"The calculations in this report refer to in-vessel steam explosions at ambient pressures near to atmospheric. Numerical values were taken from the Zion reactors. They show that the conditional probability of containment failure, given core melt during a low-pressure accident, is extremely uncertain. Indeed the results span the range of probability from 0 to 1."

The members of the SERG, with broad consensus, strongly felt that the conclusion that the probability of containment failure lies between 0 and 1 is not representative of the state of knowledge of steam explosions and is simply a consequence of the inappropriate choice of the probability density functions, as discussed above.

2.3 Response to Request Number 3: Review of SNL Proposed Steam Explosion Research Program

"Review and comment on SNL's proposed research program in particular:

(i) provide your opinion as to whether these experiments or other experiments and/or analyses will contribute to resolution of residual issues and to a further reduction of uncertainties.

(ii) provide your recommendations on pre-experiment analyses or experimental procedures which will maximize the usefulness and effectiveness of the tests."

2.3.1 SERG Views on Continuing Need for Steam Explosions Research

The discussions of the need for continuing steam explosion research were influenced by

(i) the generally-perceived low probability of containment failure as a result of a steam explosion, as reported in Section 2.1 above,

(ii) the lack of fundamental understanding in key areas of steam explosion phenomenology as indicated in the Summary of Comments on Steam Explosion Phenomena (Appendix D).

With these perceptions in mind, a concensus developed for the need for confirmatory research directed towards verification of the SERG conclusion that the probability of containment failure by a steam explosion is low.

The experimental research at this time, it was generally agreed, should continue with experiments on the scale of the FITSX program, i.e., with melt mass in the range up to 50-100 kg.

Consensus could not be reached on the advisability of pursuing the proposed SEALS test program, involving melt mass up to 2000 kg, at this time.

The group has reviewed the merits of the SNL research proposal in light of its review of the current understanding of steam explosion phenomenology. We did not attempt to independently develop a program for improving our understanding of steam explosion phenomenology and for further reducing uncertainties in estimates of a-mode failure probabilities.

2.3.2 Summary of Discussions on SNL Proposed Research Program

The reader is referred to the individual responses for detailed comments on the merits of the various elements of the proposed SNL experimental program. This summary will focus on the conclusions reached by the SERG on the merits of the SNL program. The conclusions were reached as a result of discussions at the SERG meeting on November 27, 28 and were further defined by additional written submittals by individual SERG members.

The SNL proposed research program was reviewed at the meeting. Dr. Marshall Berman presented the recent results of the experimental program and discussed the conceptual designs of the proposed experimental programs and the associated analyses. The major elements of the program which were discussed are:

(i) SEALS - steam explosion at large scale; up to2000 kg mass

(ii) FITSX - fully instrumented tests; up to 50-100 kg melt mass

(iii) ACM - stratified (alternate contact mode) explosion tests; up to 50-100 kg melt mass

(iv) SHIP - small scale high pressure tests

The following represents a summary of the SERG conclusions with respect to each of the proposed SNL programs.

SEALS

The stated objective of the SEALS program is:

"Determine the effects of scale on coarse mixing and conversion ratio for steam explosions at an ambient pressure of one Albuquerque atmosphere (0.83 bars)."

"SEALS will address the amount of melt that can mix and participate in a steam explosion by means of measuring the conversion ratio as a function of several important parameters. SEALS will be capable of preparing and delivering thermitic melts up to 2000 kg into a 1/2 scale reactor lower plenum." The major parameter was to be the melt mass delivered.

The SERG did not come to a consensus opinion about the proposed SEALS program.

Four members of the committee feel that the SEALS tests based on the conceptual design proposed would be needed (Briggs, Cho, Corradini, Ginsberg). This opinion was based, primarily, on the perceived need for experiments which would provide a basis for evaluation of the effect of scale on melt-water mixing.

Bohl would support the large scale tests if additional steam explosion tests are to be performed. In his opinion, however, modeling of core meltdown phenomena represents a higher priority.

Theofanous believes that a large scale test program such as SEALS may be potentially useful. At present, however, the drawbacks that he has pointed out in his response (see Appendix C-12), together with the perceived low probability of containment failure by steam explosion, lead him to conclude that a more useful approach would be to pursue more detailed modeling of the initial condition phase of the steam explosion phenomenon.

Cybulskis feels that larger scale experiments will probably be needed to resolve the outstanding issues. He feels, however, that the SEALS program is premature and should not be implemented at this time. He feels that improved modeling and experimental diagnostics have higher priority.

Catton believes that before any steam explosion experimental program is implemented, a review must be carried out to establish that the program will provide the needed physical insights. He feels that the seals tests are premature and that a great deal more basic understanding is required before large scale tests are conducted. Large scale tests may be reasonable at some time in the future, in which case he would envision one or two experiments directed toward proof-of-understanding.

Bankoff, Fauske and Mayinger feel confident enough about the low probability of containment failure based on their analyses that the SEALS tests are not considered warranted. Bankoff feels that the SEALS tests are certainly premature. Among his reasons are: (i) instrumentation techniques need to be improved at smaller scale. In order to obtain adequate data on initial conditions in constrained geometry before the scale is increased; x-ray, refractory resistivity probes and impact plates equipped with accelerometers and strain gauges are suggested; (ii) the measurements should yeild information directly on the initial fuel and coolant concentration at the time of triggering and the work potential of the upward moving water slug; (iii) a computer analysis should take the measured initial conditions and predict the pressure signatures, upward velocities and the work potential in agreement with data. When this capability has been achieved in small scale constrained geometries the move to larger scales can then be considered; and (iv) a combination of low containment failure probability and high cost of the experimental program would warrant further work related to items (i)-(iii) before proceeding with large-scale tests.

Squarer would agree with the need for a modified version of the SEALS tests and design concept, if one felt that the probability of alpha-mode failure is too large to live with (i.e., closer to 10^{-1} than 10^{-4}) and the uncertainty in determining this probability is excessively large (i.e., span evenly the above range).

FITSX

The stated objective of the FITSX program is:

"Investigate FCI behavior for up to 50 kg of various thermitic melts in the existing FITS vessel. Measure conversion ratio, steam and hydrogen generation, debris characteristics, and fission product release for normal (melt dropped into water) and alternate (water onto melt) contact modes."

SERG members agreed (with the exceptions noted below) with the need for a continuing steam explosion test program on the scale of the FITSX program, i.e. up to 50-100 kg melt masses. While agreeing on the general objectives as characterized by SNL in the above quotation, the SERG was not presented with, and did not discuss, the proposed FITSX program in any detail. It is noted that the proposed FITSX program would address several aspects of steam explosion phenomenology which would impact both the alpha-mode and other containment loading mechanisms (for example, hydrogen and steam generation from steam explosions).

The following recommendations regarding the proposed FITSX test program were made: (i) that the effect of solid structure in the explosion zone be explicitly investigated, (ii) that the FITSX test employ rigid wall chambers with prototypic materials and prototypic fuel/coolant mass ratios to more closely model the process in the lower plenum, (iii) that more diagnostic instrumentation (especially X-ray motion pictures) be developed for measuring the explosion conversion ratio and the extent of fuel-coolant mixing.

It is noted that Bohl and Cho feel that the SEALS program should take first priority over the FITSX program. They believe that the major uncertainty in steam explosion phenomenology is associated with scaling and that the SEALS program is the best of the proposed vehicles for evaluation of its effect in mixing and conversion ratio.

Catton feels that the FITSX program should be reviewed in detail prior to implementation.

ALTERNATE CONTACT MODE (ACM) EXPERIMENTS

A test series was proposed by Sandia as part of their proposed FITSX program. The SERG considered this program separately, however, and agreed on the need for fundamental ACM experiments in which the fuel and coolant are set in a stratified geometry. This geometry is quite possible in the postulated accident situation and lends itself to an measurement and fundamental understanding of the proposed "pre-mixing length scale" and the "coupling length scale" (see Appendix D). The SERG agreed with the concept of the proposed test program.

SHIP

The stated objective of this test program is:

"Determine the dependence of explosion triggering and conversion - ratio on ambient pressure up to 170 bars."

The SERG gave no priority to the SHIP tests, although there appeared to be no general disagreement with the concept of small-scale high pressure tests.

Catton suggests that the size of the molten mass chosen for these tests must be carefully considered.

AUG 24 1984

Dr. George Bankoff
Chemical Engineering Department
Northwestern University
Evanston, IL 60201

Dear Dr. Bankoff:

The NRC is currently reviewing the status of resolution of the steam explosion issue so that the alpha-mode of containment failure can be placed in proper perspective for various applications within the Severe Accident Research Program (SARP). We are soliciting your expert opinion and judgement on several aspects of the steam explosion issue and plans for future research to assist us in our review.

Background on Steam Explosion Issues

During the NRC/IDCOR meeting on Accident Phenomenology and Containment Loading at Harpers Ferry, on November 28-31, 1983, a consensus was reached on a number of key elements of the steam explosion issue which can be summarized as follows:

1. Both IDCOR and staff* agreed that spontaneous explosions can occur under appropriate conditions.

2. Regarding direct containment failure probability from in-vessel steam explosion-driven missiles, while IDCOR believes that this is essentially impossible, the staff believes that explosions of high energy (2000MJ) needed to fail containment are deemed unlikely, but they have not been demonstrated to be impossible.

3. Areas of major disagreement between the staff and IDCOR included:

   (a) Quantity of participating materials.

      o IDCOR: Severely limited to 10 - 100 Kg range.

      o Staff: May be limited to 1000 - 4000 Kg in-vessel and 10,000 - 20,000 Kg ex-vessel.

* Staff here refers to both members of the NRC staff and a number of its consultants and contractors.

(b) Propagation and conversion ratio.

- o IDCOR: Large fragments preclude an efficient, i.e., "energetic" explosion; propagation and conversion ratio are not relevant because "necessary premixture is precluded by CHF and mixing energy arguments."

- o Staff: Current knowledge permits conversion ratios from 0 - 15%.

Additional information is provided in Enclosure 1 which is a summary of the steam explosion phenomena issue from the Harpers Ferry meeting mentioned above.

<u>Sandia Report</u>

Recently SANDIA published a report titled "An Uncertainty Study of PWR Steam Explosions," NUREG/CR-3369, SAND 83-1438 (Enclosure 2). In this report the SANDIA investigators say that "the conditional probability of containment failure, given core melt during a low pressure accident, is extremely uncertain." They then went on to say that "Indeed the results span the range of probability from 0 to 1." It is also pointed out in this study that "this uncertainty estimate (i.e., the range of 0 to 1) is derived from the particular choice of distributions and combinations thereof used."

In this study Sandia has divided the parameters involved in calculating the probability of containment failure into three uncertainty ranges. Based upon their assumption of uniform distributions in the lower, middle and upper uncertainty ranges the containment failure probabilities estimated for these three ranges are as follows:

- lower range, P=0
- middle range, $P=10^{-4}$
- upper range, P=1.

The authors also caution that the middle range result, i.e., the 10^{-4} value, "should not be used as a best estimate of the fraction of core melt accidents leading to containment failure by steam explosions."

These results are at variance with previous studies, especially the conclusion on the range of probabilities. Among these studies are the conclusions presented in WASH-1400 and, more recently in the staff's Zion/Indian Point study reported in NUREG-0850. Both of these documents provide more limited ranges of conditional probabilities. As noted in the SNL report the WASH-1400 study adopted a range for this conditional probability of 10^{-1} to 10^{-4} with a median value of 10^{-2}. In the Zion/Indian Point study the staff concluded that the probability of a steam explosion induced failure of containment was at least two orders of magnitude lower than the 10^{-2} median value given in WASH-1400.

The authors of the SNL report go on to identify a number of other studies which have, for the most part, accepted the concept of a narrower range of conditional probabilities. These include the German Risk Study, the UKAEA PWR Degraded Core Analysis Report, the Report of the Swedish Government Committee on Steam Explosions, Fauske & Associates, Inc., Theofanous and Saito, Swenson and Corradini and Mayinger.

In the light of the above our request in this area is the following:

1. What, in your judgment, is the best estimate value for the probability of containment failure arising from a steam explosion, given a core melt, that we should be using in the severe accident program. As part of your response please provide the bases (experimental and/or analytical) and the approach used in reaching your conclusions.

2. Please provide written comments on the Sandia study described in NUREG/CR-3369 and any additional inputs and analyses which you feel are relevant to this study. As pointed out above the SNL report is based on assumed uniform distributions and some additional calculations, assuming other distributions, may be required.

3. You should plan to attend a two-day meeting in the Washington, D.C. area during the week of October 15, 1984 to discuss your response to items 1 and 2, to review the NRC research program, and to assess the comments on the Sandia study and the various inputs from the experts on containment failure probability.

Research to Resolve Issues

Sandia has proposed a research program to resolve the steam explosion issues consisting of continuation of small scale experiments, new medium and large scale experiments (400 Kg to 2000 Kg melts) and continuation of FCI model development and application. This research program was discussed at an Information Exchange meeting on molten core coolant interactions on June 4 and 5, 1984 in New Orleans, where some of you were in attendance. Enclosed is an updated version of the material presented at that meeting (Enclosure 3). By a separate letter we are asking SNL to provide further details and justification for the specific experiments they propose to resolve the issues, in particular issues (a) and (b) noted in item 3 under Background on Steam Explosion Issues above. This will include a request for elaboration and discussion of the questions, and/or comments which were raised during the New Orleans meeting. We will forward SNL's response to you promptly for your review and comment. We solicit your opinion as to whether these experiments or other experiments and/or analysis will contribute to resolution of the residual issues and to a further reduction of uncertainties. In addition we would appreciate any recommendations on pre-experiment analysis or experimantal procedures which, if followed, will maximize the usefulness and effectiveness of the tests. We also plan to discuss SNL's research program recommendations and your comments and suggestions at the meeting in October.

Your participation in this effort will be extremely valuable to the final preparation of a number of significant near-term research products currently planned under SARP and is sincerely appreciated. If you have any questions on our request please call me. I have assigned Mr. Cardis Allen to coordinate these activities including the organization and agenda for the October meeting. Mr. Allen will serve as the Executive Secretary, and will assure among other things, that licensing interests (e.g., board notifications) are served. Mr. Allen can be reached at (301) 492-7932.

Sincerely,

Denwood F. Ross

Denwood F. Ross, Deputy Director
Office of Nuclear Regulatory Research

Enclosures: As stated

cc: Dr. I. Catton
Dr. M. Corradini
Dr. T. Theofanous
Dr. T. Ginsburg
Dr. W. Bohl
Dr. D. Cho
Dr. D. Squarer
Dr. A. Briggs
Mr. P. Cybulskis
Mr. T. Butler
Dr. F. Mayinger

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September 11, 1984

To: Committee Members
Steam Explosion Experts Group

From: Michael Corradini
Assistant Committee Chairman

Mr. C. Allen of NRR and T. Ginsberg, the Committee Chairman, have requested that I compile a list of meetings and open publications where the Sandia steam explosion work has been reviewed and/or analyzed by other investigators in regard to direct containment failure (i.e. 'alpha-mode' failure). I have compiled an initial list of ~~documents~~ which are described in Enclosure 1. This is by no means a complete list of steam explosion phenomena, rather it is meant to be a list of only those documents analyzing and/or utilizing Sandia steam explosion research work in regard to alpha-mode failure.

I request that all committee members review this compilation and the attached documents to check for completeness and to familiarize themselves with past investigations. If certain pertinent documents have been inadvertently omitted from this compilation, it is the duty of the committee member to inform me or Mr. Allen or Ginsberg and to personally distribute the documents to the rest of the committee members. In this way all members will be furnished with complete and up-to-date information.

Enclosure 1

Past Critical Reviews of the Sandia Steam Explosion Work

I. Meeting Reviews (see attached documents)

   A. Zion/Indian Point Risk Studies Review Meeting, April 1982
   B. Technical Review Meeting on Steam Explosions, May 1982
   C. Molten-Core-Coolant Interaction Research Review Group, June 1984

II. Bibliography of Important Past Reviews of Sandia Experiments and alpha-mode failure

III. Key Papers on the Critical Review of Sandia Experiments and alpha-mode failure (see attached documents)

II. Bibliography

1. S.G. Bankoff, "Vapor Explosions: A Critical Review," Proc. 6th Int'l. Heat Transfer Conference, 6 p 355 (1978).

2. S.G. Bankoff et al., "Steam Explosions - Their Relationships to LWR Safety Assessments," Int'l. Mtg. on Thermal Reactor Safety, Chicago, IL (August 1982).

3. S.G. Bankoff et al., "A Model for Fragmentation of Molten Metal Oxides in Contact with Water," Int'l. Mtg. on LWR Severe Accident Evaluation, Cambridge, MA (August 1983).

4. M.J. Bird, "Thermal Interactions Between UO_2 and Water: Experimental Study Using Thermite-Generated UO_2," Fuel-Coolant Interactions, HTD-V19, ASME Winter Annual Mtg., Wash. DC (November 1981).

5. M.J. Bird, "An Experimental Study of Scaling in Core Melt/Water Interactions," ASME 22nd NHTC, No. 84-HT-7, Niagara Falls, NY (August 1984).

6. C. Carachalios et al., "A Transient Two-Phase Model to Describe Thermal Detonation Based on Hydrodynamic Fragmentation," Int'l. Mtg. on LWR Severe Accident Evaluation, Cambridge, MA (August 1983).

7. D.H. Cho et al., "Some Aspects of Mixing in a Large-Mass Energetic FCI," Int'l. Mtg. on FRS, CONF-761001, V4, Chicago, IL (October 1976).

8. M.L. Corradini, D.E. Mitchell, L.S. Nelson, "Recent Experiments and Analysis Regarding Steam Explosions," Fuel-Coolant Interactions, HTD-19, 49 (1981).

9. M.L. Corradini, "Phenomenological Modelling of the Triggering Phase of Small Scale Steam Explosion Experiments," Nuclear Science and Engineering, 78, 154 (1981).

10. M.L. Corradini, D.V. Swenson, R.L. Woodfin, L.E. Voelker, "Probability of Containment Failure Due to Steam Explosions Following a Postulated Core Meltdown Accident in a Light Water Reactor," Nuclear Engineering and Design, 66, 287 (1981).

11. M.L. Corradini, "A Proposed Model for Fuel-Coolant Mixing During a Core-Melt Accident," Int'l. ANS/ENS Mtg. on Thermal Reactor Safety (August 1982).

12. M.L. Corradini, "Hydrogen Generation During Molten Fuel-Coolant Interactions," Int'l. Mtg. on Hydrogen Behavior (October 1982).

13. M.L. Corradini, D.V. Swenson, R.L. Woodfin, "An Analysis of Containment Failure by a Steam Explosion Following a Postulated Core Meltdown Accident in an LWR," Nuclear Safety, 23(1), 21 (1982).

14. M.L. Corradini, "Analysis and Modelling of Large Scale Steam Explosion Experiments," Nuclear Science and Engineering, 82 (December 1982).

15. M.L. Corradini, "Modelling Film Boiling Destabilization Due to a Pressure Shock Arrival," Nuclear Science and Engineering, 84 (1983).

16. M.L. Corradini, G.A. Moses, "A Dynamic Model for Fuel-Coolant Mixing," Int'l. Mtg. on Severe Accident Evaluation, Cambridge, MA (August 1983).

17. M.L. Corradini, N.A. Evans, D.E. Mitchell, "Hydrogen Generation During Fuel-Coolant Interactions," Int'l. Mtg. on Severe Accident Evaluation, Cambridge, MA (August 1983).

18. M.L. Corrdini, "Fuel-Coolant Interactions with Molten Core Materials and Water," Nuclear Science and Engineering 86 (1984).

19. M.L. Corradini, "Limits to Fuel-Coolant Mixing," Nuclear Science and Engineering (submitted May 1984).

20. H.K. Fauske, R.E. Henry, "Interpretation of Large Scale Vapor Explosion Experiments with Application to LWR Accidents," Int'l. Mtg. on LWR Severe Accident Evaluation, Cambridge, MA (August 1983).

21. R.E. Henry, H.K. Fauske, "Core Melt Progression and the Attainment of a Permanently Coolable State," Proc. Thermal Reactor Fuels Mtg., Sun Valley, ID (August 1981).

22. R.E. Henry, H.K. Fauske, "Required Initial Conditions for Energetic Vapor Explosions," Fuel-Coolant Interactions HTD-V19, ASME Winter Annual Meeting, Wash. DC (November 1981).

23. R.E. Henry et al., "Vapor Explosion Potentials Under LWR Hypothetical Accident Conditions," ANS Thermal Reactor Safety Mtg., Sun Valley, ID (March 1981).

24. W. Schwalke et al., "Investigations on Shock Waves in Large Scale Vapor Explosions," Fuel-Coolant Interactions, HTD-V19, ASME Winter Annual Meeting, Wash. DC (November 1981).

25. D. Squarer and M. Leverett, "Steam Explosion in Perspective," Int'l. Mtg. on LWR Severe Accident Evaluations, Cambridge, MA (August 1983).

26. T.G. Theofanous, M. Saito, "An Assessment of Class-9 (Core-Melt) Accidents for PWR Dry-Containment Systems," Nuclear Eng. & Design, 66, No. 3 (1982).

27. P. Turinsky et al., "Boiling Film Growth in a Core-Melt-Water System," 2nd Proceedings of Nuclear Thermal-Hydraulics, ANS Annual Mtg., New Orleans, LA (June 1984).

28. H. Unger et al., "The Role of Steam Vapor Explosions during Core Meltdown of LWR's," Int'l. Mtg. on Thermal Reactor Safty, Chicago, IL (August 1982).

Sandia National Laboratories

Albuquerque, New Mexico 87185

date: September 14, 1984

to: Distribution

from: M. Berman, 6427

subject: Steam Explosion Publications

At the request of the NRC, we are distributing copies of Sandia reports and memos related to the Core Melt-Coolant Interactions Program. If you have not already received this package, it should be arriving shortly. All the people on the Steam Explosion Experts Group should already be familiar with most of this information. However, since much of this material provides a background for the Group's deliberations, please familiarize yourself with those reports which you may not have seen previously. An extensive summary of work in this program was sent to you earlier in an attachment to the letter from Denwood F. Ross, Deputy Director of the Office of Nuclear Regulatory Research on August 24.

If you need additional information or assistance, please call me at 505-844-1545.

Distribution:
Dick Anderson, Argonne National Laboratory
Dr. George Bankoff, Northwestern University
Michael Bird, AEE Winfrith, UK
Dr. W. Bohl, LANL
Thomas Butler, LANL
Dr. I. Catton, University of California
Dr. D. Cho, Argonne National Laboratory
Michael L. Corradini, University of Wisconsin
Mr. P. Cybulskis, Battelle Columbus Laboratories
Mario Fontana, Technology for Energy Corp.
Dr. T. Ginsberg, Brookhaven National Lab
Prof. Franz Mayinger, Technische Universitat Hannover. FRG
Dr. D. Squarer, EPRI
Prof. Theo G. Theofanous, Purdue University
John Telford, US NRC

10/1/84:
H. K. Fauske, FAI
C. L. Allen, USNRC

October 2, 1984

Dr. George Bankoff
Chemical Engineering Department
Northwestern University
Evanston, IL 60201

Dear Dr. Bankoff:

Discussions with several group members and NRC staff suggest that some elaboration would be appropriate regarding the questions posed to the Steam Explosion Review Group in Dr. Ross's letter of August 24, 1984.

The first of Dr. Ross's three requests is; "What, in your judgement, is the best estimate value for the probability of containment failure arising from a steam explosion, given a core melt, that we should be using in the severe accident program? As part of your response please provide the bases (experimental and/or analytical) and the approach used in reaching your conclusions."

The question is a very broad one and answers to it may be system- and sequence-dependent. We believe it would be helpful to be more specific so as to provide a common basis for calculations. We recommend that you distinguish between PWRs and BWRs in your responses. In particular, for the PWR response, we suggest you use the Indian Point-2 reactor as a specific model since plant data and core melt analyses are readily available in many existing reports. For similar reasons we recommend using the Peach Bottom Mark-I reactor as the specific BWR model. By using these specific models your analyses can take into account, on a consistent basis, the importance of the amounts of molten corium and water, the structural and thermal constraints and the distances and times involved. In addition, we suggest that you consider the implications of both high-pressure and low-pressure reactor vessel conditions at the time of the postulated interaction. These conditions can be developed from station blackout and pipe break accidents.

Focusing on realistic conditions that would be encountered in core melt events will help in making proper use of existing data and will aid in our review of potential MCCI research needs. In that regard Dr. Ross's third request asks, in part, that this review group meet "...to review the NRC research program, and to assess the comments on the Sandia study and the various inputs from the experts on containment failure probability." You will be receiving material from Marshall Berman regarding SNL's proposed research program by October 29, 1984. In commenting on the proposed SNL program please include a prioritization of the proposed experimental efforts in terms of their necessity and importance.

Sincerely,

/S/ Cardis Allen

for Theodore Ginsberg

/S/

Cardis Allen

Distribution
Central File
C. Allen
D. Ross
T. Speis
R. Bernero
R. Curtis
J. Telford

September 17, 1984

Dr. George Bankoff
Chemical Engineering Department
Northwestern University
Evanston, IL 60201

Dear Dr. Bankoff:

A letter was sent to you on August 24, 1984 soliciting your participation in the review of several matters concerned with steam explosions. We understand that you have agreed to participate in this effort.

Ted Ginsberg and Mike Corradini have been asked to serve as Chairman and Vice Chairman respectively. Both have agreed to do so.

A schedule of activities for this review has been developed as follows:

1. Marshal Berman has agreed to distribute to the group all relevant SNL generated material on the molten material-coolant interaction programs at SNL. He has agreed to provide this information by September 17, 1984.

2. Mike Corradini has agreed to distribute to the group all relevant material relating to reviews and comments on the work performed by SNL on this subject. He has also agreed to provide this information by September 17, 1984.

3. It is requested that all members of the group submit their recommendations regarding relevant material which they believe has not been included in the materials distributed by Marshal Berman and Mike Corradini. We would appreciate it if you could provide to all members either a copy of each reference or, for that material easily accessible, a complete citation. This should be completed by October 1, 1984.

4. Marshal Berman will distribute to the group further material regarding SNL's proposed research program in this area. This will be completed by October 29, 1984.

5. Written responses from each member of the group to requests 1 and 2 on page 3 of the August 24 Ross letter should be submitted to all members by November 2, 1984.

Requests 1 and 2 from the Ross letter are repeated here for convenience.

1. What, in your judgment, is the best estimate value for the probability of containment failure arising from a steam explosion, given a core melt, that we should be using in the severe accident program. As part of your response please provide the bases (experimental and/or analytical) and the approach used in reaching your conclusions.

2. Please provide written comments on the Sandia study described in NUREG/CR-3369 and any additional inputs and analyses which you feel are relevant to this study. As pointed out above the SNL report is based on assumed uniform distributions and some additional calculations, assuming other distributions, may be required.

We note here that the probability estimate requested in item 1 includes consideration of the spectrum of potential melt-down sequences.

6. Written responses from each member of the group to the request discussed at the bottom of page 3 of the August 24 letter regarding SNL's proposed research program should be distributed to all members by November 9, 1984. Our request is restated here as follows:

   Review and comment on SNL's proposed research program in particular;

   (i) Provide your opinion as to whether these experiments or other experiments and/or analyses will contribute to resolution of residual issues and to a further reduction of uncertainties.

   (ii) Provide your recommendations on pre-experiment analyses or experimental procedures which will maximize the usefulness and effectiveness of the tests.

7. An agenda for the meeting to discuss these reviews and comments will be issued by November 11, 1984. Note that only material submitted in accordance with items 1-6 will be considered at the meeting. The intent is that all relevant material will be in the participants hands well before the meeting so that ample time is available for review of these materials before the meeting takes place.

8. A meeting will be held at the "Cliffside Inn" in Harper's Ferry, West Virginia on November 27, 28 of 1984. Reservations can be made by calling (304) 535-6302. I understand a special rate may be in effect so you should refer to the "NRC meeting" when making reservations.

9. A meeting summary will be drafted and circulated by December 24, 1984. In addition to providing a record of the material provided and discussed at the meeting the summary will identify and describe all significant areas of agreement as well as any residual issues.

A line chart depicting the schedule for the activities described above is attached for your convenience. The milestone dates given above are identified on the chart. The chart is keyed to the item numbers in the text.

The addresses and telephone numbers of all members of the group are also enclosed for your convenience.

If you have any questions please contact our Executive Secretary, Cardis Allen on (301) 492-7932 or (301) 492-9737.

Sincerely,

for /s/ Cardis Allen
Ted Ginsberg, Chairman

/s/
Cardis Allen, Executive Secretary

Enclosures:
As stated

<u>Distribution</u>
Central File W/O Encl.
C. Allen

Enclosure 2

LIST OF EXPERTS TO REVIEW:

STEAM EXPLOSIONS

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Chemical Engineering Department
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Evanston, IL 60201 - Phone: (312) 492-5267

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Phone: 49-089-2105-2520

Cardis Allen
U. S. Nuclear Regulatory Commission
Washington, D.C. 20555
(301) 492-9737

SCHEDULE OF ACT. . .IES FOR
STEAM EXPLOSION REVIEW GROUP

Sep. Oct. Nov. Dec. Jan.

1. Materials on SNL studies from M. Berman	9-17
2. Materials on reviews of SNL studies from M. Corradini	9-17
3. Other relevant materials from all participants	10-1
4. Further input on SNL proposed research from M. Berman	10-29
5. Written responses to requests 1 and 2 from all participants	11-2
6. Written responses on review of SNL proposed research from all participants	11-9
7. Meeting agenda from Exec. Secy.	11-14
8. Meeting at Harpers' Ferry	11, 27-28
9. Meeting summary from Exec. Secy.	12-24

Enclosure 1

AGENDA

STEAM EXPLOSION REVIEW MEETING
HARPERS FERRY, WVA
NOVEMBER 27-28, 1984

NOVEMBER 27, 1984

0830 - 0845	Introduction	T. Speis
0845 - 0900	Meeting Format and Ground Rules	T. Ginsberg
0900 - 1030	Individual presentations of responses to Q.1 and Q.2 (Maximum of 15 minutes each)	See Attached List
1030 - 1045	Break	
1045 - 1200	Continue	
1200 - 1300	Lunch	
1300 - 1515	Continue	
1515 - 1530	Break	
1530 - 1730	Open Discussion of Responses to Q.1 and Q.2	All
1730 - 1930	Dinner	
1930 - 2130	Open for use as required (TBD)	

NOVEMBER 28, 1984

0830 - 0900	Summary of Conclusions on State of Knowledge Regarding Steam Explosion Phenomena	M. Corradini
0900 - 0930	Summary of Conclusions on State of Knowledge Regarding Estimates of α Mode Failure Probability	T. Ginsberg
0930 - 1015	SNL Proposed Research Program	M. Berman
1015 - 1030	Break	
1030 - 1200	Discussion of Comments on Research Program (Responses to Q.3)	All
1200 - 1300	Lunch	
1300 - 1515	Continue	
1515 - 1530	Break	
1530 - 1600	Summary of Comments on Research Program	T. Ginsberg

ATTACHMENT

ORDER OF INDIVIDUAL PRESENTATIONS FOR
THE STEAM EXPLOSION REVIEW MEETING
ON NOVEMBER 27

1. G. Bankoff
2. W. Bohl
3. A. Briggs
4. T. Butler
5. I. Catton
6. D. Cho
7. M. Corradini
8. P. Cybulskis
9. H. Fauske
10. T. Ginsberg
11. F. Mayinger
12. D. Squarer
13. T. Theofanous
14. M. Berman

SERG MEETING
HARPERS FERRY, WVA
NOVEMBER 27, 1984

LIST OF ATTENDEES

SERG MEMBERS	AFFILIATION
C. Allen	USNRC/NRR/DST
S. Bankoff	Northwestern University
M. Berman	Sandia National Laboratory
W. Bohl	Los Alamos National Laboratory
A. Briggs	U.K.A.E.A.
T. Butler	Los Alamos National Laboratory
I. Catton	ACRS Consultant (UCLA)
D. Cho	Argonne National Laboratory
M. Corradini	University of Wisconsin
P. Cybulskis	Battelle Columbus Laboratory
H. Fauske	Fauske Associates, Inc.
T. Ginsberg	Brookhaven National Laboratory
D. Squarer	Westinghouse Electric Corporation
T. Theofanous	Purdue University

OBSERVERS	AFFILIATION
R. Anderson	Argonne National Laboratory
C. Bell	Los Alamos National Laboratory
W. Camp	Sandia National Laboratory
R. Curtis	USNRC/RES/DAE
M. Krein	Sandia National Laboratory
B. Marshall	Sandia National Laboratory
M. Merilo	Electric Power Research Institute
A. Pressesky	American Nuclear Society
B. Raj Sehgal	Electric Power Research Institute
J. Rosenthal	USNRC/NRR/RSB
M. Silberberg	USNRC/RES/DAE
J. Telford	USNRC/RES/DAE
J. Walker	Sandia National Laboratory
M. Young	Sandia National Laboratory

SERG MEETING
HARPERS FERRY, WVA
NOVEMBER 28, 1984

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R. Curtis	USNRC/RES/DAE
M. Krein	Sandia National Laboratory
B. Marshall	Sandia National Laboratory
M. Merilo	Electric Power Research Institute
B. Raj Sehgal	Electric Power Research Institute
J. Rosenthal	USNRC/NRR/RSB
J. Telford	USNRC/RES/DAE
J. Walker	Sandia National Laboratory
M. Young	Sandia National Laboratory

Sandia National Laboratories
Albuquerque, New Mexico

date: October 29, 1984

to: Steam Explosion Review Group (SERG) Members

Marshall Berman

from: Marshall Berman

subject: Sandia Steam Explosion Research Proposal

This memo provides additional information on the experimental facilities which we are proposing. The NRC-funded program on fuel-coolant interactions (FCIs) addresses all aspects of such (in- and ex-vessel) interactions, including steam and hydrogen generation, debris characteristics and dispersal, source term effects, and direct and indirect containment failure. This memo primarily emphasizes the research required to resolve the issue of alpha-mode failure. We have included appendices containing additional material on the SEALS and ELVIS facilities. The material presented here should be treated as very preliminary, and subject to change.

TABLE OF CONTENTS

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1. PROGRAM ELEMENTS AND PRIORITIES

We have proposed a research program which includes the development of FCI models and a comprehensive experimental program containing four facilities. These facilities and their associated priorities are listed below.

Priority 1: **SEALS** Facility (Steam Explosions At Large Scale)

Objective: Determine the effects of scale on coarse mixing and conversion ratio for steam explosions at an ambient pressure of one Albuquerque atmosphere (0.83 bars).

Description: Use up to 2000 kg of iron-alumina thermite in an open (no external containment building) geometry, rigid (steel) and weak-walled (lucite) water chambers (one-half linear scale replicas of a PWR lower plenum), subcooled and saturated water. Some tests might use corium thermites, and prototypical (PWR and BWR) structures in the lower plenum.

Priority 2: FITSX Facility (Extension of current FITS system)

Objective: Investigate FCI behavior for up to 50 kg of various thermitic melts in the existing FITS vessel. Measure conversion ratio, steam and hydrogen generation, debris characteristics, and fission product release for normal (melt dropped into water) and alternate (water onto melt) contact modes.

Description: Oxidic (iron oxide) and mixed oxide-metal melts (corium, iron-alumina, UO_2-molybdenum) would be thermitically prepared and brought into contact with saturated and subcooled water; ambient pressure would be varied between 0.83 and 11 bars. The influence of many important initial and boundary conditions on FCI behavior would be quantified.

Priority 3: SHIP Facility (Small-scale HIgh Pressure)

Objective: Determine the dependence of explosion triggering and conversion ratio on ambient pressure up to 170 bars.

Description: Prepare single-droplet oxidic and metallic melts in a high pressure chamber, using a laser and an inductive melter. Measure the pulse characteristics (peak pressure, rise time, duration) necessary to trigger explosions, and the resulting yield of those explosions.

Priority Unknown: ELVIS (Enclosed Large-Vessel Interaction System)

Objective: Investigate FCI behavior in a scaled-up version of the FITS facility, using inductively-prepared oxidic and metallic melts.

Description: Prepare approximately 400 kg melts and deliver them into water chambers inside of a larger containment chamber. Investigate the influence of the important initial and boundary conditions.

The assignment of priorities is based on our estimates of the impact and importance of the possible experimental results. With respect to vessel and containment failure, we believe that the SEALS facility is extremely important for issue resolution.

Much of the current uncertainty with respect to alpha-mode failure derives from the extrapolation of small-scale data to the reactor situation. If the large-scale SEALS tests confirm that physical limits on explosion yields exist, then we will have removed a major contributor to risk from further consideration; i.e., alpha-mode failure at ambient pressure.

If the SEALS results are not positive, then the research would address other potential mechanisms for limiting or preventing containment failure; e.g., more detailed experiments and analyses of the melt progression, and the way that melt pours into the lower plenum; and improved simulation of the geometry and structures in the lower plenum, especially for BWRs. The required scale of additional experiments would be strongly influenced by the as-yet-unknown results of the SEALS tests.

SEALS is not intended to be an all-purpose facility for addressing generic FCI questions. The relatively high cost of individual tests will limit the number and scope of the tests. SEALS will primarily address the influence of increasing melt mass on the probability and consequences of steam explosions. FITSX is intended to be the primary facility for assisting in the development and assessment of FCI models important for reactor safety analyses.

2. OBJECTIVE OF LARGE-SCALE EXPERIMENTS

Many papers have been published concerning the probability of alpha-mode failure [e.g., see 1-8]. A common theme of many of these papers is that the large-scale interactions that are required for potentially threatening explosions are not physically possible. These conclusions are sometimes based on qualititative arguments. However, several analysts have developed quantitative models. Although these models agree (within an order of magnitude or so) with existing small-scale data, they predict that important threshold effects will occur for sufficiently large melt masses. If these dramatic effects can be confirmed by larger-scale tests, then the community will have much greater confidence in the unlikelihood (or even impossibility) of alpha-mode failures. If the effects are not confirmed, the experimental data will then be used to develop new and more reliable models.

The most important results to be produced from the SEALS tests will involve the following issues:

1. How does conversion ratio depend on fuel mass?

2. How does conversion ratio depend on water depth and water subcooling?

3. How is conversion ratio affected by by prototypical geometries and structures in the lower plenum?

The results have been stated in terms of conversion ratio as the primary dependent variable. Hence, although SEALS will be instrumented to measure many quantities, emphasis will be placed on the fraction of the melt thermal energy which can perform work on the vessel and cause it to fail.

3. SEALS DESIGN

The SEALS design is considered very preliminary, and is continuing to undergo review and evaluation. We welcome and encourage SERG members to criticize our design and to provide suggestions.

The following are the most important features and capabilities of SEALS:

1. Thermitically-prepared melts of 500 to 2000 kg of iron-alumina at temperatures of 2700 - 3000 K or less; corium melts of up to 4000 kg will also be used.

2. One-half linear scale representation of a PWR lower plenum, with cross-sectional area of 3.8 m^2.

3. Water depths of 0.75, 1.5, and 3.0 m.

4. Water temperatures ranging from ambient to saturated.

5. Unconfined-transparent and rigidly confined water chambers.

6. Cylindrical melt of diameter 0.85 m, delivered to the water in as compact a mass as possible.

3.1 SEALS: SCALING CONSIDERATIONS

The following table illustrates the scaling of the water chamber and the pour geometry:

Parameter	SEALS Value		PWR Equivalent
lower plenum diameter	2.2 m	x 2	4.4 m
l.p. water depth - actual	3.0 m	x 1	3.0 m
" " " - full	1.5 m	x 2	3.0 m
" " " - 1/2	0.75 m	x 2	1.5 m
pour diameter - 1/2 core	0.85 m	x 2	1.7 m
maximum pour mass:			
mass-scaled	2000 kg	x 8	16000 kg
volume-scaled	~4000 kg	x 8	~32000 kg

Since iron-alumina thermites are approximately half the density of corium thermites, the delivered melts are equivalent to about 32,000 kg in the reactor case.

Any experiment that involves less than an identical prototype of the reactor situation entails major unavoidable approximations. An uncluttered (no internal structures) water chamber scaled to one-half of a PWR lower plenum can supply useful information both for understanding the important phenomena and for scaling up to the reactor environment. It does not, however, represent the actual depth of water through which the melt can fall. Since some mixing models depend strongly on depth, experiments that employ shallower depths will predict less mixing; the results would be non-conservative. Similarly, it would be desirable to achieve ratios of fuel to coolant masses which are representative of the reactor environment. Since the number of tests will be limited, and the post-test data reduction will be extensive and time-consuming, we have arbitrarily selected some particular values.

We have decided to develop a single melt crucible design corresponding to a scaled pour diameter of half the core width. The use of a large pour diameter may be non-conservative. The below-core structures in a PWR lower plenum could enhance the breakup and mixing of molten fuel. In particular, the lower core support plate would break the melt up into multiple streams of initial diameter of about 4 - 6 cm. To address this possibility at large scale, we propose to do a few tests in SEALS in which a section of the lower core support plate will be inserted at its actual location, with 6-cm diameter holes at their actual spacings; the depth of water will correspond to the actual full depth of the lower plenum, i.e., 3 m. For the remainder of the tests, two water depths of 0.75 and 1.5 m will be used. These depths simulate, at half scale, the distance to the lower core support plate and the full depth of the lower plenum. It is important to keep in mind that the fundamental objective of SEALS is to provide data on mixing and conversion ratio as a function of the amount of melt participating. Different smaller-scale facilities (FITSX, SHIP) will be used to investigate other important FCI input conditions (e.g., ambient pressure, alternate contact modes, pour diameters and pour rates, trigger strengths, etc.) and consequences (steam and hydrogen generation rates, fission product release information, etc.).

For the two scaled water masses (2850 and 5700 kg) and melt masses of 1000 and 2000 kg, the water/fuel mass ratios will range from 1.4:1 to 5.7:1; these values are comparable to many previous FITS tests which used 20-kg melts.

3.2 SEALS: THREE-PHASE APPROACH

SEALS represents a very large extension of current experimental capabilities. Hence, there is significant uncertainty in estimating the difficulty involved in developing new systems. We have divided the construction of SEALS into three phases, which will permit the checkout and testing necessary to develop these sytems while simultaneously providing interim experimental data. This same approach was used in the construction of the EXO-FITS and FITS facilities. The three phases are conceptually illustrated in Figures 1 - 3.

3.2.1 SEALS1

The primary emphasis in phase 1 will be development of a melt preparation and delivery system. We will attempt to prepare and deliver up to 2000 kg of molten iron-alumina to the water, with the minimum drop height possible. Our current approach is to pull the crucible cylinder up and away from the melt as quickly as possible. Since the entire crucible system may be destroyed during the test, we will try to keep the costs of the delivery system as low as possible. During the development of this melt preparation and delivery system, lucite water chambers will be placed below the crucible. Pressure transducers and photography will be used to record the mixing process, and explosions if they occur. Other instrumentation will also be developed and tested during this phase.

Although SEALS1 is primarily developmental, it will be the only phase of SEALS in which the mixing process will be observed photographically. These movies should provide excellent comparisons to the smaller-scale tests conducted in FITS and EXO-FITS.

3.2.2 SEALS2

The SEALS vessel will be composed of three major parts: an upper and lower head, and a cylindrical mid-section. In phase 2, the mid-section will be attached to a base plate. Vented experiments will be conducted in this geometry. Test series will be conducted to check new instrumentation and to compare the effects of vessel confinement on conversion ratio. Means will be developed for protecting the vessel bottom from thermal attack.

3.2.3 SEALS3

The final phase of SEALS is sketched in Figure 3. Steam explosions in the lower plenum will throw a "slug" composed of water, steam, fuel melt, and fragments of the melt crucible, against the upper head. The energy imparted to the head will be measured directly by some system. Current possibilities include crushable foam (illustrated in Figure 3), compressing a gas, or frictional drag through a liquid. Cost will be an important factor in deciding between the various systems currently being investigated.

3.3 SEALS: EXPERIMENTAL PROCEDURE

We currently envision the following experimental procedure:

1. Ignite thermite.

2. Detect burn front with burn probes.

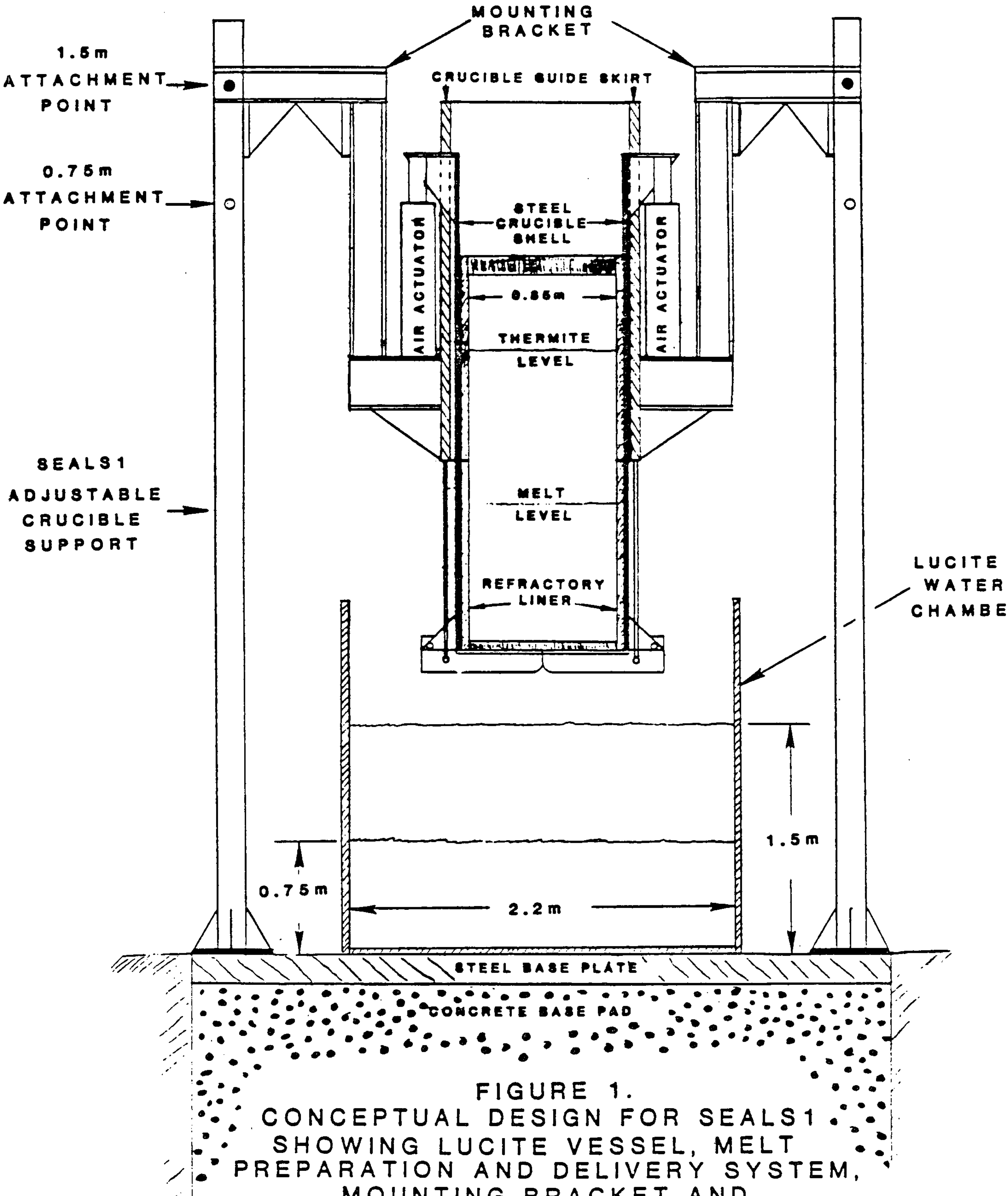

FIGURE 1.
CONCEPTUAL DESIGN FOR SEALS1
SHOWING LUCITE VESSEL, MELT
PREPARATION AND DELIVERY SYSTEM,
MOUNTING BRACKET AND
CRUCIBLE SUPPORT

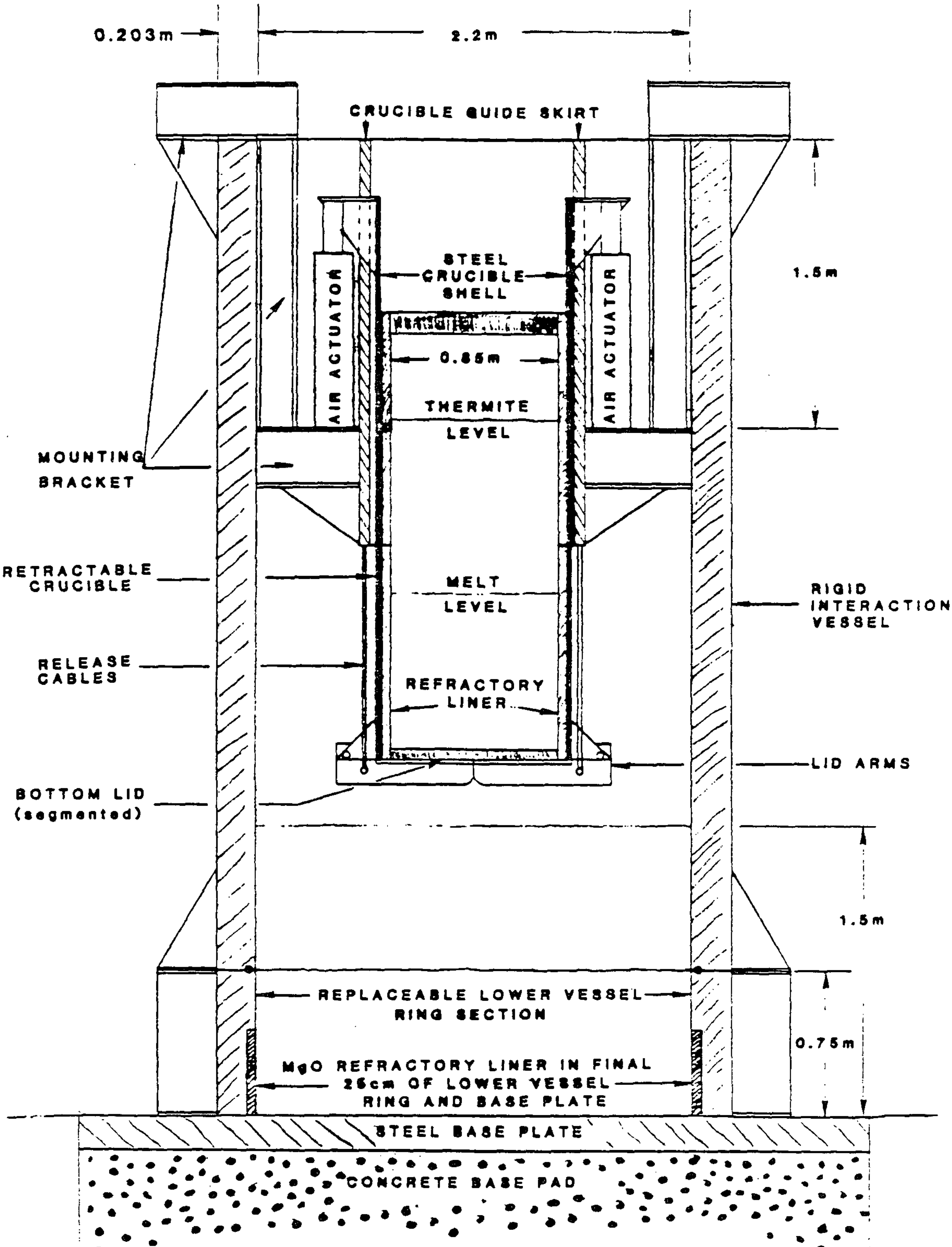

FIGURE 2.
CONCEPTUAL DESIGN FOR SEALS2 SHOWING INTERACTION VESSEL, MELT PREPARATION AND DELIVERY SYSTEM AND MOUNTING BRACKET

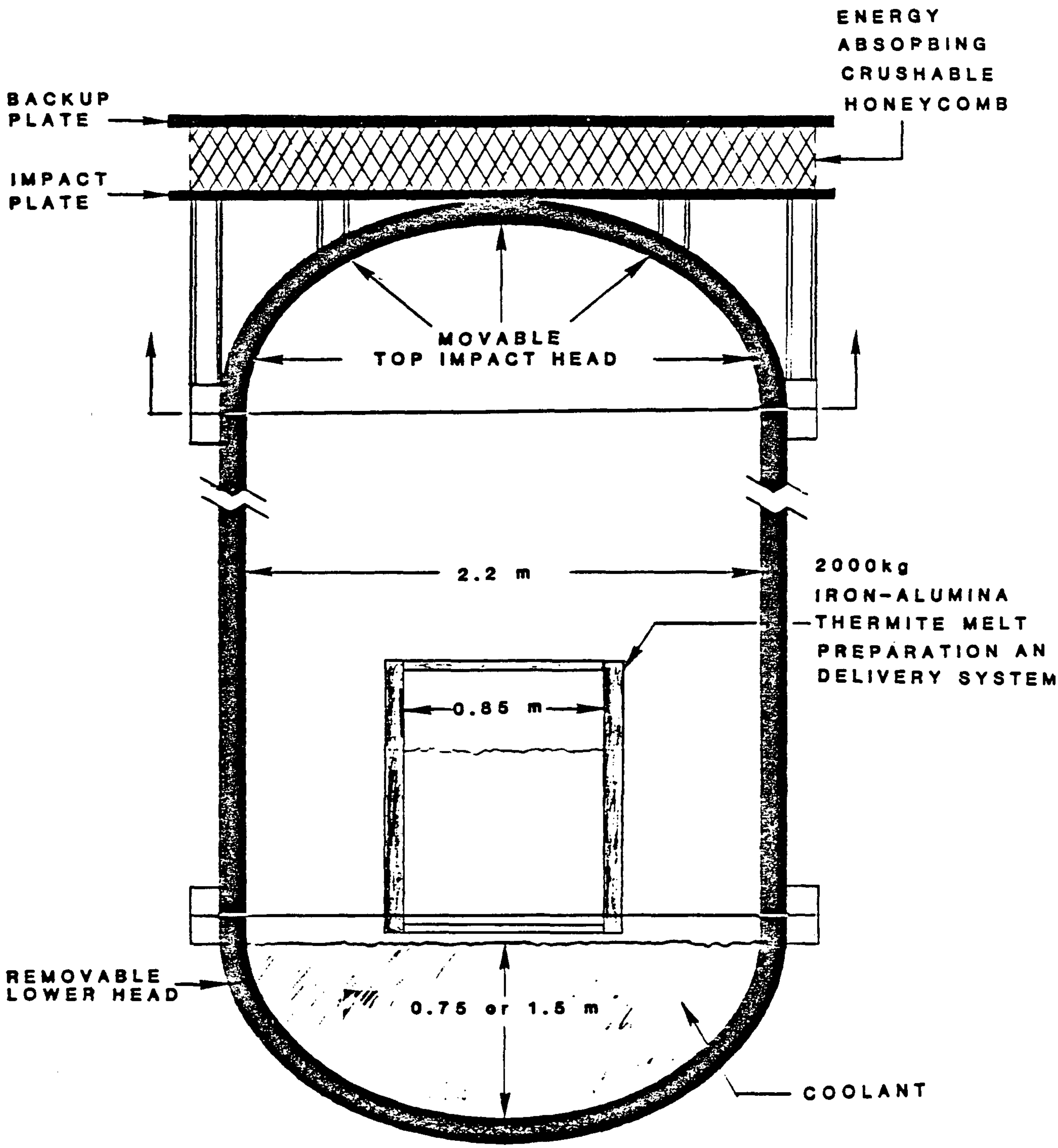

FIGURE 3.
SEALS3
1/2 SCALE REACTOR GEOMETRY

3. Begin molten thermite pour 1.5 s after burn completion.

4. Wait for spontaneous trigger until 200 ms after first contact of the leading edge of the melt with the chamber base (determined by thermocouple and ultrasonic measurements).

5. If spontaneous triggering does not occur, fire artifical trigger (explosive, "thumper," or some other device).

When the molten material is released from the crucible, the crucible will be rapidly pulled up from the initial drop position, leaving the full melt mass behind. In this way, we hope to avoid confusion resulting from large melt masses, but slow pouring rates. The experimental goal is to deliver the total prepared melt in a compact configuration.

The selection of 200 ms as the hold time until firing the external trigger is an arbitrary decision; almost all spontaneous triggers in the FITS tests occurred within 100 ms of the time that the melt front contacted the chamber base. The actual delay time might be treated as an independent variable in a later test series. This delay time will clearly play an important role in the outcome of the externally-triggered tests.

3.4 SEALS: SUGGESTED EXPERIMENT MATRIX

There are many possible test series which could be run. The actual experimental matrix may be an important discussion topic for the review group meeting. The following is a very preliminary outline for a possible experimental matrix. It can be used for discussion purposes:

SEALS1

Test Series 1: Facility checkout. Prepare melts from 100 to 2000 kg. Analyze melts for chemical composition, density, and trapped gases. Build and test the melt delivery system. During some of these tests, melt will be dropped into lucite chambers filled with water. The mixing process will be photographed. Instrumentation and the data acquisition system will also be checked.

SEALS2

Test Series 2: Fractional factorial experiment design:

3 melt masses: 500, 1000, 2000 kg
2 water depths: 0.75 and 1.5 m
2 water temperatures: ambient and saturated

Fixed quantities: iron-alumina melt, pour diameter = 0.85 m, cylindrical steel half-scale water chamber.

Test Series 3: Two tests and two repetitions:

- actual lower plenum water depth;
- actual simulation of lower core plate, diffuser and support plates;
- 2000 kg of melt;
- 2 water temperatures: ambient and saturated.

An evaluation of the results would be appropriate at the end of Test Series 2 or 3. If only very weak explosions have occurred (or no explosions at all), and the number of tests has been statistically significant, then it might be appropriate to terminate the SEALS program. If moderate or strong explosions have occurred, and questions remain with respect to the energetics of slug impact, then the program should continue into phase 3.

<u>SEALS3</u>

Test Series 4: Checkout tests possibly using comparable high-explosive materials submerged under water. This should permit us to calibrate the displacement of the head for a known source strength. The calibration will only be approximate due to the differences in "slug" properties.

Test Series 5: Repeat fractional factorial design of Test Series 2.

Test Series 6: Repeat Test Series 3 with lower plenum structures.

Additional test series will depend on the results of these tests, as well as on possible SERG committee recommendations. Variables may include simulated BWR lower plenum structures, corium melts, different pour diameters, etc.

3.5 SEALS EXPERIMENTS: POSSIBLE OUTCOMES

It is useful to consider the possible outcomes of the SEALS experiments to assist in: a) Evaluating the adequacy of the measurements and their associated accuracy; b) Estimating the means by which the experimental results can lead to resolution of the alpha-mode failure issue.

3.5.1 No Melt Fragmentation

Several of the existing coarse mixing models predict that very large particles will result when large quantities of melt are poured into water. For example, the Henry-Fauske CHF model predicts that particles larger than the vessel diameter will occur when the molten core pours into the lower plenum. In their words, "...the best estimate would be that essentially no coarse fragmentation would occur. The molten core material would pour through the lower plenum as a large mass, and a molten pool would accumulate on the lower head."[2] Using that model, we calculate that 2000 kg melts in SEALS will produce particles larger than the diameter of the SEALS water chamber. Hence, the model predicts, by a wide margin, a pour of melt into water similar to pouring molasses into water - no fragmentation will occur. Observation of such behavior, if it occurs, will be straightforward.

3.5.2 Insufficient Coarse Fragmentation

Some investigators believe that "energetic" steam explosions require the pre-establishment of a coarse mixture of water and fuel particles which are equal to or less than about 1 cm in diameter. [1,2,8] Premixtures coarser than this will result in either very weak explosions or no explosions at all. Based on the Henry-Fauske model [2], coarse particle sizes larger than about 1 cm will occur for masses of roughly 10 kg of fuel for saturated water, and roughly 50 kg for subcooled water. Hence, the entire matrix of SEALS tests should produce non-explosible mixtures, according to this model. The SEALS1 lucite-chamber tests should provide some data on particle sizes at the mixture surface. Post-test debris analysis for non-explosive tests in SEALS1 and 2 will also be useful in evaluating these predictions. SEALS3 will permit recovery and analysis of much of the debris, even for tests which are explosive.

The Corradini model [9,10] also predicts large fragment sizes of about 30 cm for the 2000 kg tests [11]. These large fragments lead to predicted conversion ratios that are much smaller than 1%. One interpretation of this model is that a small amount of the melt might explode vigorously, but most of the fuel will not participate. SEALS3 debris analysis should help to evaluate this prediction.

3.5.3 Single Steam Explosions

Based on FITS data, one possible scenario is that the fuel enters the water, begins to fragment, and then spontaneously explodes at any time between melt-water contact and shortly after the mixture contacts the chamber bottom. We have qualitatively observed that the probability of a spontaneous explosion increases with the amount of fuel in the mixture. For the large melts in SEALS, early explosions just beneath the water surface may be likely. Those explosions are likely to involve relatively small quantities of melt; the remainder of the melt and some of the water may be thrown upwards. In the vented tests in SEALS1 and 2, much of the melt will be thrown away from the chamber. In the confined tests in SEALS3, the melt will fall back into the water. Hence, this behavior would be recorded as weak single explosions in the vented tests, and possibly weak, widely-spaced separate explosions in the confined tests. If the amount of material involved in the explosion is roughly constant, then we will observe a strong decrease in explosion conversion ratio as a function of delivered melt mass.

If the melt does not spontaneously explode well before base contact, then much more melt may be involved. Strong explosions might now be possible either spontaneously or externally triggered. We expect the conversion ratio for the confined SEALS3 tests will be significantly higher than for the vented tests. FITS data and our own preliminary calculations imply that prepressurization of the chamber will occur prior to the major impact of the "slug." There is not enough time available to significantly vent the system and reduce its pressure. The pressure seen by the upper head may have two maxima: The increase due to the arrival of the shock wave, and before this pressure has time to decay significantly, the arrival of the "slug" material; this second maximum will be superimposed on the first pulse.

These single explosions may generate the major challenge to vessel integrity. The measurement of the strength of these explosions is discussed in Section 5.

3.5.4 Double and Multiple Explosions

Double explosions occurred in three of the nine FITSB experiments. [12] The first explosion was generally too weak to expel much water or fuel, but was strong enough to greatly stir and mix the fuel and water. The second explosion occurred spontaneously, about 110 - 140 ms after the first. These highly transient events might have important implications for the reactor situation. Many existing mixing models examine the steady state conditions that result from balancing steam generation rates with fluid motion. A double explosion would not be constrained by these models. Hence, the degree of mixing could be much greater than current predictions.

In the reactor case, the precursor explosions could alter the geometry in which a subsequent explosion occurs. The first explosion could break a supporting crust, and lead to a large melt pour. It might also damage or move the lower core or diffuser plates. This behavior might be observed in Test Series 3 and 6, where lower plenum structures are being simulated.

The occurrence of double explosions may be a highly random event. Many tests may be required to establish their probability and consequences. Pressure transducers in the water phase of all tests, and in the gas phase of SEALS3 should indicate their occurrence. The SEALS3 facility will be designed to separately measure the contributions of these events during a single test. Information gleaned from SEALS tests will require supplementation by additional smaller-scale tests in FITSX.

3.5.5 Explosions in the Stratified Contact Mode

In some experiments, melt may pour through the water and puddle on the bottom of the chamber. Based on some qualitative experimental data and industrial accidents [13-15], a steam explosion may be spontaneously or externally triggered in this configuration. Some current models imply that such explosions will he very inefficient, and will involve only a small fraction of the melt available. It is conceivable that some puddled fuel may simultaneously participate in an explosion which has been triggered in an overlying coarse mixture, increasing the measured conversion ratio. An explosion triggered in the layer itself might also result in a higher conversion ratio due to the higher tamping of the fuel surounded by the chamber walls and liquid above.

SEALS is not intended to investigate the separate effects of stratified explosions. Such experiments are planned for FITSX. Nevertheless, if stratified explosions occur, their contribution to the overall conversion ratio will be measured.

4. MEASUREMENT SYSTEMS

SEALS1 will: employ high- and low-speed photography to measure the shape of the coarse mixing region as a function of time, measure level swell (for average void fraction), estimate coarse particle sizes, determine explosion propagation speeds, and evaluate slug and tracer particle velocities. Water-phase pressure transducers will be used. Calorimetry and optical pyrometry will be used to determine the heat content and temperature of the melt.

SEALS2 will employ a system to measure level swell. Acoustic methods will be employed to estimate the shape of the mixing region within the opaque steel chamber. Tracer particles will be employed in the water, and their speeds measured photographically. A hot-wire-anemometry system will be tested for measuring particle velocities in the vessel. The mass involved in an explosion will be estimated by subtracting the residual mass in the water chamber from the delivered mass.

SEALS3 will employ all the SEALS2 systems, plus several additions. The energy of the upper head will be measured using crushable foam, or a similar force-distance dissipation system. Strain gages, accelerometers, and thermocouples will also be strategically deployed. In some tests, debris will be sieved and chemically analyzed.

5. CONVERSION RATIO

The primary dependent variable in SEALS is the conversion ratio, CR, defined by the following equation:

$$CR = W_e/Q_f$$

where W_e = the explosion work,
Q_f = the thermal energy available in the melt including latent and sensible heats.

Q_f will be directly measured using calorimetry. In addition, we will measure the melt temperature using optical pyrometry. The errors in these measurements should be low compared to other measurements.

The explosion work is a somewhat ambiguous term that has led to confusion both with respect to experimental data and calculations. In the Sandia single-droplet experiments, W_e is the PdV work required to expand the steam bubble [16]. In the Winfrith experiments [17,18], W_e represents the PdV work involved in the compression of a relatively small cover gas volume. In the Sandia intermediate-scale experiments [12,19,20], published values of W_e generally represented the peak kinetic energy of the dispersing "slug" of water and fuel drops. These last kinetic-energy-determined conversion ratios are generally much lower than the other measurements. In the confined FITS tests, the chamber internal energy is increased significantly during the explosion. In an analogy with high explosives, this energy is given by $PV/(\gamma - 1)$. Based on previous experiments [13,21] and neglecting scale effects, we expect the conversion ratio to increase using: rigid water chambers, confined or semi-confined interaction vessels, and saturated water.

For the vented tests (SEALS1 and 2), we will measure conversion ratio (based on "slug" kinetic energy) to an accuracy quite similar to the earlier EXO-FITS (by Mitchell, Krein et al. [12,20]) and open-geometry (by Buxton and Benedick [19]) experiments. These measurements will employ simple one-dimensional formulas, and assume the existence of "mean effective" slug masses and velocities. In SEALS3, we will directly measure the FdZ work involved in displacing the upper head. This measurement is strongly dependent on the system configuration and the "slug" properties, as well as the energetics of the steam explosion itself. E.g., a very massive upper head will move only slightly due to the impact; on the other hand, a very light upper head will move quickly in response to the leading edge of the slug, and not provide an accurate estimate of the full impulsive load. Nevertheless, the SEALS3 system should provide a reliable measure of the <u>relative</u> dependence of conversion ratio on fuel mass.

Local spatial and temporal measurements of pressure are of major interest. Pressure histories in the lower plenum and upper head ultimately determine whether the vessel or containment will fail. We will employ many pressure transducers in the water chamber and in the gas phase.

For addressing alpha-mode failure, our basic objective is to mechanistically model the FCI processes with sufficient accuracy to predict the probability and consequences of steam explosions. Some current simple models and formulations employ conversion ratio directly in their calculations [e.g.,5,6,8]. Simplifying assumptions are used to translate the explosion conversion ratio into a "slug" energy and subsequently into a pressure load on the upper head. These models are useful for providing interim information, and possibly for resolving the alpha-mode failure issue, if the experimental results are favorable. However, a much more sophisticated model is required for detailed accident analysis codes (e.g., MELPROG, CONTAIN), and possibly even for second-generation risk assessment codes (e.g., MELCOR). Such a model will also be required for alpha-mode failure analyses if the simpler models prove inadequate.

We have been developing the elements of a comprehensive steam explosion model for several years. Computer codes have been written or adapted to predict the coarse fragmentation process, the propagation and expansion phases of an explosion, and rates of steam and hydrogen generation during FCIs. These codes will be employed in the analysis and reduction of the SEALS data. In particular, the CSQII code [22] has been modified to calculate the propagation and expansion phases, and to predict the pressure histories that result from the explosion. Although the code is well-established and verified, this FCI application of the code is still developmental; however, the code has already been applied in analyzing the SEALS design (see Appendix 1). CSQ requires a description of the initial conditions prior to the triggering of the explosion and two empirical parameters: the propagation speed of the explosion through the mixture, and the energy released during the explosion. The CSQ calculation directly computes kinetic energy by summing the quantity $mv^2/2$ for each computational cell in the problem. The code has been calibrated against several FITS tests. It will be applied in the analysis of the SEALS data. Input conditions will be supplied by means of photographic coverage and level swell (for average void fraction). The code can be redundantly checked against the water-phase and gas-phase pressure transducers, debris (and tracer particle) velocity measurements, the independent measurement of conversion ratio in SEALS1 and 2, and the pressure and impulse measurements in SEALS3. In a joint effort with the MELPROG program, we will also work on developing an advanced code capable of computing both mixing and explosive phenomena.

The major result from SEALS will be the determination of the influence of increasing mass on conversion ratio or related quantities (system pressures, work done on upper head, etc.). Almost all current models predict drastic decreases in these quantities over the range of masses available in SEALS.

6. COARSE MIXING

Photographic observation of mixing processes (as in the FITS tests) is obviously confined to surface effects. Observation even of surface mixing in the SEALS steel vessel is nearly impossible, even with x- or gamma-rays. Hence, we do not expect the SEALS tests to provide separate effects data on the dynamics of the coarse mixing process. We do intend to pursue such experiments with gamma-ray photography in the FITSX facility.

SEALS can provide information on coarse mixing indirectly by using physical models to estimate the influence of mixing on integrated conversion ratio measurement. Figure 4 illustrates the various extreme stages of the mixing process. The mixing can take place in a region near the surface with the central core composed primarily of unfragmented melt (early mixing phase). At a later time, the central core might be composed predominantly of steam. Another phase might involve a comparatively homogeneous mixture. We have briefly analyzed these three possibilities to determine whether measurements external to the mixing region can distinguish between them. We believe that pressure transducers in the water outside the mixing region cannot distinguish between the different phases. Active ultrasonic probing through the surface layer appears very difficult. Measurements within the region are currently considered well beyond the state of the art, with respect to procuring information and interpreting it. A possible method for probing the interior region would be x-ray photography. The attenuation involved in SEALS tests makes this scheme impossible. It does, however, seem feasible for implementation in some FITSX tests.

7. STRATIFIED EXPLOSIONS

Some analyses of alpha-mode failure assume that explosions must take place during the formation of the coarse mixture, but cannot occur after the melt has pooled on the vessel bottom. This assumption implies that only a short time window is available for this process - the time interval required to fall through the lower plenum water, on the order of one second. This is not necessarily true. Melt that forms a stratified pool below the water may still participate in a steam explosion. This is true even if a solid crust forms between the fuel and water. Single-droplet experiments have shown that explosions occur even when the fuel is at its freezing point and almost completely encased in a solid crust. As long as some liquid-liquid contact can take place, an explosion can be triggered [23,24].

In the well-known Appleby-Frodingham accident, molten steel and water in a "torpedo" were separated by a solidified layer of slag for a period of time [15]. When the torpedo was connected to a train, a violent explosion occurred when the train began to move - eleven people were killed. Evidently the motion had generated cracks in the slag layer which permitted some liquid-liquid contact, and subsequent self-induced mixing and a propagating explosion. In the accident report, the following statement illustrated the previously held, but erroneous, belief that "alternate contact mode" explosions had not been considered dangerous [15]: "The general philosophy of employees with regard to molten metal/water contact was that whilst it was recognised that it would be extremely hazardous to pour hot metal on to water it was comparatively safe to pour water on to hot metal."

In the FITS5A test, the melt had formed a pool at the bottom at the time that the explosion was externally triggered [20]. The conversion ratio of this stratified configuration was essentially

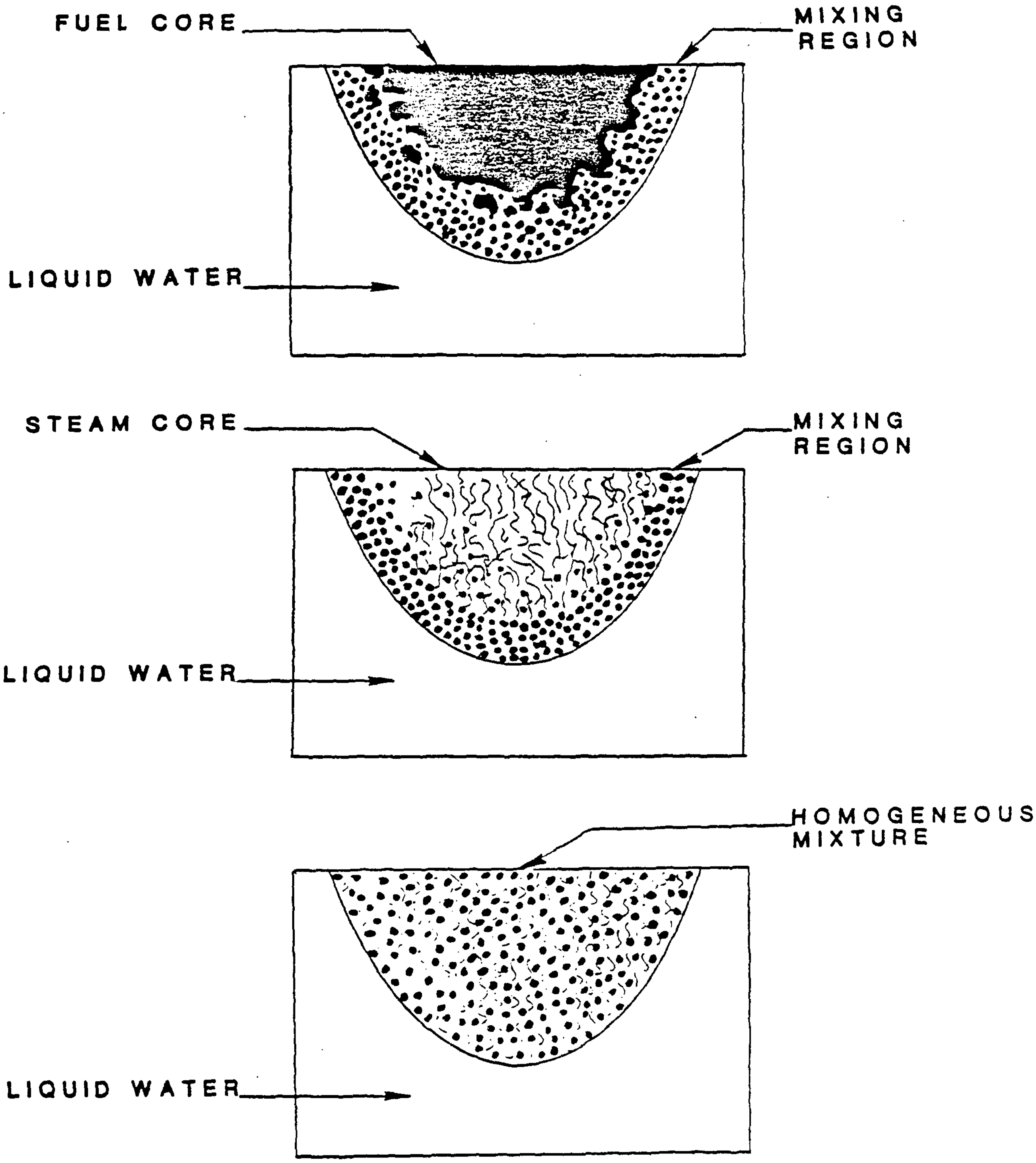

FIGURE 4.
VARIOUS MIXING CONFIGURATIONS

the same as in test FITS3A, where the explosion was spontaneously triggered as the coarsely mixed melt fell through the water, i.e., the more commonly expected picture of an explosion propagating through a premixture.

In experiments conducted at Brookhaven, explosions occurred for molten pools of bismuth, lead, and wood's metal beneath layers of water [14].

We conducted some preliminary tests (ACM 1 and 2) in which water was poured on top of molten alumina and iron [13,21]. A vigorous explosion occurred for one of the two tests.

To my knowledge, there are no existing models which predict the degree of self mixing and the conversion ratio for stratified liquid systems. The IDCOR steam explosion analysis [2] says that the maximum particle size which can undergo an energetic steam explosion is of the order of 1 cm. A possible extension of this idea would be to equate the particle size to an equivalent maximum layer depth. Theofanous has estimated that the particle sizes may be as large as 10 cm [3]. For the cross-sectional area of a PWR lower plenum, a 1-cm layer of fuel corresponds roughly to 1000 kg. An explosion triggered in the coarse mixture created by subsequently falling melt could have a high probability of triggering a coincident explosion in any stratified melt lying below. A 10-cm layer could contribute 10 tonnes of additional mass to the explosion. We have no data available to determine what depth of melt could possibly participate in a self-mixing and propagating explosion, nor what the efficiency of such an explosion might be. The relatively high degree of confinement of this system might also result in more efficient explosions.

The SEALS tests allow for the possible formation of stratified layers and subsequent explosions. If they occur, their contributions to the overall conversion ratio will be included in the measurement. If explosions do not occur, SEALS will be designed to prevent any facility damage due to the attack of melt on the water chamber bottom.

8. PRETEST PREDICTIONS

To maximize the utility of the SEALS experiments, we recommend that the safety community, including Sandia analysts, generate pretest quantitative predictions of the outcomes of these experiments. We will supply the initial and boundary conditions. Post-test data will include estimates of conversion ratio, debris velocities, gas- and water-phase pressures, and for SEALS3, debris characteristics and work done on the upper head assembly. The pretest predictions, if correct, will provide a convincing demonstration of the reliability of the models. If incorrect, we would expect that the models would be modified to agree with the experimental data.

9. OTHER SEALS DESIGNS

Many different experimental systems can be imagined which will address the effects of scale on steam explosion probability and consequences. The design discussed in this memo and the attached appendix is not our first proposal. We have discarded many other designs because they were too complex or too costly. Indeed, there is no single "best" design. We remain open, however, to any suggestions by the SERG which would improve the current system, or decrease its cost.

10. SUMMARY

A comprehensive FCI research program has been proposed which is intended to resolve certain key issues and to result in the development of verified computer models for accident analyses. To maximize information and minimize costs, the program has been developed in a conditional fashion. The current basic FITSX facility will address most of the important FCI phenomena in a facility capable of delivering up to 50 kg of molten fuel simulant. The SEALS facility will provide large-scale confirmation (or refutation) of the FITSX results, and the models based in part on those results. The SEALS effort can be truncated if the results favorably support some of the current hypotheses. In the event that SEALS demonstrates unfavorable scaling behavior, we might conclude that the FITSX facility is inadequate for the development and assessment of the required computer codes. The ELVIS facility has been proposed to meet such a need, if it should develop.

It is clear that the FITSX-SEALS-ELVIS facilities are closely connected, and provide the capability to resolve almost all important FCI issues. The important exception is FCI triggering at very high pressures. The SHIP facility is intended to address this question. In conjunction with the high-pressure larger-scale experiments planned at Winfrith in the United Kingdom, the high-pressure effects on FCIs should be resolvable in the near future.

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Sandia National Laboratories
Albuquerque, New Mexico

date: November 15, 1984

to: Steam Explosion Review Group (SERG) Members

Marshall Berman

from: Marshall Berman

subject: Computer Codes and the Resolution of FCI Issues

In previous memos [1-3], we have discussed past and current research results and our plans for new experimental facilities and continued model development. Reference 3 discussed current models and hypotheses concerning alpha-mode failure, and how the predictions of these models would be tested in the proposed SEALS, FITSX and SHIP facilities. This memo briefly discusses the computer codes which have been, or are being developed, and how they will be employed in resolving the important FCI issues.

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1. IMPORTANT FCI-RELATED ISSUES

The phenomena associated with molten fuel - coolant interactions play important roles during severe reactor accidents. The key questions related to FCIs include:

1. The rates and magnitudes of steam and hydrogen generation due to FCIs;

2. The characteristics of the debris produced by FCIs, including particle size distribution, porosity, and coolability;

3. The influence of the FCIs on accident progression and the nature of the source term (including fission product chemistry, release rate, particle size, and dispersal);

4. The consequences of pouring water on the melt in order to terminate the accident;

5. The probability and consequences of direct containment failure by an in-vessel steam explosion (alpha-mode failure).

For the following discussion, we will group the above questions into two major end uses of FCI research: The determination of the probability and consequences of alpha-mode failure; and all the other questions collectively labelled General Accident Analysis.

2. FCI COMPUTER CODES

The NRC program on core melt - coolant interactions has resulted in the generation of FCI models on: vapor film collapse around a fuel droplet, the fragmentation of molten fuel as it falls through water, the associated rates of steam and hydrogen generation for a mixture of droplets, and on the propagation of a steam explosion. The models have been incorporated into new and existing codes. For example, the WISCI (WISconsin Core Melt Interactions) fragmentation and mixing model was coded as the M1-Module of the MEDICI ex-vessel interactions code [4]. Table 1a lists some of the FCI-related codes, as well as the general accident analysis codes, all of which contain some model of fuel-coolant interactions and associated phenomena.

The first major accident analysis code was MARCH [5]. The FCI model in MARCH essentially produced an answer specified by the user's prescription of the degree of fragmentation (Subroutine HOTDROP). The model was of value for parametric analyses of FCIs, but was not mechanistic. The second-generation NRC codes will contain a mixture of mechanistic FCI models and empirical correlations.

In conjunction with the MELPROG program [6], an integrated FCI model, IFCI, will be developed (Integrated FCI code). The code will predict FCI behavior, within the constraints of run time and program size, beginning with separated fuel and coolant, and continuing up to, but not including, the response of structures. Currently, these phenomena are predicted with a suite of codes that separately treat each of the aspects of FCIs. WISCI predicts the fragmentation of the fuel into droplets, and the steam and hydrogen generated during the quenching of the droplets. WISCI has relied heavily on previous FITS data. The

Table 1a. FCI CODE DEVELOPMENT AND ASSESSMENT

CODE TYPE	EXAMPLES	INPUT	OUTPUT
ACCIDENT ANALYSIS CODES	MELPROG, CONTAIN, MELCOR, MAAP	ACCIDENT SEQUENCES	ACCIDENT CONSEQUENCES
INTEGRATED FCI CODE (2-D, 4-FIELD, IMPLICIT)	IFCI	SEPARATED MELT AND WATER, THERMODYNAMIC AND GEOMETRIC INITIAL AND BOUNDARY CONDITIONS	FRAGMENTATION, STEAM AND H_2 GENERATION. PRESSURE WAVES, HEAT TRANSFER, MATERIAL MOTION
2-D STEAM EXPLOSION CODES	CSQ, SIMMER	INITIAL AND BOUNDARY CONDITIONS AT TIME OF EXPLOSION TRIGGERING	PRESSURE WAVES, MATERIAL MOTION, VESSEL LOADING HISTORY
1-D FRAGMENTATION CODES	WISCI/MEDICI, TEXAS	SEPARATED MELT AND WATER, THERMODYNAMIC AND GEOMETRIC INITIAL AND BOUNDARY CONDITIONS	FRAGMENTATION, STEAM AND H_2 GENERATION, MATERIAL MOTION
STRUCTURAL ANALYSIS CODES (2-D OR 3-D, FINITE ELEMENT, AXI-SYMMETRIC)	HONDO, MARC	GEOMETRY, MATERIAL PROPERTIES, PRESSURE HISTORIES	ESTIMATES OF STRUCTURAL FAILURE
PROBABILISTIC CODES	MONTE CARLO	MECHANISTIC MODELS, PROBABILITY DENSITIES FOR UNCERTAIN OR STOCHASTIC PARAMETERS	FAILURE PROBABILITIES AND ASSOCIATED UNCERTAINTIES AND SENSITIVITIES

WISCI models will continue to be assessed against FITSY and SEALS data. These models will eventually be incorporated either directly, or by simple correlations, into IFCI.

There are several codes available that can model the propagation and expansion phases of a steam explosion: e.g., TEXAS [7] in one-dimension, and CSQII [8] and SIMMERII [9] in two-dimensions. CSQ and SIMMER begin with the initial and boundary conditions prior to the explosion - they do not begin with separated fuel and coolant, in contrast to WISCI and IFCI. These hydrodynamic codes contain different assumptions and approximations (all codes are approximate); they also model the explosion in different ways. For example, CSQ requires an initial description of the mixing region (geometry, average void fraction), and the external region (residual water, vessel size and shape, etc.); it then models the steam explosion as though it were a propagating chemical detonation. The detonation parameters required are propagation speed, and total energy released (i.e., transferred from the fuel to the water in the mixing region) during the explosion.

SIMMER also requires a thermodynamic and geometric description of the initial and boundary conditions prior to the explosion. SIMMER does not model the explosion as a detonation. Rather, fuel droplet sizes and heat transfer coefficients are specified. The code then proceeds to calculate the rapid transfer of heat from the fuel to the water, and the subsequent explosive vaporization. CSQ and SIMMER differ in many aspects, many of which have been discussed in Reference 10.

In addressing the probability and nature of vessel failures, the pressure histories predicted by CSQ or SIMMER are used to drive finite element structural analysis codes.

The above suite of codes can address the mechanistic phenomena associated with explosive and non-explosive FCIs. To answer questions related to probability, however, a probabilistic code was developed. This code employs simple models and probability density functions to sample a class of input conditions. The Monte Carlo sampling provides an estimate of failure probabilities.

Table 1b shows the developmental status of the various codes listed in Table 1a.

3. INTEGRATION OF CODES AND EXPERIMENTS

The experiments which we have proposed will provide the data necessary to develop and assess the codes listed in Table 1. Similarly, we have used and will continue to use the codes to assist in the design of the facilities and in the analysis of the results (see Section 4). Figure 1 shows the interrelationship between the experimental facilities, code types, and the two major end uses.

TABLE 1B. FCI CODE STATUS

THE FOLLOWING CODES ARE FULLY DEVELOPED. IN MOST CASES THEY HAVE BEEN EXTENSIVELY ASSESSED AGAINST EXPERIMENTAL DATA. THEY ONLY REQUIRE MODIFICATION OR ADAPTATION TO SPECIFIC FCI PROBLEMS:

CSQ, SIMMER, HONDO, MARC

THE FOLLOWING CODES (IN ONE OR MORE VERSIONS) ARE COMPLETE, OR IN ADVANCED STAGES OF COMPLETION. THEY HAVE NOT, HOWEVER, BEEN FULLY ASSESSED:

WISCI/MEDICI, MELPROG, CONTAIN, MAAP, MONTE CARLO

THE FOLLOWING CODES (IN ONE OR MORE VERSIONS) ARE ACTIVELY BEING DEVELOPED:

IFCI, TEXAS, MELCOR, MELPROG, CONTAIN, WISCI/MEDICI

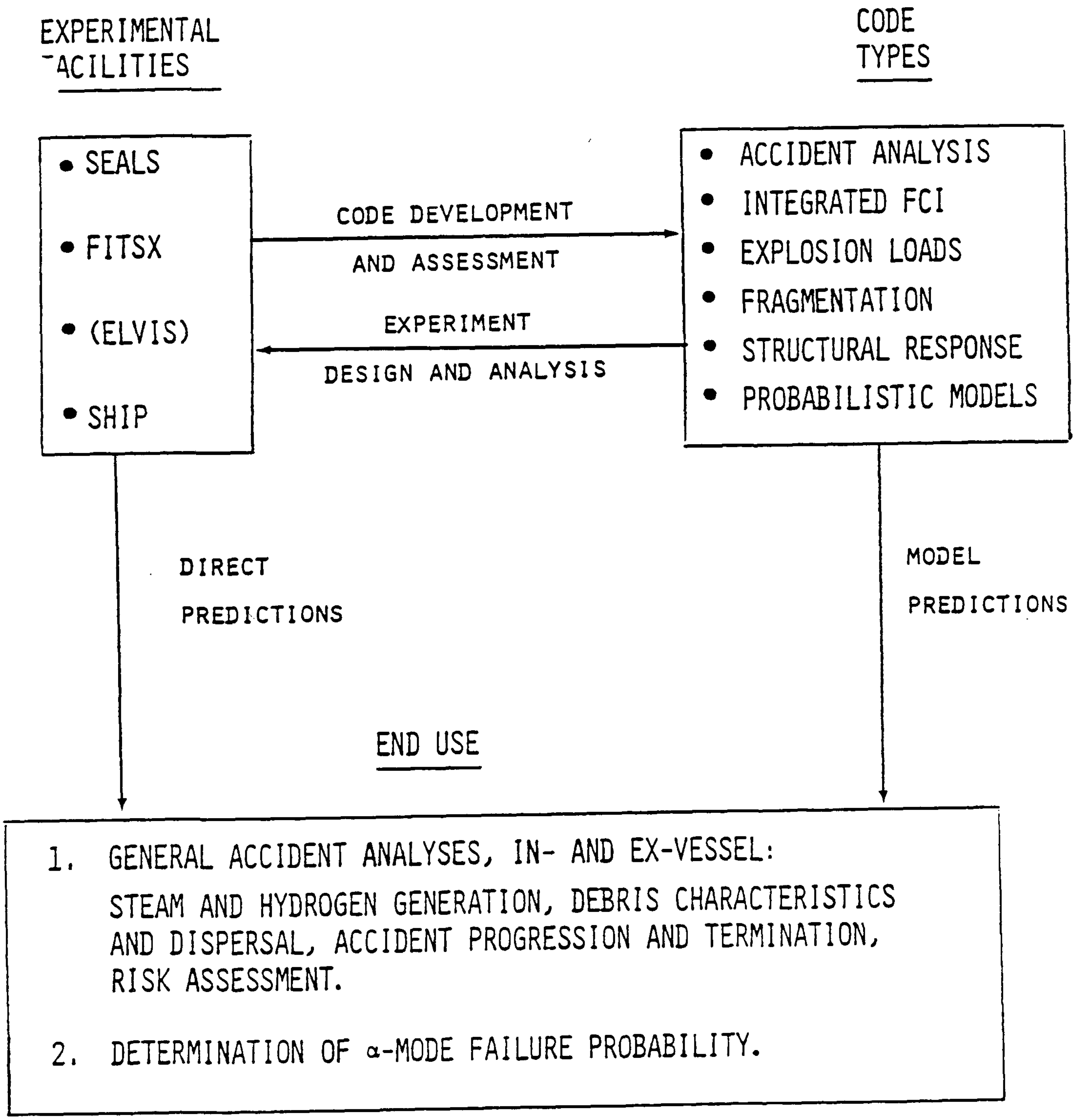

Figure 1. FCI RESEARCH PROGRAM

It is important to understand that the current codes do not mechanistically model many important FCI phenomena. For many of the poorly understood processes, the modelling involves experimental correlations or analogous processes (e.g., substituting chemical detonation models for steam explosions). For processes as complex as explosion triggering, no models exist at all. This situation emphasizes the need for reliable experimental data in developing these models. A detailed example of the process of integrating experimental data and model development is presented in Section 4.

3.1 END USE: GENERAL ACCIDENT ANALYSES

For achieving the goal of developing a tool for accident analyses, the progression of code development is straightforward. As shown in Figure 2, models are required for the various phases of explosive and non-explosive FCIs. We expect that the fragmentation model produced at the end of the research will predict mixing for all fuel-coolant contact modes, including those which originate in a stratified mode with separate layers of fuel and coolant. Since current models of mixing predict very strong effects of increasing mass, the FITSX and SEALS facilities are needed to provide appropriate data on scaling. If current scaling models are correct, we believe that FITSX will be adequate for providing data on steam and hydrogen generation, fission product source term, debris characteristics, and alternate contact mode FCIs; the important FITSX limitation to thermite melts, however, may need to be separately addressed. If SEALS demonstrates that current scaling models are inadequate, then a facility larger than FITSX (possibly ELVIS) may be needed for the generation of the appropriate experimental data.

3.2 END USE: ALPHA-MODE FAILURE PROBABILITY

Because of the different nature of this end use, and the fact that many models have already been proposed, it is possible that this issue may be resolved directly by the appropriate experiments. Consider Figure 3. If the SEALS tests show that no explosions, or only very weak explosions, occur at large scale, then the probability of alpha-mode failure at low ambient pressure can be taken to be zero. The definition of "very weak" might be taken from Reference 11 in conjunction with an estimate of 1000 to 2000 MJ for containment failure [12]. From Reference 11, the range of specific heat content is 0.8 to 1.6 MJ/kg. 2000 kg of iron-alumina in SEALS corresponds to about 32,000 kg of corium in a PWR; this mass falls in the middle of the ranges of poured masses considered in Reference 11 (p.17). For the high specific heat, a conversion ratio of 2% (x 32,000 kg x 1.6 MJ/kg) will yield about 1000 MJ. Hence, a "very low" conversion ratio would be much less than 2%. For conversion ratios of the order of 2% or larger, the probability of alpha-mode failure might still be small, but it would not be zero. For conversion ratios much higher than this value, high failure probabilities could not be

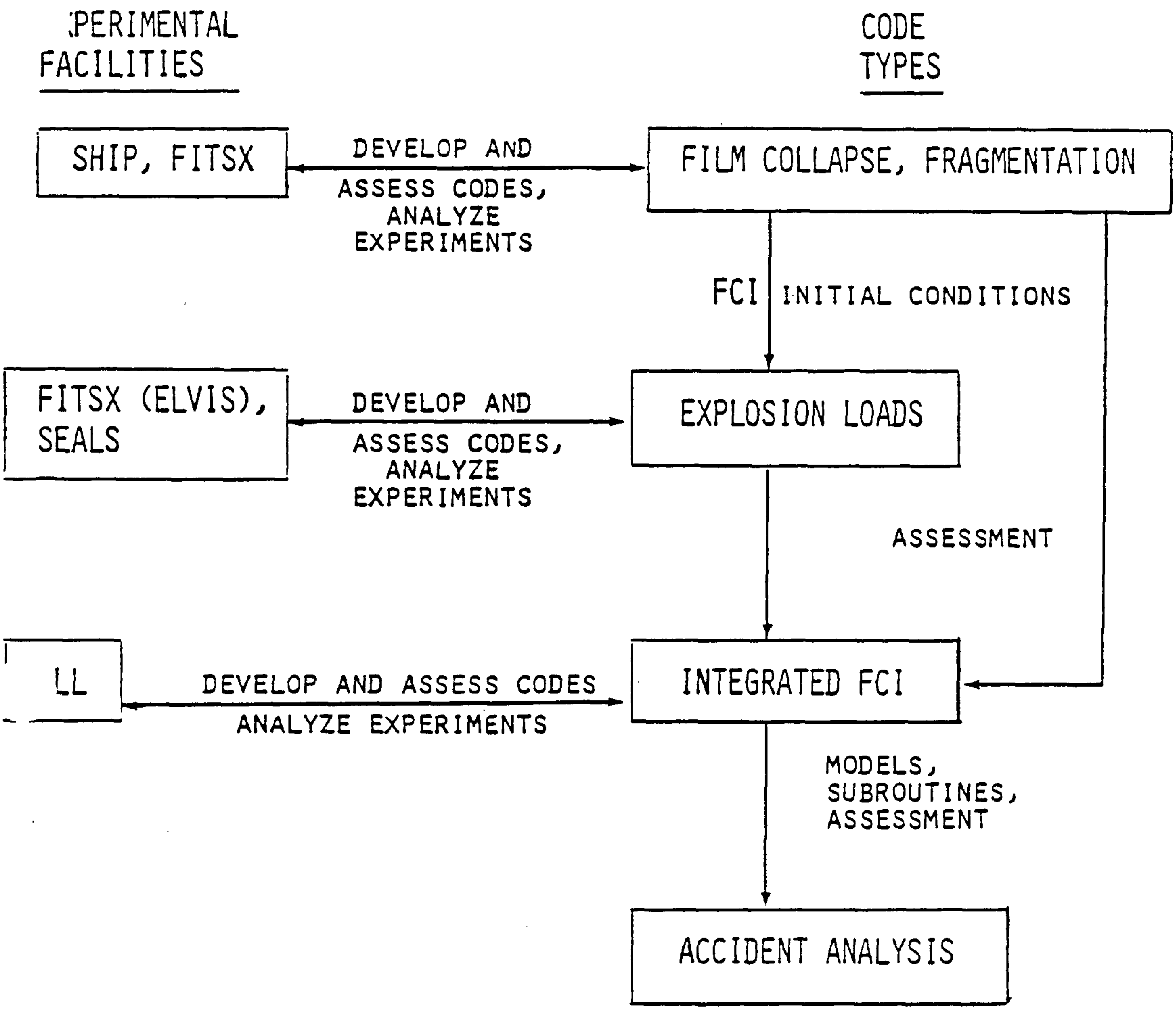

Figure 2. MODEL DEVELOPMENT 1. - END USE: ACCIDENT ANALYSES

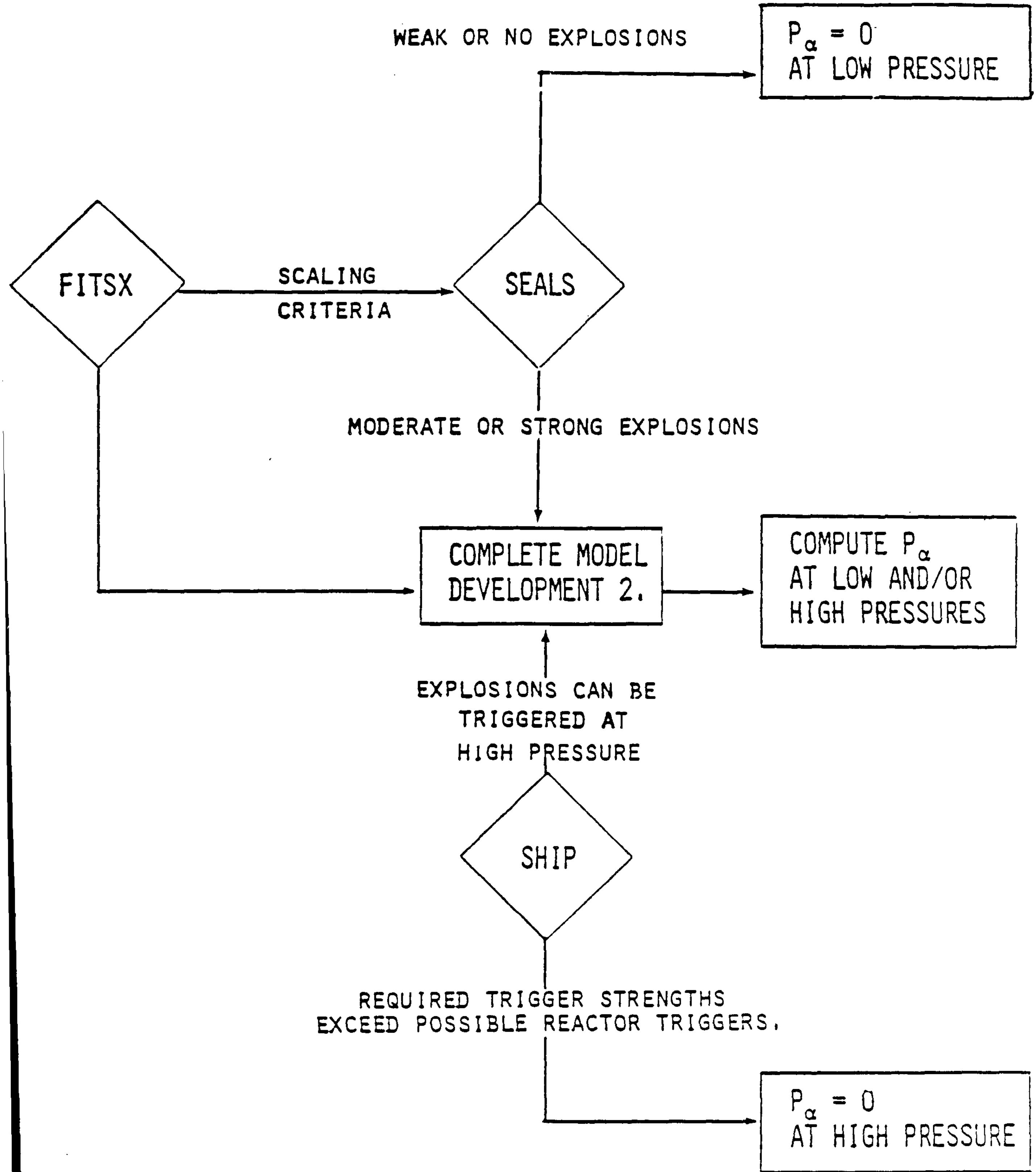

Figure 3. ALPHA-MODE FAILURE PROBABILITY, P_α

ruled out without additional data and more extensive modelling.

The SEALS tests will also indicate the <u>trend</u> in conversion ratio with increasing fuel mass. If this trend showed a strong decrease in conversion ratio with increasing mass, then new models could still result in predictions of low failure probability. It might be necessary, however, to attempt larger pours to restore confidence in the new models.

In the event that either the SEALS or SHIP [1,3] experiments are inconclusive, then additional model development would be required. Figure 4 illustrates the required calculations for mechanistic and probabilistic estimates. Comparing Figures 2 and 4, we immediately see the different modelling approach required. Whereas the accident analysis code is the end product in Figure 2, we must begin with the accident analysis for end use 2, Figure 4. The accident calculations determine the initial and boundary conditions for the FCI. Three alternate approaches are shown for mechanistic evaluations. Current capabilities involve explosion loads codes (e.g., CSQ, SIMMER) which are used to generate pressure histories at the upper head. Structural response codes then determine whether and how the upper head fails. Alternate paths might flow through the integrated FCI code; ultimately, the accident analysis codes themselves would perform the calculation all the way from meltdown to the structural loads. Note that these computations only determine determine whether containment fails or survives for the specified accident scenario.

To determine a failure probability for a <u>class</u> of plants and accidents requires the use of a probabilistic code, similar to the Monte Carlo code used in Reference 11. The simple models in such a code would be updated to correspond with the existing data base and modelling capability of the more sophisticated codes. Input probability distributions which remained would represent parameters which either still contained residual uncertainty, or which were considered stochastic. The code would then sample the input distributions and produce estimates of failure probabilities and the associated remaining uncertainties.

It is important to reiterate that most current models predict conversion ratios much, much less than 2% for 2000 kg in the SEALS tests. Some models predict conversion ratios of zero, and by a wide margin [13].

A similar reasoning process can be applied to alpha-mode failure at high ambient pressure. We begin with an analysis of the range of possible triggers that might occur during an accident; the triggers would be characterized both by peak pressure and pulse width. SHIP experiments could then be used to find a threshold trigger (in both peak pressure and total impulse delivered) and the corresponding threshold ambient pressure. For ambient pressures higher than this threshold pressure, no explosions would be possible. For lower pressures, calculations would be required to determine failure probabilities.

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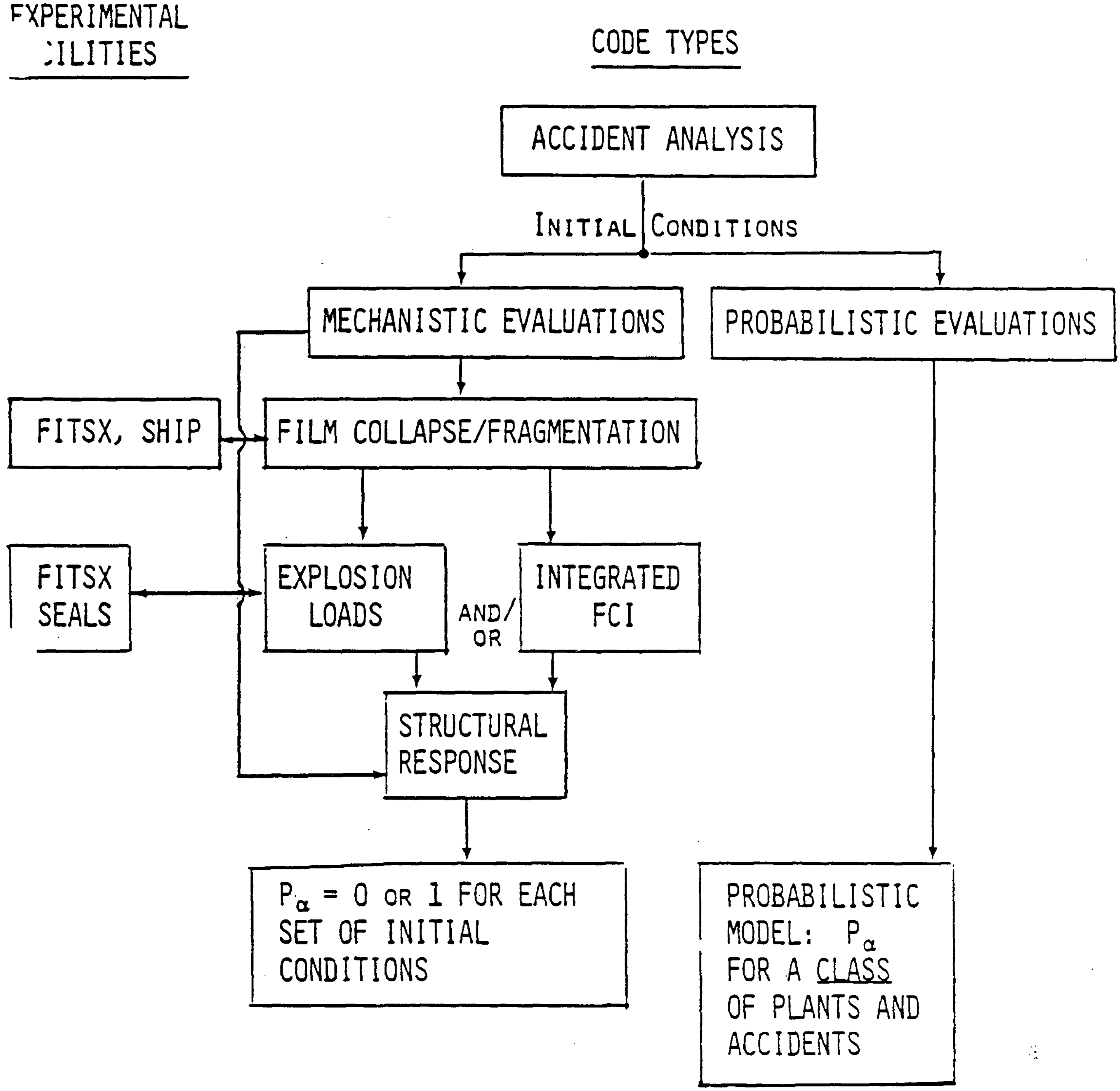

Figure 4. MODEL DEVELOPMENT 2. - END USE: P_α

4. EXPERIMENT ANALYSES AND REACTOR APPLICATIONS

The three-phase approach to the SEALS facility, as discussed in Reference 3, provides many advantages compared to a single step to the final facility. The multi-phase approach allows for:

1. The orderly development of the many components and instruments required for successful experimentation:

2. The generation of timely interim data to meet current needs for decision making and code development;
3. The possibility of changing the facility or the test matrix as a result of interim development and testing, without incurring excessive costs;

4. The truncation of the program with a significant savings in costs and time, if the interim results are highly favorable;

5. The orderly development and assessment of computer codes for analysis of the experiments and extrapolation to reactor scale.

Many of the codes listed in Table 1 serve the dual purpose of analyzing experiments as well as accidents. Both applications require a great deal of skill and physical intuition on the part of the code user, since all the codes entail major approximations and compromises. None of the sophisticated FCI codes can be rationally used as "black boxes." As an example, we will discuss the application of CSQ to the analysis of the experiments conducted in the three phases of the SEALS facility [3]. Figure 5 shows the SEALS3 apparatus prior to a test. Figure 6 shows a typical CSQ representation of the problem. For a SEALS3 test, the flat top on the cylinder shown in the figure would be replaced by a hemispherical dome, as in Figure 5. For the open geometry tests in SEALS2, the top would be open. For the weak-walled lucite chambers in SEALS1, the sides in the calculation would also be removed.

The SEALS1 lucite tests are very important for observing the shape and size of the mixing region during the coarse fragmentation phase. Those tests will also allow observation of the fall velocities, and the reagglomeration and stratification at late times. Fuel and tracer-particle velocities will also be observed and measured. CSQ will be heavily involved in the analyses of these experiments. The initial conditions for CSQ come from the films just prior to the triggering of the explosion. Explosion propagation speeds can frequently be visually determined from the films. Hence, the only parametric variation remaining to completely define the CSQ detonation model is the energy transferred from the fuel to the coolant during the explosion. This parameter can be varied until reasonable agreement is reached for the independent measurements of water phase pressures around the tank.

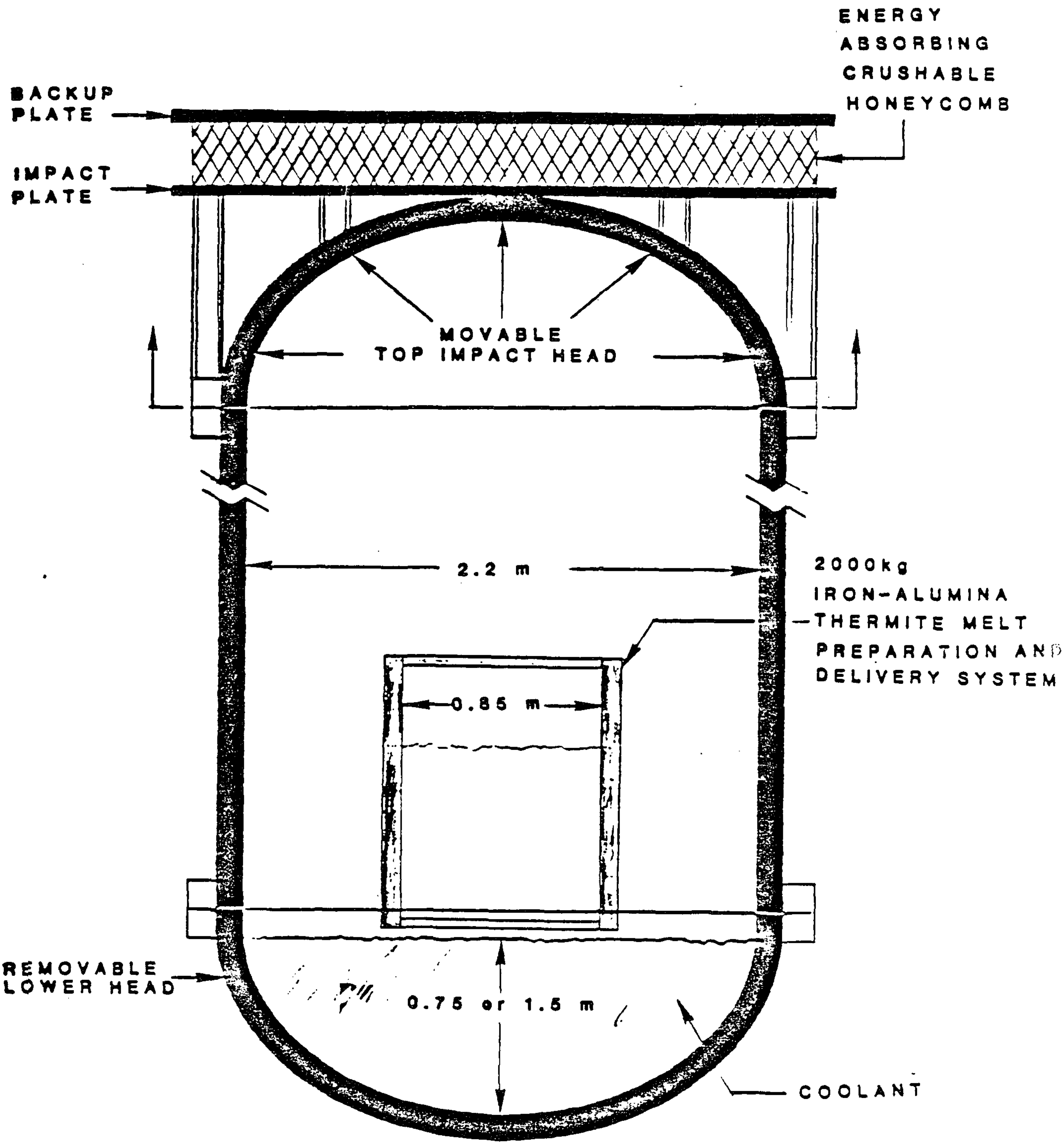

FIGURE 5.
SEALS3
1/2 SCALE REACTOR GEOMETRY

SEALS CSQ PROBLEM

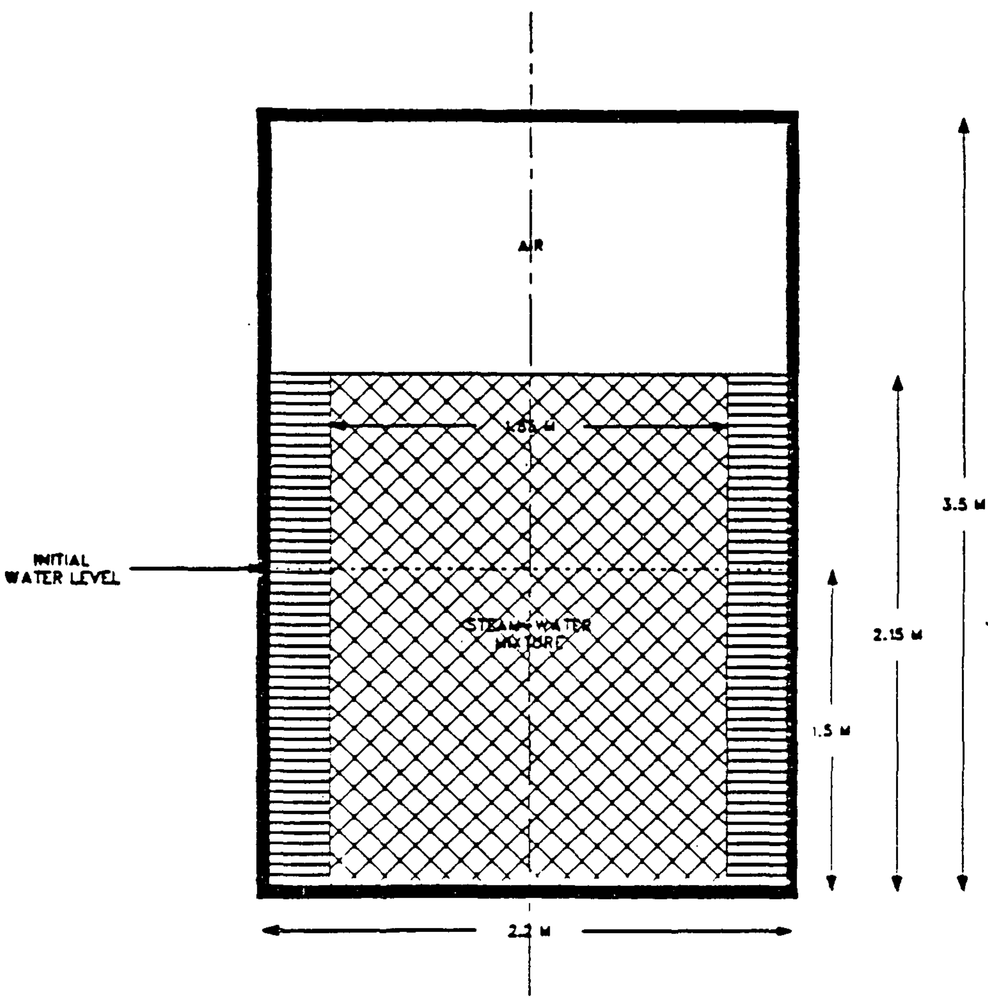

Figure 6
SEALS Chamber Geometry for CSQII

After calibration against the pressure gages, CSQ is now capable of calculating important experimental quantities. The total kinetic energy, and the kinetic energy of each material, are calculated in the code by simply summing $mv^2/2$ for each cell in the problem. Similarly, material velocities are predicted by the code and can be compared directly to experimental measurements. Note that CSQ does not require information concerning fuel particle sizes in the mixing zone. If information on the spatial dependence of energy transfer was made available, it could be modelled with CSQ; for example, if the mixing region contained a core of steam, and the rapid heat transfer was confined to a small region near the periphery of the mixing zone. However, such spatial resolution is not required to achieve reasonable agreement between experiment and analysis.

In a very similar fashion, SIMMER could be used to analyze these experiments. The code user would be required to estimate (or parametrically vary) fuel particle sizes and heat transfer coefficients until reasonable agreement was achieved with experimental measurements of pressure. These calculations would increase our confidence in our understanding of the phenomena if they agreed with the CSQ calculations. If disagreements occurred, the different modelling assumptions in the codes would provide clues for improving the modelling capabilities.

In the SEALS2 tests, the lucite is replaced by an opaque rigid steel chamber. Mixing region geometry and explosion propagation speeds will initially be extrapolated from the SEALS1 tests. Acoustic measurements will be attempted to determine the general shape of the water-mixing region interface. Level swell measurements would provide estimates of average void fraction in the mixing region. Calculations would then be performed using the energy deposition numbers determined from the SEALS1 tests. We currently believe that these predictions would then strongly <u>underestimate</u> the pressures in the explosion and the ultimate conversion ratio. We anticipate this behavior because of observations of a strong explosion in the RC2 test [2]. One possible explanation involves the increase in heat transfer near the rigid walls. In the lucite tests, the initial and only heat exchange between the fuel and coolant takes place in the interaction zone only during the very short time of the interaction; the materials then rapidly move apart and heat transfer falls dramatically. In the steel chambers, the initial interaction is similar, but the fuel and coolant liquids must again approach one another as they collide with the walls. This additional prolonged heat exchange is not accurately reproduced if one uses CSQ input parameters based only on weak-walled vessels.

Another possible explanation for an increase in conversion ratio, suggested by Bill Bohl of LANL, is additional fragmentation of the fuel due to reflected shock waves. Recall that CSQ has no capability for calculating fragmentation due to shock waves, nor can it model the additional heat transfer as a result of increased confinement. The loads caused by the explosion, however, can still be accurately predicted by modifying the code's detonation model; this can be done, given reliable experimental data, by increasing the total energy transferred from fuel to coolant. In summation, we expect that the additional confinement of the steel chamber will result in more efficient explosions; the code can easily model this if provided with the correct information. As before, CSQ will be calibrated against the pressure measurements, and will be used to predict conversion ratios and material velocities.

The final tests in SEALS3 will provide strong and redundant confirmation of the code calculations. The model, previously validated against SEALS1 and SEALS2 tests, will now be required to predict slug properties, the pressure in the gas phase near the upper head, and ultimately the work delivered to the head. If we have done our job adequately, we will have developed a tool which can predict the steam explosion energy itself, and the coupling of this energy to the upper head by means of a multiphase, multimaterial "slug" which impacts the head.

Although the above discussion concentrated on the CSQ code, it applies equally well to other explosion codes, and to the integrated FCI code which is being developed.

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November 20, 1984

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I regret that I have not been able to adhere to your time schedule, since I have been traveling in Japan and elsewhere for the past month. Upon returning to the office on November 16, I found the large quantity of material which you had sent to me, along with other committee members, for perusal. Most of the comments below, however, were written before I left for Japan. This letter contains my reply to Question 1 from Dr. Ross.

Because it is framed in terms of the Monte Carlo probability calculations of NUREG/CR-3369, there is some overlap and repetition in my answers to Question 1 and 2. Apart from this, however, we have been independently performing under an EPRI contract of detailed calculations on fuel-coolant mixing process and estimations of the theoretical work resulting therefrom (Appendix 1). Besides this, we have been working, under an NSF grant, on nonlinear Taylor instability of a water slug being accelerated upwards in a glass column on rupture of a diaphragm. We have arrived at a new correlation which indicates that most, if not all, of the overlying liquid slug in the reactor accident may be eroded away by formation of droplets before the slug hits the top of the vessel. These considerations, plus others which will be detailed below, lead me to believe that the probability is less than 10^{-4} of containment failure, given a core melt, and may be as low as 10^{-6}.

The analysis can be couched in terms of the five stochastic variables which are considered in the NUREG/CR-3369. It is seen that if all five of the variables are in the low range, there is a zero probability of failure, and if all five are in the middle range there is a 10^{-4} probability. Separately, however, we have our own direct estimates of ideal work which imply essentially zero threat to the containment.

Referring now to each of the stochastic variables in the above report:

(1) My subjective estimate of maximum fraction of core molten,

based upon the spatial distribution of decay heat, is that 0.5 would be maximum with a probability density peak somewhere around 0.25. This is based upon various core melt calculations performed in this country and in Germany.

(2) I would make a M-shaped distribution for the pour diameter. The small end of this would be somewhere around 25 cm, since it seems likely that the progression will be fastest along the central regions. As melt-through starts the ablation of the holes will not be large, since triggering occurs, with intact geometry below the lower core plate, in times less than 0.3 sec. Based on simple calculations of the radial thermal boundary layer thickness in the steel during that pour time, it appears that the ablation process will not increase the pour diameter by more than a few centimeters. On the other hand, a preliminary explosion can blow the lower grid plate and diffuser plate out of the way and cause a catastrophic dump of the melt into the pool. This is treated in some detail later.

(3) I would peak the distribution of pour length at 1 meter or less and cut it off at 2. I see no reasonable possibility of the lower support plate being blown away in a preliminary explosion which would not be so massive as to make the second explosion relatively weak. Also, the location of the baffle plate and the shape of the lower support plate, plus the 96 support columns, all tend to make the pour length estimate prior to triggering quite small. An important factor here is that high pressure interactions can be eliminated due to overheating and failure of the primary system seals during the heatup process (Theofanous, et al; also Denny and Sehgal). Furthermore, the time delays before triggering in the FITS and EXO-FITS experiments in Lucite containers may be ascribed to the rapid generation of decomposition gases, which prevent effective contact of the fuel with the container bottom.

(4) We have performed some calculations using the PHOENICS code with the ideal work potential and the conversion ratio resulting from a catastrophic failure of the lower grid plate due to a preliminary steam explosion at or near the surface of the pool, and also for a fragmenting jet from a hole in the center of the lower core plate. It is assumed in the first case that the water from the preliminary explosion enters the core melt region and disperses it into lumps of average diameter 50 mm, which fall downwards into the pool at a terminal velocity of 5 m/sec and a total rate of 24,000 kg/sec. When the fuel drops enter into the water they decelerate to their terminal velocity of approximately 1 m/sec (Appendix 1). Assuming that triggering occurs at various times after the onset of penetration of the fuel front into the pool, the conversion ratio and work output are calculated by the ideal work for each of ten mesh cells based on constant-volume temperature equilibration within the mesh cell, followed by isentropic expansion. These ideal works are summed to give the total work, despite the fact that the initial pressures are not equal for the expansion, although the final pressures are made equal and consistent with the final total expansion volume of 120 $m^3 \pm 3\ m^3$. The maximum work output thus found over the range of 5 to 60 bars was 1.5 GJ. Reducing these by a factor of at least 3 to account

for nonidealities, the total work is found to be nonthreatening to the containment. Similar calculations are shown for the two-dimensional case where a central fuel jet enters the pool at 24,000 kg/sec, and begins eroding at 24,000 kg/sec into 10 cm fuel particles after it enters. The important point is that the conversion ratio, based on the total fuel particles in the pool at the time of triggering, decreases sharply as the time increases. The explanation is that as the radius of the descending fuel cloud increases, the fraction of the fuel which is in contact with very high vapor concentrations also increases, giving a lower theoretical efficiency. Based on these results alone it is possible to rule out the alpha mode of containment failure.

(5) Some calculations are given elsewhere which indicate that the overlying slug of fuel will be largely eroded by Taylor instability before it hits the top of the vessel. Because of the irregular nature of the slug, it is highly likely that premature breakthough will occur, which will result in pressure equalization on both sides of the slug. The effective fuel concentration in the slug is therefore considerably less than 0.25, which is the bottom of the low range in the Sandia report.

(6) Furthermore, I understand that any explosion which might otherwise threaten the containment will certainly blow off the bottom of the vessel first. Some estimates are given in Appendix 2 on the rate of sidewards venting in this case, which indicate significant reduction of the damage potential to the upper head.

All these results lead to an overall probability of 10^{-4} or less, purely from the Sandia Monte Carlo calculations, and probably much less. Independently, however, it is seen that the work calculation gives a total work well below that necessary to fail the containment. The other points simply add to the incredibility of such an event.

Answers to requests numbers 2 and 3 of Dr. Ross, together with appropriate attachments, are enclosed herewith.

Sincerely yours,

S. George Bankoff

S. George Bankoff
Walter P. Murphy Professor

SGB:csp

Enclosures

S. George Bankoff

Reply to Question 2: Comments on NUREG/CR-3369

An Uncertainty Study of PWR Steam Explosions
M. Berman, D. F. Swenson, A. J. Wickett
May, 1984

General Comments

In some ways this study is a throwback to the state of knowledge when the WASH 1400 report was written. It assumes that nothing has been learned about core melting and slumping, or about mixing or explosion limitations in the lower plenum. In this state of ignorance, therefore, it is possible to assign only flat probability distributions over the entire conceivable ranges of the key parameters. This leads predictably to alarming conclusions.

The procedure is to identify five uncertain variables (the fraction of the core that is molten, the pour diameter, the pour length, condensed-phase volume fraction in the slug, and the conversion ratio), to each of which a subjective probability distribution is assigned over the largest conceivable range of variation of the parameter. A Monte Carlo analysis is then performed, in which each of the parameters is sampled according to the probability distribution, and the resulting scenario calculated. The end product of each calculation is an estimate as to whether a large missile will be be created which will fail the containment. By repeating this procedure many times an estimate is obtained of the probability of containment failure from an in-vessel steam explosion, given a meltdown accident in a PWR. As can be seen, however, from Table 5 (page 39) a choice of these five parameters anywhere in the upper third of the range of variation leads to containment failure. There is thus no need for computer calculation, since the probability of failure is at least once in $3^5 = 243$ meltdowns. Although the calculations were apparently not made, it seems likely from the figures given that the probability of containment failure is very close to 1 if any 4 of the 5 parameters are chosen from the high range and the 5th parameter is chosen from the middle range. This brings the probability of containment failure to nearly 1 in every 100 meltdowns. Detailed calculations would probably show that the probability of failure is even larger than this by a significant amount. This is obviously a matter of considerable concern, and justifies the careful examination of the procedure. Because of the complexity of the subject, the remainder of these comments will be keyed to individual pages of the report in sequence, followed by a conclusions section.

Specific Comments

Page 8, middle of the bottom paragraph: The justification for using rectangular distributions is misleading. Any other distribution would also cover the whole parameter range and allow sampling from different parts of the ranges separately. The property of the rectangular distribution is that it assumes that absolutely nothing is known about the behavior of the variable, and hence no preference can be expressed for different parts of the ranges. This is, of course, a subjective probability and is quite simple to employ, but begs the fact that a great deal is known from physical reasoning, computational analysis, and experimental information which allows one to do much better than this. In a sense, the choice of these distributions justifies the avoidance of detailed physical calculations.

Page 8, bottom of the last paragraph: I do not understand these two sentences since they imply that the distributions do not cause underestimation or overestimation of the uncertainty in the containment failure probability. If the uncertainty is neither under- or overestimated, this means to me that the estimation is exact.

Page 10: The five uncertain parameters are all seen to be given by rectangular probability distributions which are independent of each other. This is clearly incorrect, since conditional probabilities must be involved. The mixing limitation and the conversion ratio of the fuel with water, as shown by our calculations, depends upon the total mass of fuel entering the water. The pour diameter will depend upon the fraction of the core that is molten.

Page 11. It is difficult for me to visualize a situation in which 0.75 fraction of the core is molten before mixing with water takes place. Slumping and melting takes place first near the central regions and only later towards the low power density periphery. Once a crust has formed on the bottom, the heat generated in the pool is dissipated only by melting the crust either downwards or sidewards. The crust becomes thinnest therefore at the bottom center and breakthrough occurs first there. If the crust is thermally unstable due to fracture, it will fail even more readily, but in any case, penetration would be expected first in the region of the bottom center. Another possibility is sidewards penetration through the core barrel if the crust on the vertical walls is not adherent and tends to fall away. This is conceivable, since the melting point of steel is below the freezing point of UO_2, but rather unlikely since the crust-steel interface temperature would not be raised to this level until the crust is quite thin. In either case, it seems to me that a figure of 0.5 is a more realistic upper bound than 0.75.

Pages 12 to 14: It is misleading to state that the volume of melt participating in the explosion is the product of the area of the hole and the pour length. The actual fraction of melt participation is generally considerably less than unity. This statement makes sense only in terms of the Sandia definition of conversion ratio, which will be discussed later.

Referring now to Figure 2, it seems likely that the initial breakthrough will be in a relatively small area. A significant increase in this hole area during the time of the melt pour before triggering can be easily discounted as follows. Triggering can occur at any location at up to 30 ms after hitting the diffuser plate, as shown by the FITS results. This gives a pour time about of 0.1 sec before triggering. During that time the radial thermal boundary layer growth outwards from the hole in the lower core plate is about 1 cm if no melting takes place. Since melting does occur, and since the edges of the plate may be covered by frozen melt, the actual increase in radius may be less than this.

There are two major points in this connection: (1) the small explosion which would then occur brings up the possibility of multiple explosions (more than two and possibly as many as ten), which would essentially preclude containment failure due to this scenario. (2) It should be realized that water can be driven upwards into the overlying melt region by this mechanism, which would greatly disperse the melt above the lower core plate and lead to incoherent interaction. This possibility is not considered in the Sandia report. The probability distribution for pour diameter should thus be M-shaped, with the peaks at about 0.5 m and 2.5 m.

If, however, the interaction is not triggered when the melt hits the diffuser plate, as might occur at high pressures, it might fall down to the lower support plate where again an interaction might not be triggered and finally down to the bottom of the vessel. In this case, the interaction may never proceed, and the vessel would simply melt through. The high pressure scenario is thus very different from that in near-atmospheric pressures. It should be realized, however, that the high pressure scenario is very unlikely, due to failure of the seals in the primary coolant system resulting from natural convection during the meltdown process. This important point has been discussed in detail by Theofanous et al. Earlier free-convection calculations by Denny and Sehgal resulted in similarly high PCS temperatures.

Similar considerations apply to the scenario shown in Figure 3. It is difficult to believe that the pour will initiate over one-quarter of the circumference, and as mentioned above, the increase in the area of the hole would be small over the pour times involved. Page 16: For the reasons mentioned above the likely trigger location is the diffuser plate. Clearly the probability of the diffuser plate being blown out of the way depends upon the size of the pour hole, and this in turn determines the pour length. Hence, conditional probabilities are involved. It is highly unlikely, in my opinion, that the lower support plate, which is quite massive and also contains numerous holes through which the pressure can be relieved, can be blown away by a preliminary explosion. Hence, the probability distribution should also be peaked sharply towards the smaller pour lengths (peak 1m).

Page 17, top of first paragraph: The fact that spontaneously-triggered surface explosions have been observed by Krein and Berman with hot water is very interesting, and is in fact one of the important results from this experimental program. At near-atmospheric pressures the water in the lower plenum will certainly be saturated and all future experiments should be at atmospheric pressures with a full plenum of saturated water.

Pages 17 and 18. Mixing limitations. This is, of course, the heart of the problem about which there has been considerable controversy and for which the Sandia experiments are designed to give information. The Henry-Fauske model is discounted on the basis that it requires a steady-state one-dimensional flow pattern. It is true that this model is based upon extremely simple calculations, but it does not seem to me that it should be attacked on these grounds. For a very large explosion, which is needed in order to threaten the containment, the pour diameter must be a very significant fraction of the diameter of the core in order to obtain mixing of 24,000 kg of fuel in the pour time before spontaneous triggering takes place at atmospheric pressure. (The figure of 24,000 kg of fuel is arrived at by multiplying the thermal energy content of 1.2 GJ/(metric ton) and a conversion ratio of 5%, giving an explosion energy of about 1500 MJ.) In this case the flow pattern will be essentially one-dimensional except at the edges. Furthermore, it is not at all essential for the steady-state assumption to be satisfied for the basic idea that very poor mixing of liquid water and fuel is obtained in a one-dimensional flow. This is shown in the attached paper of Bankoff & Han, where fuel particles of diameter 5 cm were assumed to fall into the pool at a rate of 9000 kg/sec and a system pressure of 1.0 MPa. A diagram of the system is shown in Figure 1 of the paper. Figure 2 shows the fuel volume fraction in the mixing zone as a function of space and time over the period of 1 second, and Figure 3 shows the void fraction in the coolant region as a function of the same variables over the same time interval. The initial pool level was at 2 m and the dotted vertical lines show the pool swell level at the various times. It is seen that the fuel particles are indeed levitated upward by the ascending steam and that the region around the bulk of the fuel particles has a void fraction, based only on the steam/water region, of over 0.7. These figures illustrate that it is not adequate simply to specify the amount of fuel that has been mixed, but it is necessary to get a measure of the quality of the mixing. All 9000 kg have been mixed in the sense that the fuel is surrounded by coolant phase, but the water has been driven out of most of the region surrounding the fuel particles, and most of the water in the lower part of the plenum has very little fuel in it. There was thus mixing, but very poor mixing, which greatly influences the theoretical, as well as the actual, explosion efficiency.

A more viable objection to the Henry-Fauske theory is the unspecified mode of subdivision of the fuel until the critical heat flux criterion is satisfied. In this regard, the substitution by Corradini (page 18) of a fluidization criterion, rather than a critical heat flux criterion, for levitation of the water seems to me to be clearly wrong, and leads to steam velocities an order of magnitude too high. This is a countercurrent flow limitation (flooding) situation, with water as the continuous phase, rather than vapor, in the initial stages when the fuel particles enter the water. Hence, the critical heat flux criterion, which is essentially the same as the CCFL limitation of Kutateladze, both of which correspond to a continuous liquid phase, is the correct value to use.

Similarly, there are objections to the Corradini model which may be mentioned here. As I understand it, the fuel and coolant are assumed to mix in packets at the surface of the pool, and the volume of the packet increases as it falls due to the generation of vapor within the packet. Empirical correlations based on FITS measurements are used for the key variables in the packet. This does not take into account (except possibly indirectly) the levitation of the fuel particles by the up-flowing steam, or indeed the relative velocities of fuel and coolant. This is, indeed, a serious limitation.

Page 19. Fraction of water that mixes: Although the mixing zone in the FITS experiments contained 40 to 60 percent by volume of liquid water, the uniformity of mixing was poor, as shown in the attached paper by Hadid, Han, & Bankoff.

Page 19. Conversion ratio: Sandia employs the concept of conversion ratio based on total fuel in the pool, which is the fraction of the total excess thermal energy of the melt entering the pool which is converted into kinetic energy of the upward moving slug. This avoids the necessity of estimating how much of the melt actually participated on an explosive time scale, although it could be extracted from the FITS data. However, this information would not be available in any large-scale open-vessel test. Winfrith has also performed closed-vessel tests in which the debris was collected and sized. They used the criterion of 280 microns for the split between particles which participate and those which do not. On this basis the efficiency (or conversion ratio based on particles smaller than 280 microns) appears to be fairly independent of system pressure. The conversion ratio based on the total fuel in their tests increases with pressure, largely because of the increased fraction of the fuel which participates in the explosion. The implication then is that the total conversion ratio would level out at somewhere near 1 MPa, when nearly all the fuel participates. Detailed one- and two-dimensional calculations of ideal mechanical work and conversion ratio are given in Appendix 1.

Note that high-pressure fuel-coolant mixing can be ruled out, based on PCS excessive temperatures, leading to failure. Furthermore, at pressures of the order of 10 MPa, triggering is difficult or impossible. This is shown by the many submarine vulcanisms on the ocean floor, which do not cause large-scale explosions. An enormous coherent release of explosive energy, such as occurred at Krakatoa, where it was estimated by the Royal Commission that approximately six cubic miles of sea water had entered the base of the volcano, could only occur, in my opinion, if a sudden shift occurred which allowed the pressure to be relieved rapidly, at which point the explosion took place.

At low pressures the attached papers and Appendix 1 show that the conversion ratio will be in the low range.

Page 22: The kinetic energy which is relevant is that of the upward-moving mass. The tests with weak sidewalls, although they are useful in obtaining visual information, do not allow realistic estimates of this quantity. The idea that all the melt that does not participate in the explosion remains above the explosion, and that all the water not participating in the explosion lies below the explosion seems reasonable to me. This implies that most of the upward-moving slug is, in fact, melt.

Page 23. Slug composition: The question is whether there will be a slug at all, based on Taylor instability of the slug lower boundary. The volume fraction of fuel in the time interval of impact will thus be much less than 0.25. Simple calculations indicate that the interaction of 20,000 kg of fuel and 5000 kg of water, with 10 percent conversion efficiency would give an explosion energy of 3000 MJ. This would result in an initial pressure immediately after mixing before the expansion phase begins of 240 MPa, resulting in an acceleration of 1.23×10^5 m/sec^2. Experiments in our laboratory in which a liquid slug is being accelerated upwards in a column by an expanding gas mass find that the velocity of entrainment front into the slug goes as the square root of the acceleration rather than the fourth root. The best-fit equation to our data is $v_e = 0.13\ g^{1/2}$, where v_e is the rate of erosion of the bottom of the water column, in m/sec, and g is the acceleration of the upper surface of the column. Note that previous studies have all used downwards acceleration. This gives an entrainment velocity of about 14 m/s which would penetrate a fuel slug of average thickness 0.36 meters (20,000 kg) in about 0.025 seconds, assuming for conservatism that the same acceleration of 1.23×10^5 m/sec^2 is sustained throughout the expansion by entrainment of molten fuel drops. The time of impact on the vessel head would be approximately 0.04 sec. Hence, breakthrough occurs before impact, which substantially reduces the total kinetic energy of the upward moving mass, and reduces the volumetric fraction of fuel to less than 0.25.

Summary and Conclusions

Principal criticisms are:

(1) Some important factors, such as the lack of uniform mixing in the lower plenum at low pressures, the high probability at low pressures of premature triggering when the melt hits the water surface and/or the diffuser plate, and the strong probability of breakup by Taylor instability of the upward-moving slug have not been identified, or are mentioned only in passing.

(2) The uniform probability distributions for those factors which have been identified employ ranges which are close to the maximum conceivable for each parameter, and then assume uniform probability

distributions. These are assigned without regard to physical calculations to check their reasonableness. Consequently, highly-biased results are obtained. No attempt is made to define conditional probabilities for these parameters, although several of them are clearly interdependent.

In summary, the authors have made a serious attempt to employ Monte Carlo techniques to a difficult problem. However, assumptions are such that I would have little or no confidence in the results.

S. George Bankoff

Question 3: Discussion of Proposed Sandia Experimental Program

General Comments

1. The principal purpose of these experiments is to distinguish between the Henry-Fauske and Corradini mixing models. However, as shown by our calculations, all fuel which goes down into the pool is mixed in the sense that it comes in contact with coolant (steam/water). However, the quality of the mixing is central in that some proportions of fuel/steam/water give high ideal efficiencies (assuming isentropic coolant expansion) while others are very low. The low values occur either because the equilibrium temperature is quite low (small amounts of fuel) or because very little heat is extracted from the fuel before the equilibrium temperature is established (small amounts of water). Our calculations indicate that relatively poor mixing, in the above sense, will prevail at low pressures. This is consistent with both the Henry-Fauske and Corradini models. However, the proposed experiment will tell us nothing about the quality of the mixing.

2. I have previously suggested that resistivity probes made of refractory materials be employed throughout the pool in order to give information on the local vapor/liquid content and/or the arrival of molten material. I am very dubious about obtaining conversion ratio data from measurements of the velocity by high-speed cameras of the water being driven upwards. In such a large explosion the central mass is invisible, and I would expect that only the water on the edges would be seen. Further, there is no way to measure the mass of water being driven upwards from the picture. The crushable base information does not allow us to predict the mass of the upward slug.

3. It is a waste of time and money to do experiments in weak-walled (Lucite) containers. The weak walls dissipate the explosive energy which might be directed towards producing an upward-moving slug, resulting in a highly nonconservative experiment. On the other hand, the existence of an appreciable delay time after the falling fuel mass hits the bottom in several of the FITS experiments is, in my opinion, an artifact of the experimental materials employed, and does not have bearing on the actual accident sequence. The Sandia group have obtained some excellent information on corium-water interactions, but there is a tendency to ignore all previous work on fuel-coolant interactions. This has led them to overlook the classical series of experiments by Long pouring molten aluminum into water, also on a 20 kilogram scale. Long found that a coherent jet of aluminum falling on to the bottom of an open steel tank, with water depth below that required to produce freezing, always resulted in vigorous explosions. However, when he painted the bottom of the tank with tar or grease, the explosion was not triggered, or was highly variable in triggering. Other organic materials had a similar effect. The reason advanced by Hess and Brondyke was that thermal decomposition of the organic

coating produced noncondensable gases which prevented good thermal contact between the melt and the bottom, and hence prevented entrapment of water under the fuel. Exactly the same phenomenon would be encountered with polymethyl methacrylate, although perhaps at different rates.

4. Recent experiments at Winfrith indicate an almost constant explosion efficiency for the part of the fuel which participates in the explosion, independent of the pressure up to about 10 bars. This seems quite reasonable to me, since one would expect that the energy received by the coolant in the time scale of the explosion, and hence available to do explosive work, depends only upon the actual heat transfer from the fuel, and only weakly on the system pressure. On the other hand, the total conversion ratio and also the total work increases sharply with system pressure in the closed vessel, because the fraction of the fuel participating increases with increasing pressure. The fact that the bolts were permanently stretched in the highest pressure test is indeed significant and requires further study. The effect of the rigid container walls is to prevent premature dispersal mixing during the explosive phase. This property cannot be studied in an open-type geometry, but is of great importance.

5. In reviewing the methods used in estimating conversion ratio for the FITS 1B series, I find that I have little faith in the calculated results. The pressure histories show a sharp spike, corresponding to the arrival of a gas-phase pressure shock wave at the top of the chamber, followed by a very rough plateau pressure, which has been used in estimating the total conversion ratio. This indicates that breakthrough occurred from the explosive gaseous expanding mixture through the surface of the liquid out into the chamber region. If, indeed, a coherent liquid slug had been produced which covered the cross-section of the chamber, there would have been no gaseous pressure shock wave at the top. The breakthrough negates the compression work calculation. The measurements of the sidewall velocities by camera, and also the integration of the output of the wall-mounted pressure transducers are helpful, but it is very difficult to estimate the relative amounts of kinetic energy imparted in the six directions. The Winfrith explosion efficiency measurements are more convincing, since, in this case, the melt was contained in a massive cap which detonated when the cap hit the bottom. It seems reasonable then, that pressure gauges in the gas space above the liquid could give a measure of the mechanical work done by the liquid in compressing the gas above it (assuming again that no breakthrough of gas occurred). It would have been useful, however, to measure the temperature of the gas with fast response instrumentation at the same time as the pressure was measured in order to check the assumption of isentropic compression with no added gaseous mass. More importantly, the Winfrith experiments were highly non-prototypical in the method of introducing the melt into the water. The pouring mode of contact envisaged in the SEALS experiment, and used in most previous Sandia experiments, is in fact the correct one to use for estimates of damage work potential. However, this mode poses severe difficulties in

measuring slug kinetic energies, as noted above. A preliminary experimental and analytical program is therefore indicated.

6. I do not believe that the experimental design is appropriate. A factorial design determined in advance can help estimate, in a particular geometry, the importance of various factors in the result. With poor reproducibility, as is inherent in this type of experiment, this implies that a fairly large number of replications are needed in order to give adequate confidence limits. However, the object of these tests is not to flood the entire parameter space with information, but to determine the worst possible condition and determine how bad it is. A hill-climbing procedure, such as involved in an experimental search for an optimum, is therefore called for. One should design a realistic (prototypic) scaled-model experiment, and begin with what is believed to be the worst possible combination, in terms of work output. One should then make variations on this with one parameter, or perhaps two parameters, at a time, and observe the results. If the results are improved (in the sense of having higher work output), then additional moves should be made in the same direction. The whole technique of experimental search for an optimum is well-advanced. The time-series analysis of Box and Jenkins, and hill-climbing techniques, such as conjugate gradient methods, should be reviewed. In my opinion, this is the only way to locate the worst possible case and also to estimate its frequency.

7. An important, and perhaps insufficiently emphasized, argument against <u>any</u> open-vessel tests is the undue and unnecessary fears that would be induced in the general public and into the law-making bodies by films of such events. Since they would be quite unrepresentative, they can only inflame the present bias against nuclear power.

8. There is no need to investigate the effect of pressure, since calculations by Denny and Sehgal and also by Theofanous and his group have shown that the primary coolant system will fail due to natural convection prior to delivery of the melt to the lower plenum. Furthermore, there is no need to look at subcooled water, since the flashing to atmospheric pressure will certainly leave the lower plenum full of saturated water. These represent two tremendously important time and money savers.

9. There seems to be some idea that fundamental information can be obtained by doing these experiments in simplified geometries under non-prototypic conditions. Large experiments of this type are virtually useless for this purpose, since practically no information can be obtained concerning the initial quantities and locations of the fuel, water, and steam at the instant of triggering, and the open geometry even precludes the collection of debris and the estimation of fraction of the melt which actually participated. In any case, this a costly and indeed wasteful way of obtaining fundamental information, when so little fundamental information has been obtained from the 20 kg tests.

Recommendations

The emphasis should be on performing a few tests, if at all, which are highly prototypical, particularly in the vertical length scales, and are highly instrumented to give all possible information from each test. I am afraid that I would also have to recommend that the single-drop tests at high pressures be eliminated, since there seems to be no realistic probability that high pressure in-vessel steam explosions can occur. Furthermore, I would recommend that the proposed 50 kg FITS tests be eliminated in favor of consolidating the 50 kg series and 400 kg series into a single test series at 200 kg.

Thus my recommendation is that the entire test program as outlined by Sandia be scrapped, and that the money saved be deployed elsewhere. Alternatively, it could be used to run a closed-vessel, 200 kg melt test series with prototypic length scales in the vertical direction and area scaling in the horizontal direction. This is similar to steam generator and core reflooding experiments in which full-scale vertical construction is employed, but with a small number of tubes or pin bundles. In these cases, the characteristic times and vertical velocities are all important, and cannot be extrapolated with confidence from shortened geometries. The same is true here, where the delay time before triggering depends upon the obstacles in the lower plenum, and the energy of the upward-moving slug impacting the upper head depends upon obstacles in the upper plenum and core, together with the tendency for slug breakup due to instabilities. Thus, if we are modeling a 20,000 kg fuel drop into the lower plenum, the diameter of the vessel in a 200 kg drop should be 0.3 meters, and the diameter of the pour hole should be 0.1-0.2 meters. The full-length vertical scales should be preserved, including the diffuser plate and the lower core support plate in the lower plenum, together with the proper spacing of support columns. Obstacles corresponding to the upper support plate, intact fuel rod lengths, and control rod guide tubes should be present. The exact geometry of these obstacles need not be preserved, but only the fraction of cross-sectional area occupied and their approximate mass. The bottom of the thermite vessel should have an insulated trapdoor arrangement as shown in the SEALS design, with the top open so that the explosive mixture can expand through it. Some refractory conductivity probes and/or thermocouples should be supported by, or embedded in, the lower plenum steelwork in order to give readings of void fraction as long as possible, and to also indicate the time of arrival of the melt. An impact plate should be mounted below the bottom head, which is instrumented with accelerometers and strain gauges, in order to determine the work done by the impact. As noted above, only saturated water at atmospheric pressure covering all, or nearly all, of the full height of the lower plenum should be employed. This considerably reduces the range of possibilities. Flash x-rays should be used for following the melt and also possibly the explosion. An experimental series of this type map be considerably cheaper than the proposed program as it now stands, and can be concluded in a much shorter time. My guess is that it will only require fewer than ten experiments to settle the question whether or not the alpha mode failure is a serious concern.

Appendix 1

To: Dr. M. Merilo, Electric Power Research Institute
From: S. G. Bankoff, Northwestern University

Progress Report for October-November 1984

Contract EPRI-2509-1
Modeling of Fuel/Coolant Thermohydraulics

Work has concentrated on fuel water mixing, and ideal work calculations, using prototypical parameters. In addition, some consideration has been given to the formulation of an explosion program, using the PHOENICS code. Only the former work will be reported on at this time.

1. Ideal Efficiency and Theoretical Work of a Steam Explosion Resulting from a Catastrophic Failure of the Lower Grid Plate
(S. H. Han)

We consider here the possibility of a preliminary steam explosion which blows away the diffuser plate and lower grid plate, allowing the rest of the molten core material to dump into the lower plenum. The preliminary explosion would inject water and steam into the molten core region in amounts depending upon the depth of penetration before the explosion and the size of the initial pour hole. This water will interact with the core melt and will disperse it violently. However, because of the constrained geometry the coarsely-fragmented corium will fall back down into the lower plenum. It is assumed that the water pool is 4.2 m in diameter by 3 m in depth and that 24,000 kg/sec of fuel ar 3000 K falls into it with average particle diameter of 5 cm. These particles fall at their terminal velocity in the gas region of about 5 m/sec and are slowed down when they hit the pool to their terminal velocity in liquid of about 1 m/sec. A one-dimensional calculation , using ten mesh cells, was performed in which the distance of the fuel front from the bottom of the vessel at the time of triggering was considered to be a parameter. The ideal total work was calculated by summing the ideal work for the fuel-coolant in each mesh cell at the time of triggering separately, assuming the specific heat of corium equal to 0.64 kJ/kgK. In each cell it was assumed that the fuel and steam and water mix at constant value to reach temperature equilibrium instantaneously, independently of all other cells. The working fluid, consisting initially of supercritical water, was then allowed to expand in perfect thermal contact with the fuel in the cell. This is thus a Hicks-Menzies (H-M) calculation, and is hence very conservative. The total volume of all regions at the end of the expansion was constrained to be 120 ± 3 m^3. This requires an iteration loop to determine the final pressure. Note that this calculation is even more conservative than H-M, since the initial pressures before expansion are not equal in the various regions, nor are the times of expansion. These incoherencies will reduce the ideal work.

The total mass of fuel as a function of the distance from the bottom surface is shown in Fig. 1. Taking these mixing data as initial conditions for adiabatic expansion of the mixture, the maximum work was calculated. Figure 2 shows the maximum work and the corresponding conversion efficiency of thermal energy at various initial pressures.

(1) The efficiency decreases as the trigger time, and hence amount of fuel which interacts, increases, ranging from a maximum of 28 percent when the fuel front has only entered 0.1 m, down to 3.5 percent when the fuel front has penetrated 2.4 m.

(2) The effect of pressure is strongest at the very low penetration distances, with the efficiency decreasing with pressure.

(3) The total work output, on the other hand, increases as the fuel front penetrates, flattening out at about 1.5 MJ when the fuel front is nearly at the bottom of the vessel. The pressure has little effect at this depth of penetration.

(4) The effect of decreasing pressure is to decrease the efficiency and work output. This effect is particularly strong when the front has only penetrated 1 m or less, which is the likely range of triggering, in view of encounters with displaced core support tubes, remnants of the lower grid plate and diffuser plate, and peripheral portions of the lower core support casting. We have been unable to obtain convergence at a system pressure of 1 bar, presumably because of the very rapid volumetric vapor generation rates which result in local concentration shocks. However, it can be seen that a five-fold decrease in vapor density results in a substantial decrease in work output in the range from 25 to 5 bars, and it can be presumed that a similar decrease will take place in going down to 1 bar.

One can apply a factor of at least 3 in reducing these ideal total work values to actual work done on the overlying slug of liquid. This is conservative since most experiments show a reduction of an order of magnitude or more. Hence, the maximum work output would be expected to be of the order of 0.5 GJ, which is insufficient to threaten the containment.

2. **Fuel Jet Fragmenting While Falling through the Pool: Two-Dimensional Mixing and Ideal Work Calculations**
(A. Hadid)

In this a fuel jet is assumed to fall at the rate of 24,000 kg/sec and a velocity of 6 m/s at the center of the pool. As it penetrates the pool, its surface fragments into 0.1 m diameter particles at the rate of 24,000 kg/sec (Fig. 3). The particles are thus released as a line source along the pool centerline at a uniform rate over the length of the jet. Ten axial nodes and five radial nodes are employed. The ideal work and efficiency calculations are the same as outlined above for the one-dimensional case, except, of course, that 50 mesh cells, rather than 10 mesh cells, are involved. Velocity and concentration fields are shown in Figs. 4 and 5 at time 0.25 seconds.

The efficiency decreases with time and levels off to about 5% for the two-dimensional problem (Fig. 6), and 3.5% for the one-dimensional problem. The maximum theoretical work output for 24,000 kg fuel in a one-dimensional calculation after one second is about 155 MJ, and for the two-dimensional calculation for 6,000 kg fuel after 0.25 s is 760 MJ. Additional work is being performed to extend the mass of fuel mixed from the jet to larger values, which require overcoming convergence problems at these massive fuel input flowrates.

Appendix 2

Energy Dissipation by Bottom Failure

I understand that recent calculations at Los Alamos indicate that failure of the bottom is virtually certain under conditions where the containment might be threatened. The estimate of the reduction of the kinetic energy of the upwards-moving slug due to failure of the vessel bottom in NUREG-3369 seems much too low. It is based on an elastic collision formula, in which the energy partition between two bodies of unequal mass, initially in contact and at rest, is inversely proportional to their masses. For the case of bottom failure, however, the two bodies flying apart are continuously in contact with an expanding gas/liquid mass. The one-dimensional equations of motion of the two masses are simple to write down, but do not admit an analytical solution if side-venting is taken into account, and I have not carried out the computer solution. However, some qualitative remarks can be made. If the entire bottom of the vessel comes off, the vent area is proportional to its distance of travel, and becomes equal to the core cross-sectional area at a distance of travel of 0.8 m. For a 20,000 kg liquid slug and 30,000 kg bottom plus contents, the velocities at any instant are in the ratio 1.5, and hence also the distances traveled. Venting is thus essentially complete when the slug has traveled 1.2 m, at which point the energy is only about 20% of its maximum explosion expansion energy, $P_oV_o/(k-1)$.

If, on the other hand, the bottom fails along the line of instrument, etc. penetration welds, as might occur if the melt were stratified on the bottom at the instant of triggering, venting could occur essentially instantaneously.

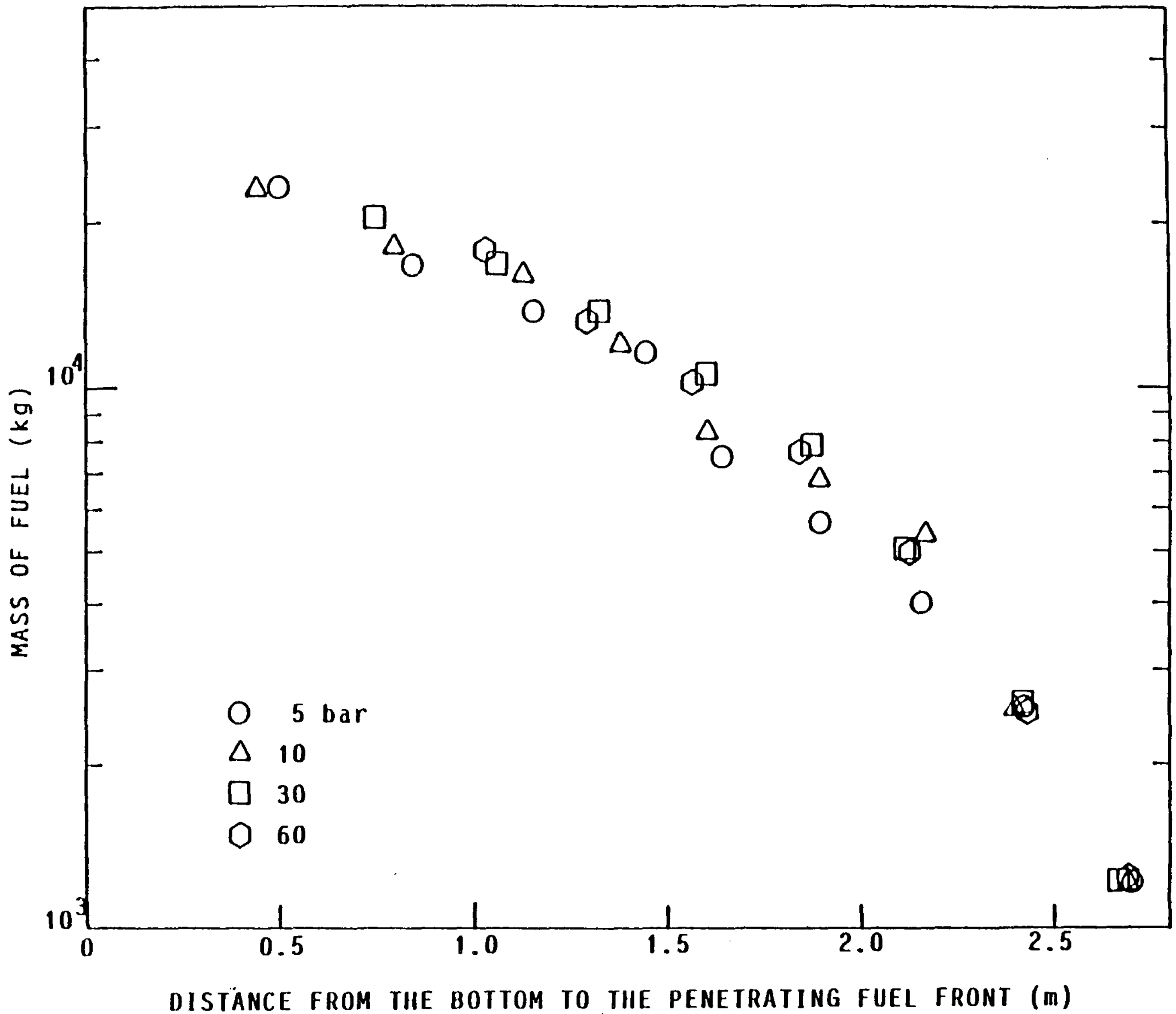

Fig. 1. Mass of fuel vs. fuel front.

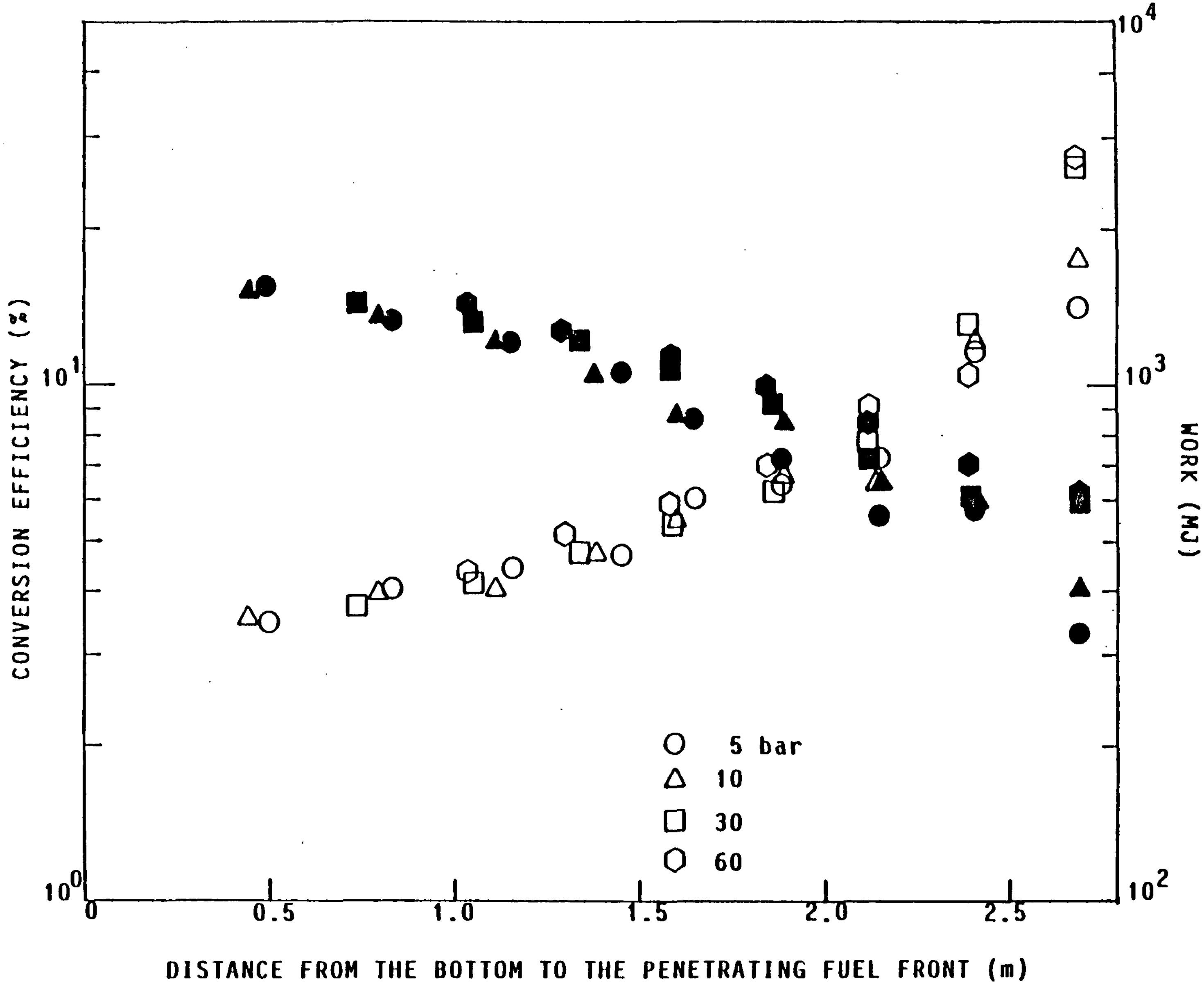

Fig. 2. Work (closed) and conversion efficiency (open) vs. fuel front.

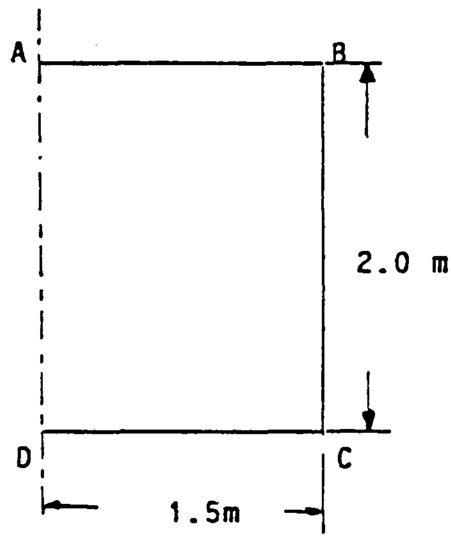

Fig. 3. Vessel geometry.

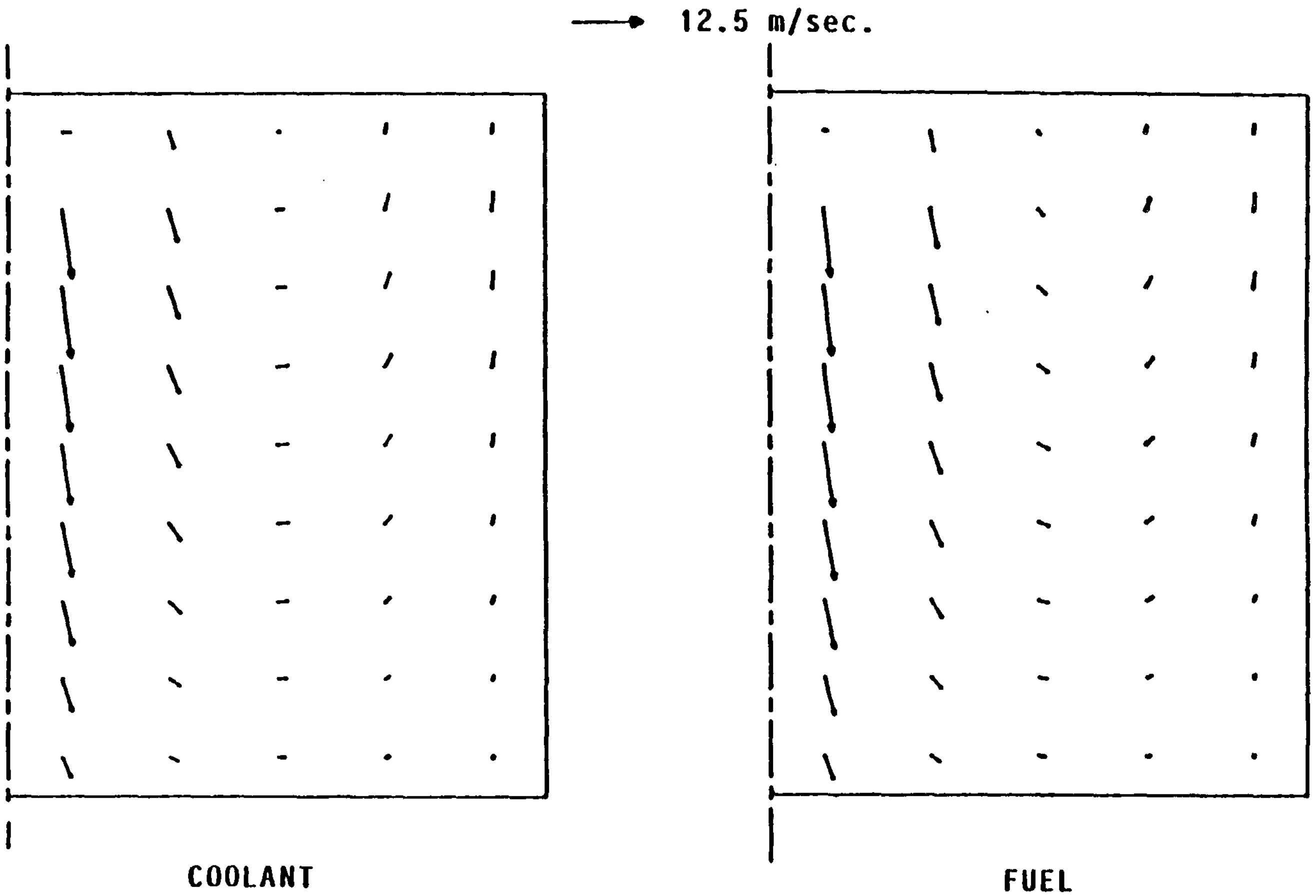

Fig. 4. Velocity vectors of coolant and fuel.

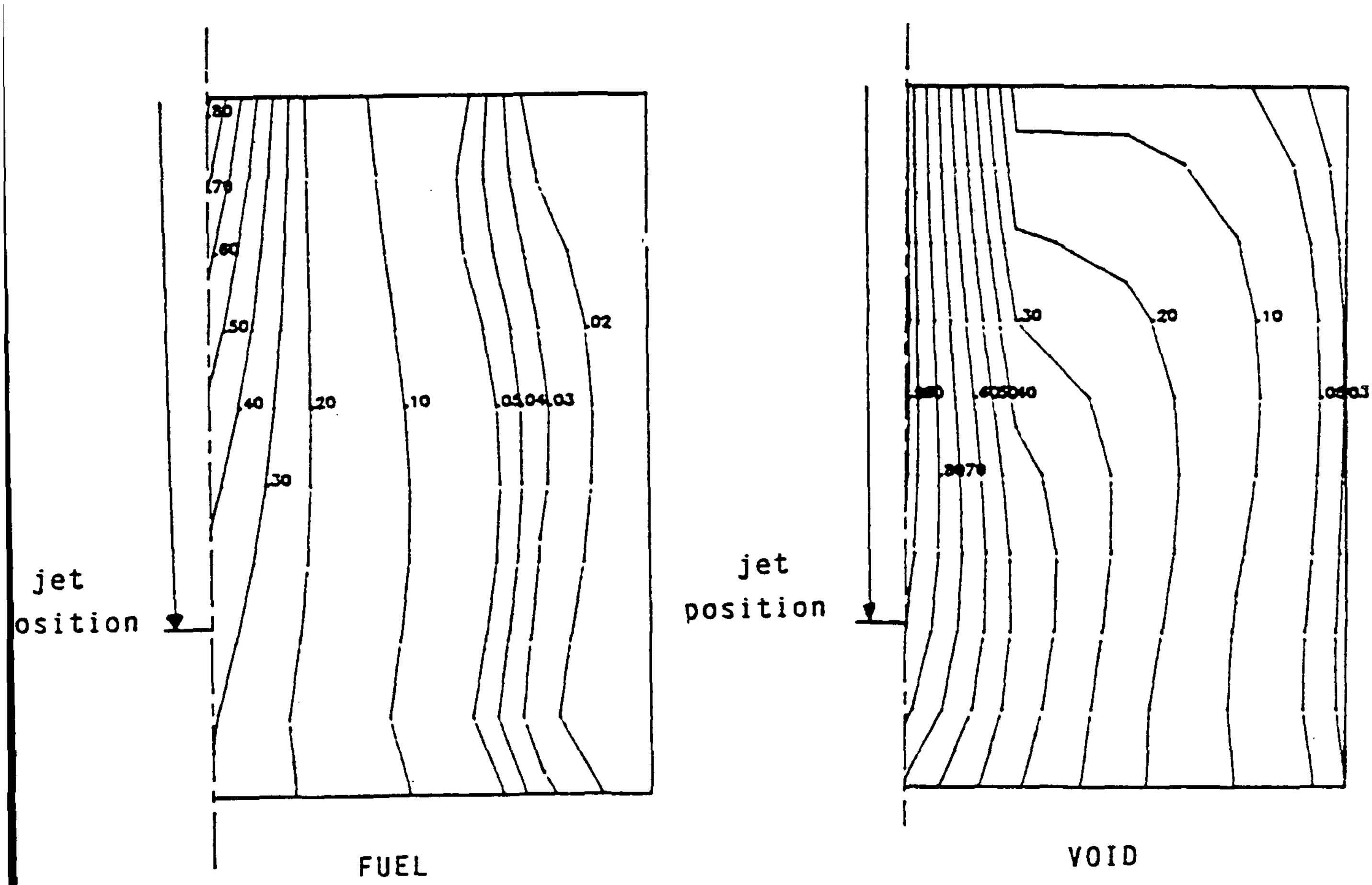

Fig. 5. Fuel and void fractions.

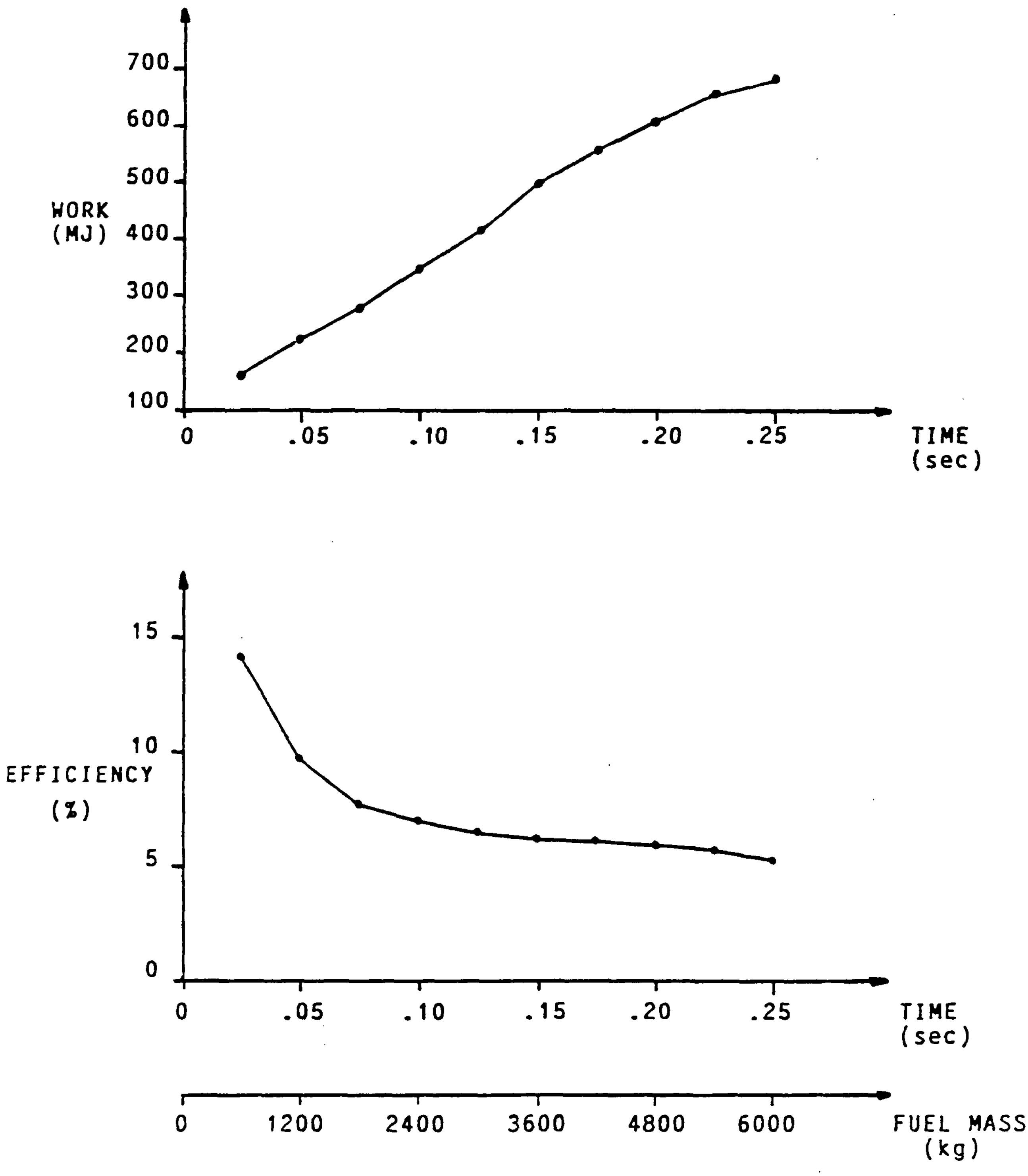
WORK (MJ)
700
600
500
400
300
200
100
0
.05
.10
.15
.20
.25
TIME (sec)
EFFICIENCY (%)
15
10
5
0
0
.05
.10
.15
.20
.25
TIME (sec)
0
1200
2400
3600
4800
6000
FUEL MASS (kg)

Fig. 6

To be presented at the International Nuclear Power Plant Thermal Hydraulics and Operations Topical Meeting in Taipei, Taiwan, ROC, October 22-24, 1984.

Rec'd at meeting
11-27-84

[illegible]

THE APPLICATION OF A USER-FRIENDLY CODE TO NUCLEAR THERMALHYDRAULIC REACTOR SAFETY PROBLEMS [1]

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ABSTRACT

The multiphase, multi-dimensional thermalhydraulics code, PHOENICS, which is the property of CHAM Ltd. of London, has been used for calculations of fuel-coolant mixing in the lower plenum following a meltdown accident in a light water reactor. Because of the simplicity of its command structure and the many built-in options, it was possible to produce significant calculations of local void fractions and velocities of the phases within a period of six weeks by personnel who were completely unfamiliar with the problem or the code. Other thermalhydraulic calculations which have been performed in similar fashion are mentioned briefly.

INTRODUCTION

Both before and after the Three-Mile Island accident there has been a continuing demand for complex computer codes which can be used to predict the course of various hypothetical accidents in either pressurized water reactors or boiling water reactors. In the thermalhydraulic area, the major codes which were developed for large-break (intact-core) accidents are TRAC (Los Alamos National Laboratory), RELAP (Idaho National Engineering Laboratory), and COBRA (Pacific Northwest Laboratory). Since Three-Mile Island, attention has focused on severe accidents, especially with small-break initiators, and a large variety of codes have proliferated. These generally describe one specific phase of the accident and must be tied together in order to produce a prediction of the accident sequence. There are considerable problems in making these connections. In addition, the codes all share the common feature that they are large, complex, and very difficult for a beginner to comprehend and to use. Further, they are highly specialized, and hence are difficult to adapt, except for very experienced practitioners. For the non-expert programmer a small bug can cost weeks of searching through the code to find the difficulty. For the really large codes, such as TRAC and RELAP, it takes nearly a year for an engineer to become sufficiently competent that he can adapt the code to his particular problem and be confident of his results.

More general codes, such as K-FIX (Los Alamos), are also available, but require considerable expertise to adapt to specific problems. Hence, one finds that the applications of codes such as K-FIX are severely limited outside of the originating group.

More recently, however, a versatile user-friendly code, named PHOENICS, shows promise of being adaptable to a wide variety of practical problems by engineers who are not professional programmers. Clearly, this has implications, both for architect engineers and utilities who are required to perform design and licensing calculations, as well as for manpower training in the operations area.

In this paper, we describe first some of the features of the PHOENICS code which make it especially adaptable to thermalhydraulic calculations, and then describe in general detail an example of a complex problem for which useful results were obtained in less than two months by an engineer previously unfamiliar with the code or with the problem.

DESCRIPTION OF PHOENICS

The code PHOENICS, which stands for Parabolic, Hyperbolic or Elliptic Numerical Integration Code Series, is a proprietary code by Concentration, Heat, and Momentum, Ltd. (CHAM) of Wimbledon, London, England. It is a versatile general software package designed for the simulation of fluid flow, heat transfer, mass transfer and chemical reaction processes in engineering equipment and in the natural environment. It has two main components: an efficient general purpose partial differential equation solver called EARTH, and a number of specialized satellite programs. EARTH is a machine-language program, inaccessible to the user, which solves the laws of conservation of mass, momentum, and energy and related quantities in one, two, or three space dimensions and in time. Twenty-five dependent variables are allowed for automatically and others can be added. The 25 variables which are contained in the program include the pressure and pressure correction, velocity components for two phases, enthalpies, volume fractions, concentrations, and turbulence quantities, such as k (kinetic energy) and ϵ (dissipation rate). Cartesian and cylindrical coordinates are built into the system and spherical, conical, and other geometries can be provided. The equations are solved by a finite-domain approach involving integration over many small control volumes, followed by iterative solutions of the resulting nonlinear algebraic equations. The so-called satellites of PHOENICS, by which the user communicates with EARTH and sets the initial and boundary conditions of his particular problem are based on a general use satellite program called GUSSIE, which comprises a separate program in which the user provides problem-definition or information, together with "Ground Station" subroutines which link with EARTH. The conservation equations which are solved are of general form, which include accumulation, diffusion, convection, and volumetric sources of the quantity being transported. The finite-domain equations are obtained by integrating these equations over a finite set of subdomains in a staggered grid, with a fully implicit formulation and an optional donor-acceptor option. The solution proceeds by successive sweeps through one-dimensional slabs in the domain of interest, in connection with an outer iterative loop over the entire domain. A wide variety of processes have been already simulated, as demonstrated by various journal articles,[1,2] as well as demonstration reports. These demonstration reports, which are available from CHAM, involve such topics as mixing at a tee junction, coal combustion, glass melting, small-break loss-of-coolant accidents, explosion containment in a fast breeder reactor, natural convection in an enclosed cavity, flows in pin bundles containing blockages, and self-preserving turbulent free jets using the k-W-g model.

As an example of the user-friendly nature of this code for complex multiphase problems, we describe in the next section results obtained in a reactor safety problem of considerable current interest. This problem consists of the coarse pre-mixing of molten fuel and core material falling through the lower support plate of the reactor core into the water pool in the lower plenum of a light water reactor during a meltdown accident. These results were obtained in six weeks by a person unfamiliar with the background literature or the problem, as well as the PHOENICS code. Additional research is under way to extend these results over a wide variety of parameters and potential accident conditions.

STATEMENT OF THE PROBLEM

There are several types of severe reactor accidents which could threaten the public health and safety. One of these, which is admittedly quite remote, is a large-break accident accompanied by both on-site and off-site loss of power for extended period of time. The consequence is that the core fuel and steel would begin to melt and eventually slump downwards towards the lower support plate. After melting through the support plate the molten mass would fall into the water contained in the lower plenum. One possible consequence is the triggering of a large-scale vapor explosion. A vapor explosion is produced when very hot molten material is coarsely premixed with a vaporizable coolant such as water and is then triggered by the presence of a pressure pulse somewhere in the system. This pressure pulse collapses the vapor layer surrounding the fuel particles, which then proceed to fragment rapidly and mix with the surrounding coolant in time scales of the order of milliseconds. The resulting shock wave can be propagated through the mixture, producing a true physical explosion. It is estimated that if as much as 10 percent of the core were to react efficiently with the water pool a serious threat to both the vessel and the containment might exist. However, the distribution of the fuel within the water pool at the time of the triggering of the explosion is likely to be quite uneven. Furthermore, the rapid generation of steam by means of the molten ceramic particles will tend to levitate both the liquid water and the particles up out of the pool. These factors tend to mitigate the severity of the explosion, to an extent which is not yet fully understood. It is therefore of considerable interest to follow the fluid mechanics and heat transfer of the preliminary mixing of the fuel mass and the water below. A realistic model considers the molten mass to fall through a hole in the lower support plate in the form of a jet which then breaks up when it hits the surface of the water pool. This jet will tend to spread as it falls due to the generation of vapor internally. Eventually, the countercurrent flow of fuel and vapor can cause a turnaround in the particle stream lines so that fuel is actually levitated out of the system. In any case, the water is driven out before, which tends to reduce the conversion ratio of mechanical energy to thermal energy.

For these calculations it was assumed that the flow was two-dimensional and axisymmetric, and that the fuel particles were spheres of equal diameter. It is easily shown that the temperature of the fuel particles remains quite high throughout the period of interest, so that an acceptable assumption is that the fuel surface temperature remains constant. Previous calculations have shown that the slip between the liquid water and the steam is not an important factor. In these calculations the coolant is assumed to flow as a homogeneous mixture locally of steam and water. Details of the equations which are solved are given in the Appendix.

Initially the vessel is filled with water at rest. The fuel jet enters the vessel at the boundary AB (Fig. 1) with a prescribed mass flux of 1000 kg/m^{2}s. BC is a prescribed pressure outflow. CD and DE are wall boundaries, and AE is the axis of symmetry. The flow domain was resolved by a coarse grid of five cells in the y-direction and ten cells in z-direction.

In these calculations, the fuel drops are 0.1 m in diameter falling at 1.0 m/sec. The pressure is 1 MPa. Convergence was achieved to within 0.3% of the total volume.

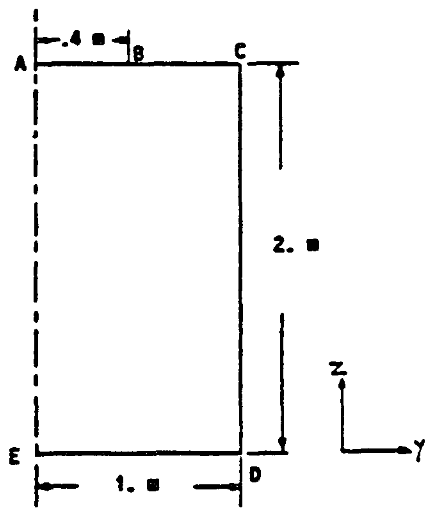


Fig. 1 Vessel geometry

Figures 2a and 3a show the contours of the particle volume fraction at various time stops. Figures 2b and 3b show the contour plots for the void fraction. The void fraction reaches a value of 0.9 near the fuel entrance region but decreases further down the vessel. The contours stretch downward and also radially, indicating fuel disperison of the jet in the radial direction. This can also be seen on Fig. 2a where the contours of the particle volume fraction are shown.

*Fuel contour values are between A=0.001 and Y=0.400

*Void contour values are between A=0.010 and Y=0.900

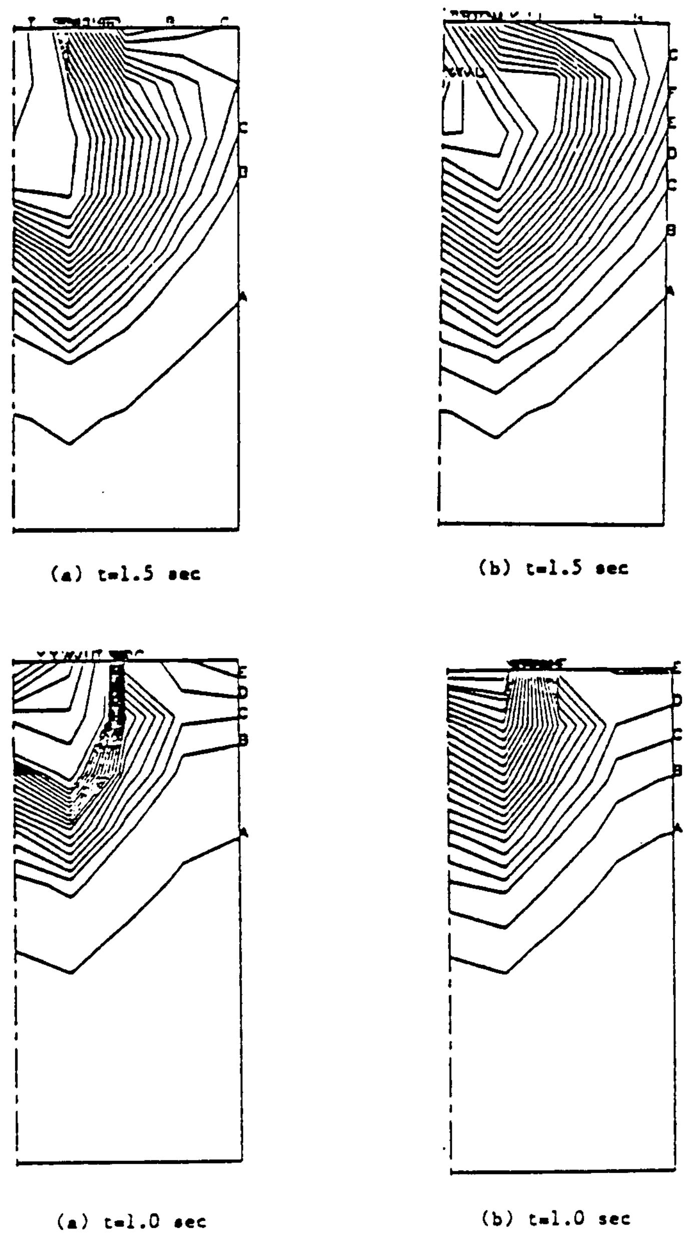
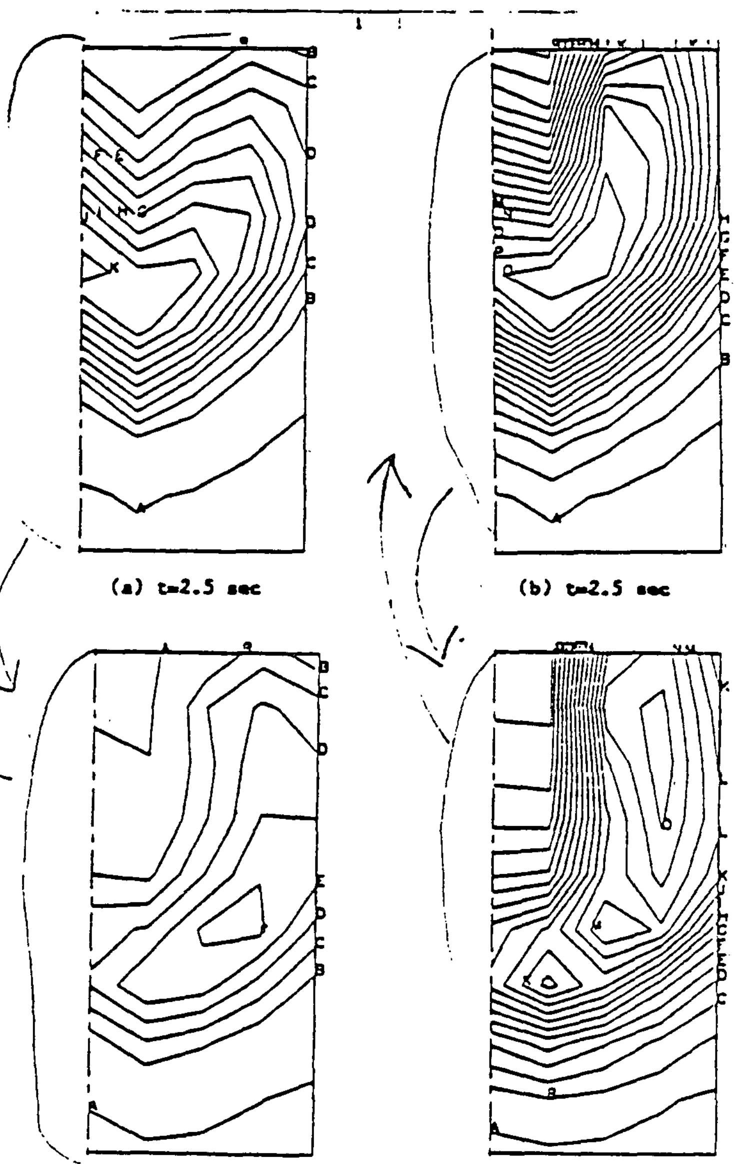


Fig. 2 Contour map of
(a) Fuel volume fraction
(b) Void fraction

As shown in Fig. 3a, the volume fraction of fuel particles remains everywhere small, even after 2.5 sec., with as much as 769 kg total input. The corresponding void fraction is well over 0.5. The explosion efficiency, if one can occur for these initial conditions, is not well-understood, but it is known a propagating one-dimensional detonation at supercritical pressures with a void fraction above 0.5 is not theoretically possible. This does not, of course, rule out lower pressure interactions.



(a) t=2.5 sec (b) t=2.5 sec

(a) t=2.0 sec (b) t=2.0 sec

Fig. 3 Contour map of
(a) Fuel volume fraction
(b) Void fraction

Figures 4a and 4b, and 5a and 5b show the velocity vectors for the homogeneous mixture phase and the fuel particle phase respectively. The introduction of fuel drops into the water causes the water to swell due to the increase in void fraction.

The homogeneous coolant mixture tends to flow out, as shown in Figures 4a and 5a. The fuel drops pick up some velocity in the direction of the homogeneous mixture, but the dominant direction seems to be sideways and downward.

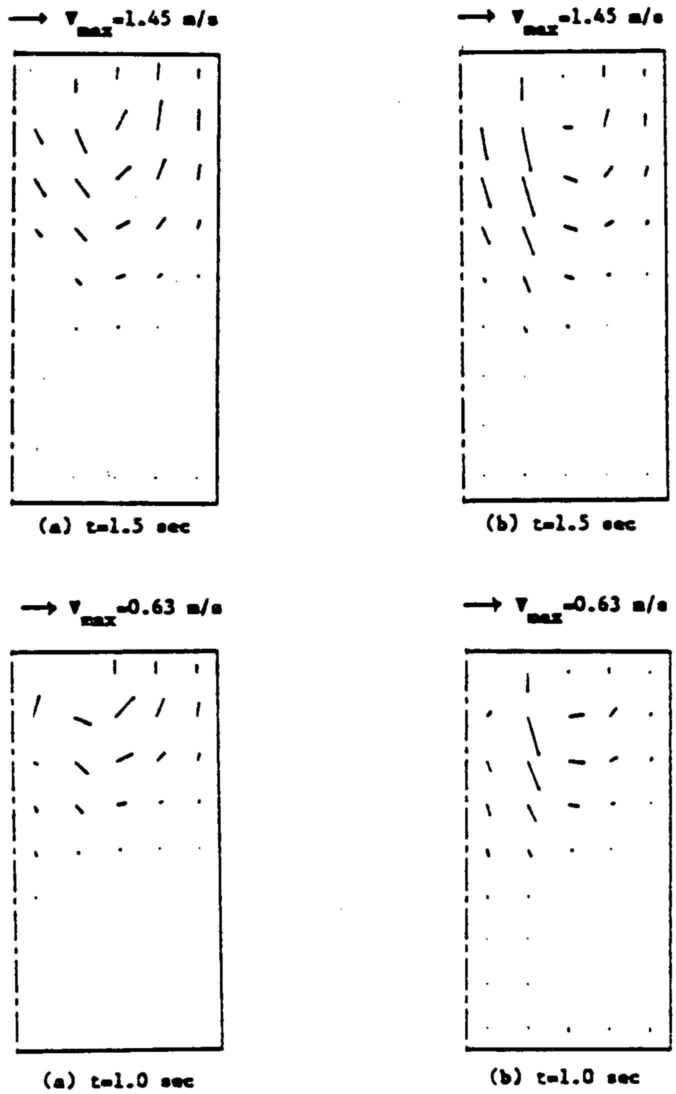


Fig. 4 Velocity vectors
(a) Homogenous fluid phase
(b) Fuel particle phase

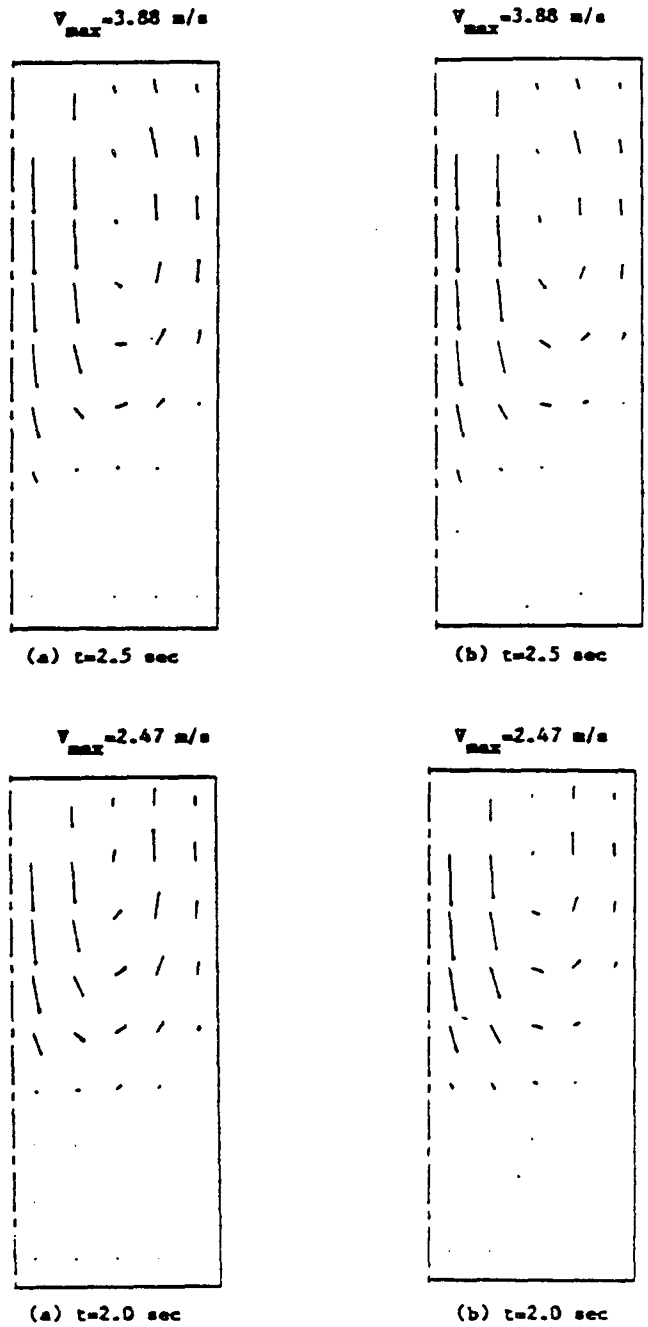


Fig. 5 Velocity vectors
(a) Homogenous fluid phase
(b) Fuel particle phase

Figures 6 and 7 show a comparison between FITS melt penetration results[3,4] and the present calculations. In Fig. 6, two values of the full volume fraction at the penetrating front were considered (0.05 and 0.10) in calculating the height of the melt. Both values seem to lie within the experimental band.

Figure 7 shows a non-dimensional mixture volume versus a non-dimensional time (T^+). The results show qualitatively the spread of the melt-coolant mixture, which can be approximated as a quadratic function of time. The mixture volume was obtained by integrating over the region of fuel volume fraction $\geq$ 0.05.

The non-dimensional time is defined as:

$$T^+ = \frac{v_{in} t}{D_f} \left(\frac{\rho_c}{\rho_f}\right)^{1/2}$$

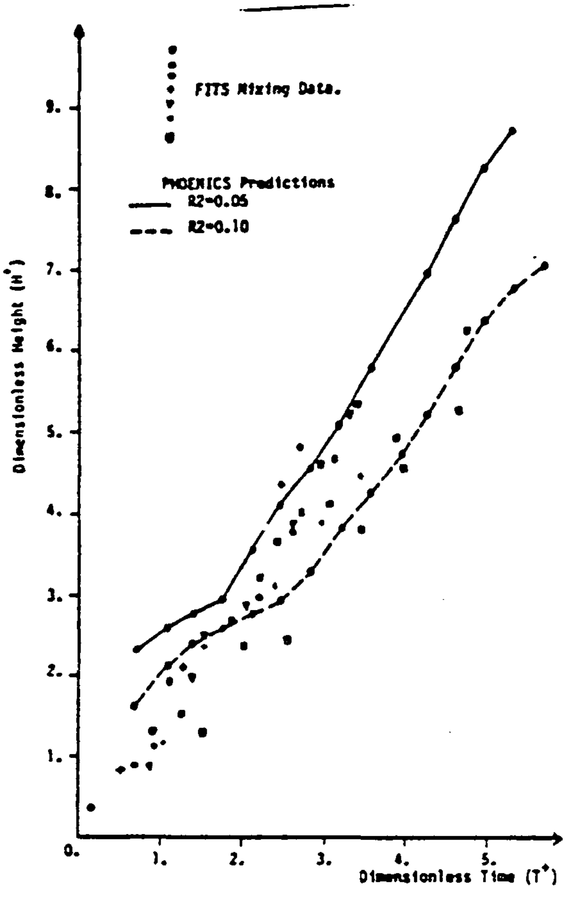


Fig. 6 Height versus time.

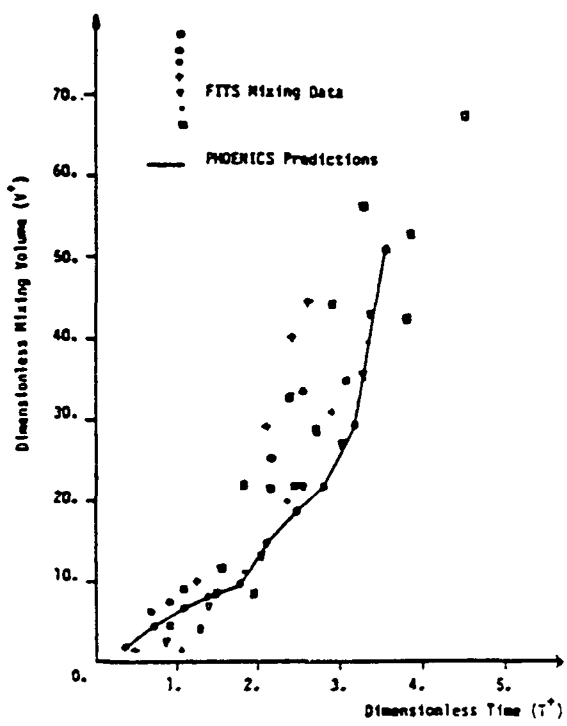


Fig. 7 Mixture volume versus time.

CONCLUDING DISCUSSION

The subsequent calculations have been focused on comparisons with experiments made by Mitchell et al.,[3], and analyzed by Corradini.[4] In these experiments about 20 kg. of molten iron oxide-aluminum thermite mixture were dropped into a pool of water. Excellent agreement was obtained with the rate of penetration of the fuel mass and the volume of the fuel coolant mixture as a function of time. These results were obtained without reference to the data, using only well-known heat transfer and drag coefficient correlations. The ease with which meaningful results could be obtained speaks well for the capability of general purpose codes of this type for manpower training and for applications to complex fluid mechanical problems by engineers and scientists who are not primarily numerical analysts.

ACKNOWLEDGMENT

The royalty-free use of this code by CHAM and its director, Professor D. B. Spalding of Imperial College, London, is gratefully acknowledged.

APPENDIX

The equations solved are the mass, momentum, and energy equations for the fuel and coolant separately, with auxiliary equations for interphase friction and heat transfer. The latter takes into account both the forced-convection film boiling heat transfer and the radiant energy transfer.

The volume fractions, densities, and velocities of each of the two phases obey the following equations which can be written in a general form as:

$$\frac{\partial}{\partial t}(r\rho\phi) + \mathrm{div}(r\rho\vec{u}\phi - r\Gamma_\phi\ \mathrm{grad}\phi) = rS_\phi \quad (1)$$

where

r = volume fraction r_1 for phase 1 (homogeneous coolant mixture) and r_2 for phase 2 (fuel particles),

and

$$r_1 + r_2 = 1 \quad (2)$$

$\vec{u}$ = velocity vector (v_1, w_1 for phase 1 in y- and z-directions, and v_2, w_2 for phase 2 in y- and z-directions, respectively).

Γ_ϕ = exchange coefficient for ϕ.

S_ϕ = source of ϕ per unit phase volume.

ϕ = ϕ-content of material crossing phase boundaries and can stand for:

- = 1 for phase mass conservation.
- = v, w velocity components in y- and z-directions.
- = k, ε phase turbulent quantities (kinetic energy and dissipation rate).
- = h phase enthalpy.

Momentum Conservation Equation

S_ϕ contains contributions of:

$\vec{i}\cdot\mathrm{grad}\,P$ the pressure gradient in the relevant direction.

$\vec{i}\cdot\vec{g}$ gravitational force.

$F_i\ \vec{i}\cdot(\vec{u}_1-\vec{u}_2)$ the interphase friction term where F_i is the friction factor and $\vec{u}$ is the velocity vector of the appropriate phase.

The formulation used for the friction force per unit volume is

$$F = C_f\cdot r_1\cdot r_2\cdot|u_1-u_2| \quad (3)$$

To determine the friction coefficient (C_f), it is noted that the friction force is also given by

$$n_p\cdot C_D\cdot\rho_C\cdot\frac{1}{2}\cdot u_r|u_r|\cdot\frac{\pi}{4}D_p^2 \quad (4)$$

where, n_p is the fuel particle number density, ρ_C is the fluid coolant density, and D_p is the fuel particle diameter.

To evaluate C_D (the drag coefficient), the fuel drop in the film boiling mode is considered to be a bubble with the drop diameter. Ishii and Zuber[5] represented the drag coefficient as

$$C_D = 0.45\left[\frac{1+17.67\,(1-r_2)^{9/7}}{18.67\,(1-r_2)^{1.5}}\right]^2 \quad (5)$$

Therefore, C_f in Eq.(3) can be written as

$$C_f = \frac{3}{4}\cdot C_D\cdot\frac{\rho_C\,|u_r|}{r_1\cdot D_p} \quad (6)$$

where u_r is the relative velocity between the two phases and is given by

$$u_r = \left[(v_1-v_2)^2+(w_1-w_2)^2\right]^{1/2} \qquad (7)$$

Energy Conservation Equation

For the energy equation, S_ϕ contains contribution representing inter-phase heat transfer. The rate of heat transfer from the fuel drop to the surrounding fluid is given by

$$\dot{q}_p = A_p(h_c+h_r)(T_p-T_{sat}) \qquad (8)$$

where A_p is the fuel particle surface area. The convection heat transfer coefficient for forced-convection boiling around a sphere is[6]

$$h_c = 2.98\left[\frac{\rho_g k_g h'_{fg} \cdot u_r}{D_p(T_p-T_{sat})}\right]^{1/2} \qquad (9)$$

where ρ_g is the density of steam, k_g is the gas thermal conductivity, T_p is the temperature of the fuel particles, and T_{sat} is the coolant saturation temperature.

$$h'_{fg} = h_{fg} + 0.68\ C_{pg}(T_p-T_{sat}) \qquad (10)$$

h_{fg} is the latent heat of vaporization, and C_{pg} is the specific heat.

The radiation heat transfer coefficient is

$$h_r = \frac{\sigma F(T_p^4 - T_{sat}^4)}{(T_p-T_{sat})} \qquad (11)$$

where σ is the Stefan-Boltzmann constant.

The radiant interchange factor

$F = F(\varepsilon_p, \varepsilon_\ell\ )$, where ε_p and ε_ℓ are the

particle and liquid emissivities.

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An Unsteady One-Dimensional Two-Fluid Model for Fuel-Coolant Mixing in an LWR Meltdown Accident

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ABSTRACT

The multiphase code PHOENICS is used to predict the mixing of molten core material with the lower plenum water pool in a severe light-water reactor accident. One-dimensional behavior is assumed, corresponding to catastrophic failure of the lower support plate, possibly due to a preliminary steam explosion. For the dangerous range of fuel drop sizes (10-100 mm) it is found that there is rapid level swell, with most of the drops being levitated upwards in a region of high steam volume fraction. This condition is thought to be unfavorable for an efficient explosion.

NOMENCLATURE

A	cross-sectional area
C_D	drag coefficient
C_p	specific heat
d	diameter
F_{ij}	interphase friction factor
g	gravitational acceleration
h	enthalpy
h_c, h_r	convective and radiational heat transfer coefficients, respectively
h_{fg}	latent heat of vaporization
k	thermal conductivity
$\dot{m}$	mass flow rate
$\dot{m}_{ij}$	interphase mass flow rate
P	pressure
$\dot{Q}_{ij}$	volumetric heat source
$\dot{q}_j$	volumetric heat source in the jth cell
S	source term
T	temperature
t	time
$\vec{v}$	velocity vector
z	distance
Δz_j	height of the jth cell

Greek Letters

α	volume fraction
Γ	exchange coefficient
ϵ	emissivity
	viscosity
ρ	density
σ	Stefan-Boltzmann constant
ϕ	any dependent variables

Subscripts

0	initial
1	steam-water mixture
2	fuel drops
g	gas
ℓ	liquid
sat	saturated
∞	terminal

INTRODUCTION

In a LWR core melt accident, mixing of molten core material with an underlying water pool, either in-vessel or ex-vessel, or both, will probably occur. The fluid mechanics and energetics of this process is obviously of interest, both from the point of view of the possibility of a large-scale efficient steam explosion, and also the rate and amount of production of hydrogen. Several models have been proposed. Henry and Fauske [1] postulated that the fuel drops subdivided until the steam volumetric flux upwards exceeded the Kutateladze flooding limit [2], or equivalently the Zuber film boiling vapor flux [3]. The implication was that the dangerous range of fuel drop diameters (say 10 to 100 mm), the void fraction in the surrounding coolant mixture would be too large to permit an efficient explosion. Fuel particles much smaller than this range lose their heat to the coolant too rapidly during the premixing stage, while particles much larger than this range require too much viscous energy dissipation for interaction on explosive time scales (Cho, Fauske, and Grolmes [4]). The mechanism of subdivision in this one-dimensional model was not specified. Corradini [5] and Corradini and Moses [6] proposed a two-dimensional model, which allowed radial expansion of the falling fuel particle cloud, and hence was also time-dependent, in contrast to the Henry-Fauske model. However, the fuel particle diameter, the particle volume fraction, and the local void fraction were all taken to be empirical functions of dimensionless time obtained from the data. Local relative velocities were not calculated, as that fluidization criteria could not be checked. Bankoff and Han [7] performed a quasi-steady one-dimensional calculation of the mixing, in which the fuel drop size is held constant with time. This might correspond to a catastrophic failure of the lower support plate, possibly due to the combined effects of melting from above and a small surface explosion below. It was further assumed that the fuel particle concentration, viewed from the falling front, changes only slowly with time. The validity of this assumption needs further investigation.

In the present work the one-dimensional, unsteady mixing of fuel drops and coolant is numerically studied, using the PHOENICS code, which is the property of CHAM, Ltd. of London. The growth of the steam volume fraction within the water and the penetration and mixing of fuel drops are estimated, using the mass, momentum, and energy conservation equations for fuel and coolant in a two-fluid formulation with generally-accepted constitutive relations.

STATEMENT OF THE MODEL

As molten fuel drops fall into the water pool in the lower plenum at their terminal velocity and the mixing proceeds, the fuel drops would be in a film boiling mode. A cylindrical coordinate system was introduced, with the origin at the bottom of the pool, and the z-direction upward. The geometry considered is shown in Fig. 1.

The following assumptions were made: (1) Fluid 1 is a homogeneous mixture of steam and water, and Fluid 2 consists of the fuel drops. (2) The water is initially at the saturation temperature, and the temperature of the fuel drops remains constant. (3) the fuel drops are spherical and of equal diameter. (4) The ambient pressure remains constant throughout the mixing process. Only the effect of hydrostatic pressure was taken into consideration. Hence, the density of the homogeneous mixture depends only on its enthalpy:

$$\rho_1 = \rho_\ell (a + bh_1)^{-1}; \qquad a = 1 - \rho_\ell h_{\ell,sat}/\rho_g h_{fg} \;;$$
$$b = \rho_\ell/\rho_g h_{fg} \tag{1}$$

The governing equations for the ith phase are in general form given by

$$\frac{\partial}{\partial t}(\alpha_i \rho_i \phi_i) + \nabla\cdot(\alpha_i \rho_i \vec{v}_i \phi_i - \alpha_i \Gamma_{\phi_i} \nabla\phi_i)$$
$$= \dot{m}_{ij}\phi_i + \alpha_i S_{\phi_i} \quad ; \quad i = 1,2 \tag{2}$$

where ϕ_i stands for any of the dependent variables, α_i is the volume fraction; ρ_i is the density; $\vec{v}_i$ is the velocity vector, Γ_{ϕ_i} is the exchange coefficient and S_{ϕ_i} is the source term for ϕ_i.

The continuity equation for the ith phase is

$$\frac{\partial}{\partial t}(\alpha_i \rho_i) + \nabla\cdot(\alpha_i \rho_i \vec{v}_i) = 0 \; ; \quad i = 1,2 \tag{3}$$

where $\alpha_1 + \alpha_2 = 1$.

The momentum equation for the ith phase is

$$\frac{\partial}{\partial t}(\alpha_i \rho_i \vec{v}_i) + \nabla\cdot(\alpha_i \rho_i \vec{v}_i \vec{v}_i - \alpha_i \mu_i \nabla \vec{v}_i)$$
$$= -\alpha_i \nabla P + \alpha_i \rho_i \vec{g} + F_{ij}\,(\vec{v}_j - \vec{v}_i) \; ; \; i = 1,2 \tag{4}$$

where F_{ij} is the interphase friction factor and given by

$$F_{ij} = 3/4\; C_D \frac{\alpha_i \rho_i}{d_2} |\vec{v}_i - \vec{v}_j| \tag{5}$$

To evaluate the drag coefficient, the fuel drop in the film boiling mode is considered to be a bubble with the drop diameter. One thus obtains [8],

$$C_D = 0.45 \left[\frac{1 + 17.67\,(1 - \alpha_2)^{9/7}}{18.67\,(1 - \alpha_2)^{3/2}}\right]^2 \tag{6}$$

Since the interphase heat transfer is dominant over viscous stress, interphase friction, gravity and other source terms in the energy equation, the energy equation for the ith phase becomes

$$\frac{\partial}{\partial t}(\alpha_i \rho_i h_i) + \nabla\cdot(\alpha_i \rho_i \vec{v}_i h_i - \alpha_i \frac{k_i}{C_{p_i}} \nabla h_i) = \dot{Q}_{ij} \tag{7}$$

where the interphase heat transfer, $\dot{Q}_{ij}$, is given by

$$\dot{Q}_{ij} = 6\,\alpha_2 (h_c + h_r)\,\frac{T_2 - T_{1,sat}}{d_2} \tag{8}$$

The convective heat transfer coefficient for forced-convection film boiling around a sphere is taken from Witte [9] as

$$h_c = 2.98 \left[\frac{\rho_g k_g h'_{fg} v}{d_2 (T_2 - T_{1,sat})}\right]^{1/2} \tag{9}$$

where

$$h'_{fg} = h_{fg} + 0.68\, C_{p_g} (T_2 - T_{1,sat}) \tag{10}$$

The radiation heat transfer coefficient is defined as

$$h_r = \frac{\sigma\, \epsilon_2\, (T_2^4 - T_{1,sat}^4)}{(T_2 - T_{1,sat})} \tag{11}$$

RESULTS

The initial depth of the water pool was assumed to be 2 m and the diameter of the pool 3 m. The water pool was subdivided into ten uniform cells. The time interval was 0.05 sec. The pool swell phenomenon due to rapid vaporization of water and fuel inflow was also taken into consideration. If no escape of fluids from the mixing region is assumed, the swell velocity of the pool can be estimated as

$$\frac{dH}{dt} = \frac{\dot{m}_2}{A\rho_2} + \frac{\sum_{j=1}^{N} \dot{q}_j \Delta z_j}{\rho_g h_{fg}} \tag{12}$$

where H is the height of the pool, A is the cross-sectional area of the pool, $\dot{q}_j$ is the volumetric heat source in the jth cell, and Δz_j is the height of the jth cell.

Figures 2 and 3 show penetration of fuel drops and growth of void fraction within water, respectively, when a constant fuel inflow was assumed. It is seen that, due to the rapid increase in void fraction, most of the fuel drops are present in the region where the void fraction is greater than 60% in less than 0.5 sec. Furthermore, the penetration of fuel drops is progressing very slowly, and the level swell places the high void fraction region well above the original pool level, so that the fuel and coolant ght be expected to disperse up the downcomer, and to interact further with the core.

When the increase in mass inflow due to the pool swell was taken into consideration which is given by

$$\dot{m}_2 = \dot{m}_{2,0}\left(\frac{v_{2,\infty} - \frac{dH}{dt}}{v_{2,\infty}}\right) \qquad (13)$$

where $\dot{m}_2$ is the initial rate of fuel inflow and v_2 is the terminal velocity of fuel drops, the pool swells more rapidly due to the increased mass inflow of the fuel drops. The overall behavior of fuel drops is, however, very similar, whether a constant or a variable fuel inflow is considered (Figs. 4 and 5).

Figs. 6 and 7 show the effects of ambient pressure on the void fraction within the coolant and the volume fraction of fuel drops, respectively. The homogeneous mixture of steam and water has a lower velocity at 1.0 MPa than at 0.48 MPa, as expected, and the void fraction of water near the top surface is about 80%. Most of the fuel drops are, however, still present in the region where $\alpha > 0.6$ (~70% of drops at 1.0 MPa, and ~85% of drops at 0.48 MPa). The effect of fuel drop size is shown in Figs. 8 to 11. The void fraction within the water increases rapidly as the diameter of the fuel drops decreases, while the curve of volume fraction of fuel drops, as a function of distance at t = 0.5 sec., varies little when the drop size is larger than 6 cm.

However, a different behavior of the fuel drops was observed when the fuel inflow was cut off after the amount of fuel present in the mixing region reached a certain limit. Figure 12 shows the penetration of fuel drops and the growth of void fraction in the water pool for 2250 kg of fuel. It is seen that most of the fuel drops are present in the middle of the mixing zone instead of near the top surface after 1.6 sec. Although accumulation of fuel drops at the bottom of the pool has not been achieved yet, there is a strong possibility of formation of a stratified bottom layer if a longer mixing time is considered. Figure 13 shows a similar result for fuel drops of 10 cm. diameter. The maximum point in the total volume fraction of fuel drops moves downward with time. When 4545 kg of fuel drops were allowed to be mixed with water, fuel drops begin to accumulate at the bottom of the pool after 3 sec (Fig. 14). Figure 15 clearly shows the formation of a bottom layer at the bottom of the pool for 6921 kg of fuel after 5 sec. However, it should be noted that even though fuel drops can penetrate into the bottom of the pool, the void fraction within the water is still above 50% in most of the mixing zone.

When these results are compared to the previous quasi-steady calculations [7], it is found that the void fraction within water grows exponentially in both cases. However, the volume fraction of fuel drops at the penetrating front is much less (< 10%) than assumed in the quasi-steady calculation (50%), so that a direct comparison is difficult.

CONCLUDING DISCUSSION

We show elsewhere [10] in a two-dimensional transient calculation that a falling jet of fuel particles spreads out, and eventually turns partly around due to the upflowing steam. Hence, the levitating effect of the steam on the water in the neighborhood of the fuel particles, as well as the fuel particles themselves, is considerably reduced, but nevertheless very significant. Elsewhere we also show [11] that for large initial void fractions the frictional energy dissipation between the front of the explosion wave and the Chapman-Jouget surface dominates the process, and hence considerably reduces the damage potential.

ACKNOWLEDGMENT

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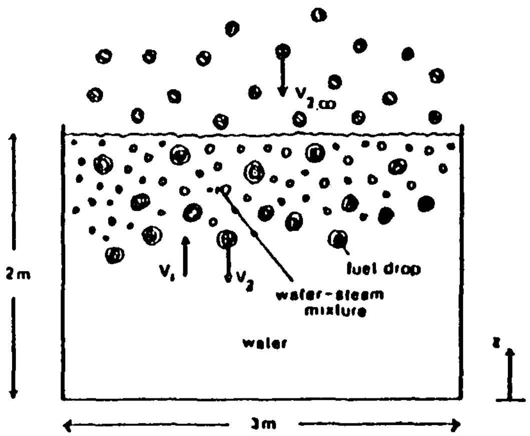

Figure 1. Description of the system.

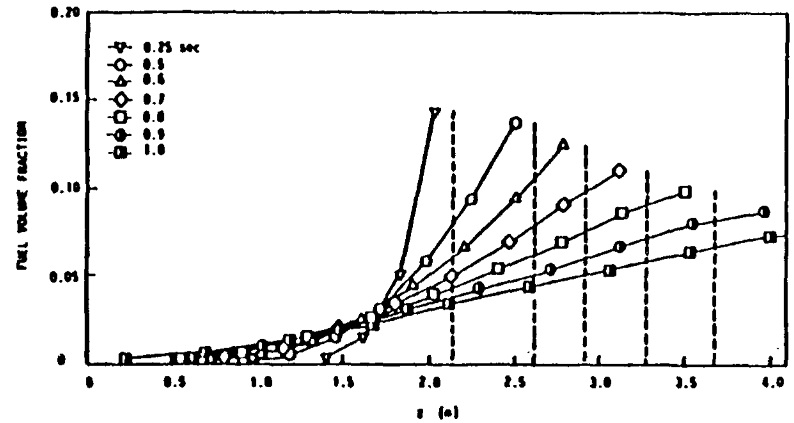

Figure 2. Fuel volume fraction in the mixing zone (constant fuel mass flow rate): P = 1.0 MPa, $\dot{m}_2$ = 9000 kg/s, d_2 = 5 cm.

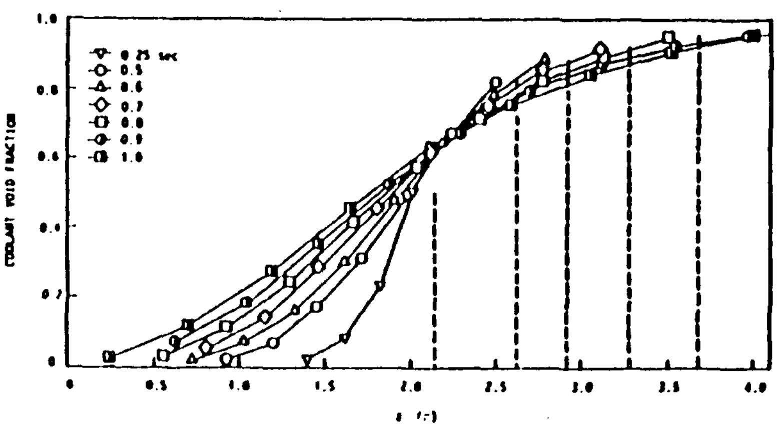

Figure 3. Coolant void fraction in the mixing zone (constant fuel mass flow rate): P = 1.0 MPa, $\dot{m}_2$ = 9000 kg/s, d_2 = 5 cm.

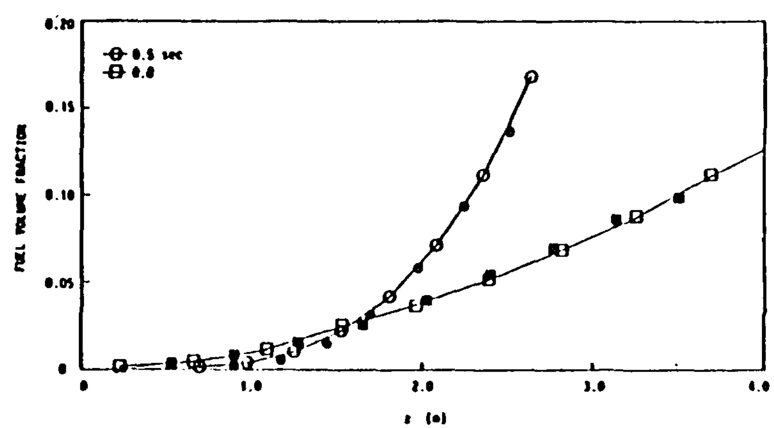

Figure 4. Fuel volume fraction in the mixing zone: P = 1.0 MPa, $\dot{m}_{2,0}$ = 9000 kg/s, d_2 = 5 cm.
(■● = for constant fuel mass flow rate).

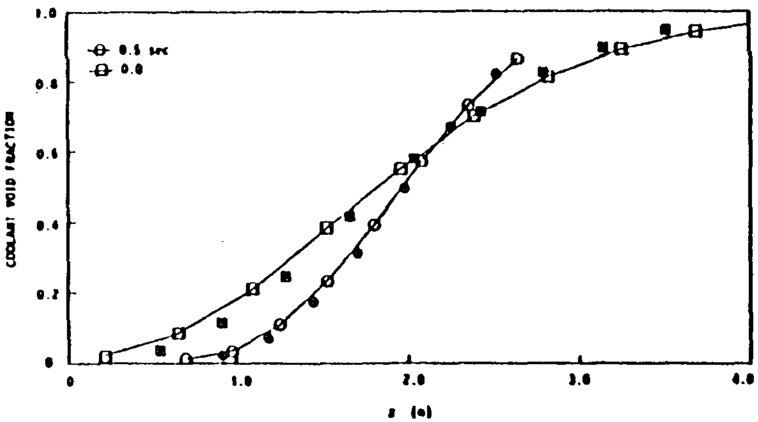

Figure 5. Coolant void fraction in the mixing zone: P = 1.0 MPa, $\dot{m}_{2,0}$ = 9000 kg/s, d_2 = 5 cm (■ ● = for constant fuel mass flow rate).

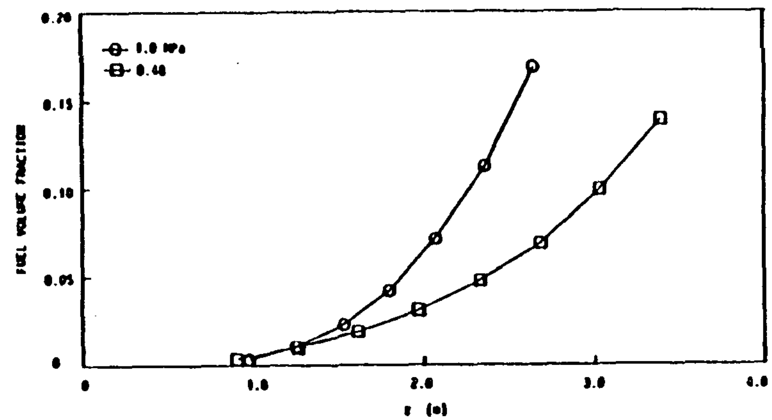

Figure 6. Fuel volume fraction in the mixing zone as a function of ambient pressure at t = 0.5 sec: $\dot{m}_{2,0}$ = 9000 kg/s, d_2 = cm.

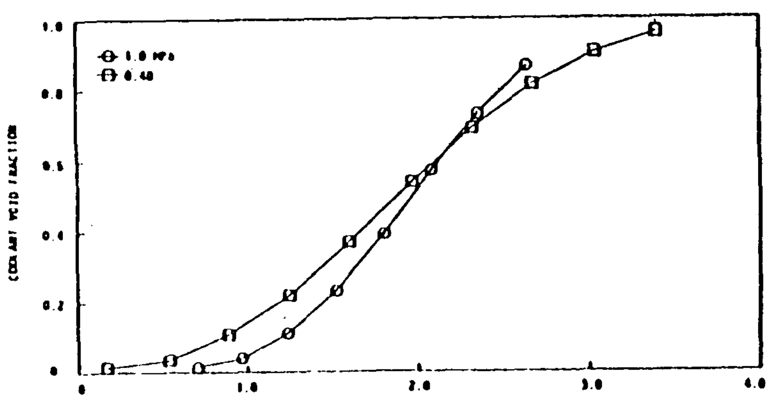

Figure 7. Coolant void fraction in the mixing zone as a function of ambient pressure at t = 0.5 sec: $\dot{m}_{2,0}$ = 9000 kg/s, d_2 = 5 cm.

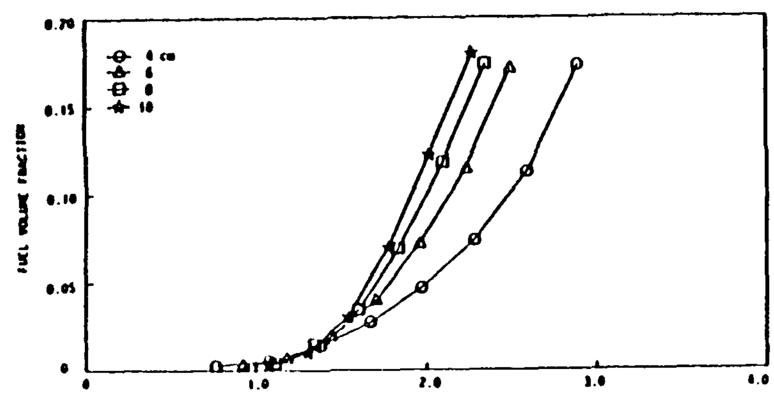

Figure 8. Fuel volume fraction in the mixing zone as a function of d_2 at t = 0.5 sec: P = 1.0 MPa, $\dot{m}_{2,0}$ = 9000 kg/s.

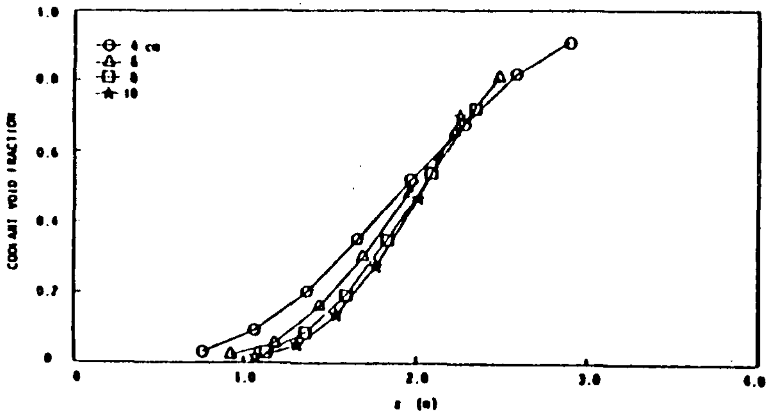

Figure 9. Coolant void fraction in the mixing zone as a function of d_2 at $t = 0.5$ sec: $P = 1.0$ MPa, $\dot{m}_{2,0} = 9000$ kg/s.

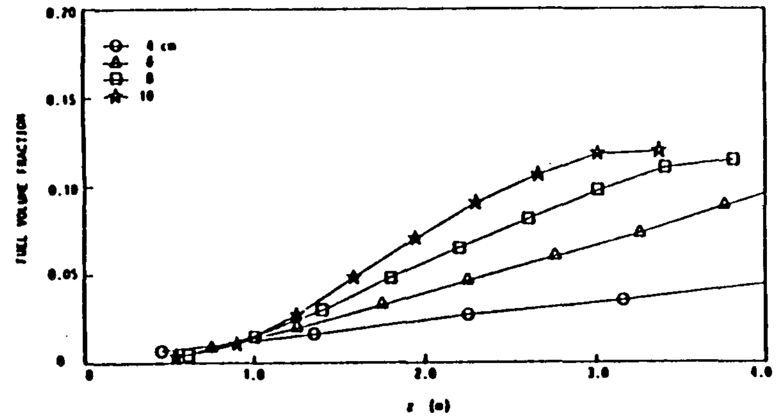

Figure 10. Fuel volume fraction in the mixing zone as a function of d_2 at $t = 1.0$ sec: $P = 1.0$ MPa, $\dot{m}_{2,0} = 9000$ kg/s.

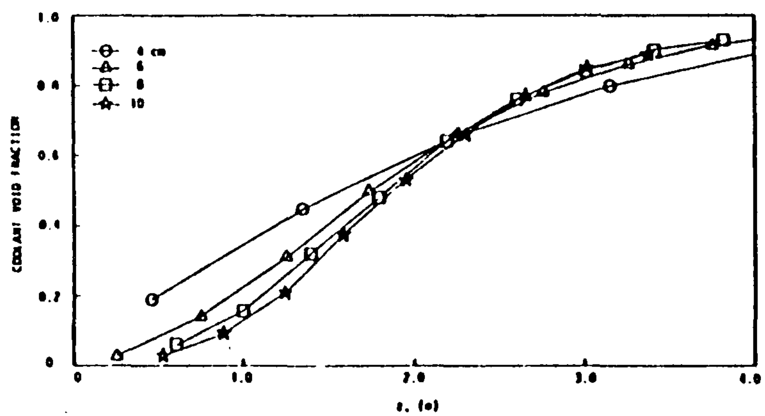

Figure 11. Coolant void fraction in the mixing zone as a function of d_2 at $t = 1.0$ sec: $P = 1.0$ MPa, $\dot{m}_{2,0} = 9000$ kg/s.

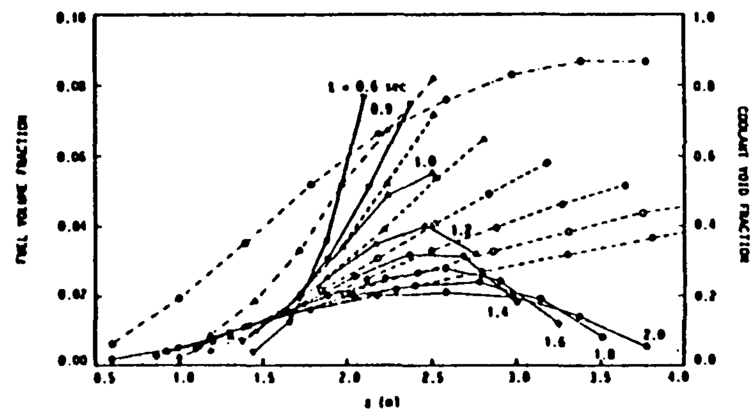

Figure 12. Fuel volume fraction (——) and coolant void fraction (-·-) in the mixing zone: $P = 1.0$ MPa, $\dot{m}_{2,0} = 2250$ kg/s, $m_2 = 2250$ kg, $d_2 = 5$ cm (---- = fuel volume fraction for unlimited fuel flow).

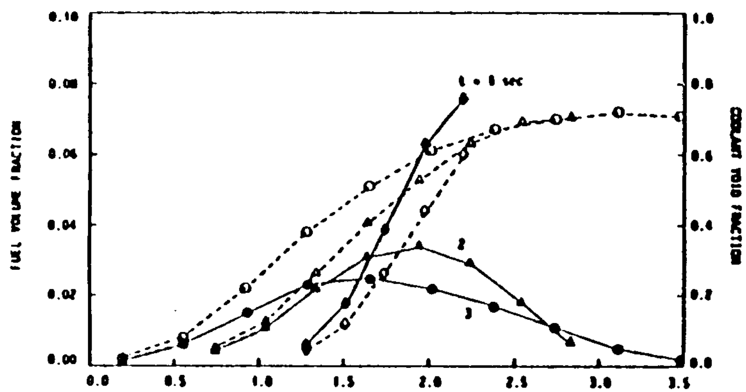

Figure 13. Fuel volume fraction (——) and coolant void fraction (---) in the mixing zone: P = 1.0 MPa, $\dot{m}_{2,0}$ = 2250 kg/s, m_2 = 2250 kg, d_2 = 10 cm.

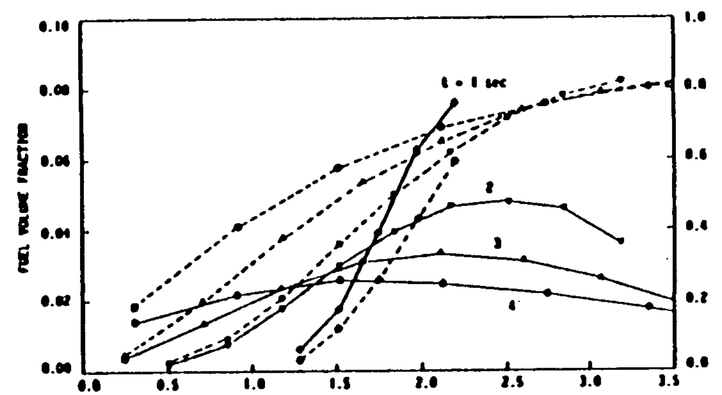

Figure 14. Fuel volume fraction (——) and coolant void fraction (---) in the mixing zone: P = 1.0 MPa, $\dot{m}_{2,0}$ = 2250 kg/s, m_2 = 4545 kg, d_2 = 10 cm.

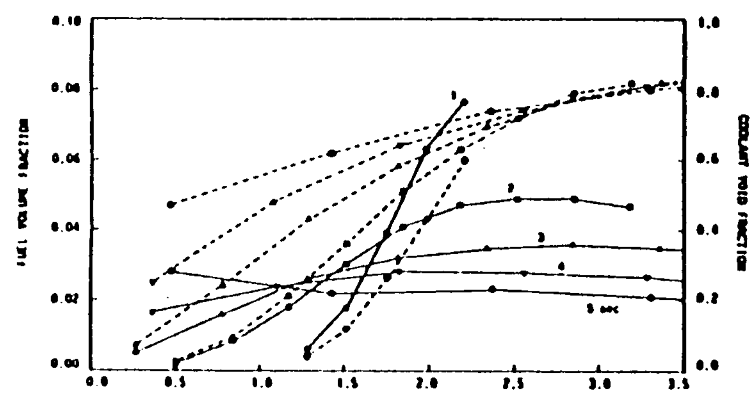

Figure 15. Fuel volume fraction (——) and coolant void fraction (---) in the mixing zone: P = 1.0 MPa, $\dot{m}_{2,0}$ = 2250 kg/s, m_2 = 6971 kg, d_2 = 10 cm.

SOME COMMENTS ON THE PROBABILITY OF CONTAINMENT FAILURE FROM STEAM EXPLOSIONS

W. R. Bohl

T. A. Butler

SUMMARY

This report addresses questions one and two asked by Denwood Ross in his letter of August 24, 1984.

These questions are as follows:

1. What, in your judgment, is the best estimate value for the probability of containment failure arising from a steam explosion, given a core melt , that we should be using in the severe accident program. As part of your response please provide the bases (experimental and/or analytical) and the approach used in reaching your conclusions.

2. Please provide written comments on the Sandia study described in NUREG/CR-3369 and any additional inputs and analyses which you feel are relevant to this study. As pointed out above the SNL report is based on assumed uniform distributions and some additional calculations, assuming other distributions, may be required.

A summary of our response is as follows:

1. For a Zion-type PWR the conditional probability of direct containment failure by a missile resulting from a steam explosion, given core melt, is estimated to range from effectively zero to about 0.1. We believe the upper value should not be unity because of the difficulty of obtaining a truly big steam explosion with melt and water initially separated in a meltdown sequence and the unlikelihood of even a big steam explosion to transfer enough momentum to generate a missile sufficiently energetic to fail the containment. Results expected from the current Los Alamos steam explosion program may allow this

upper limit to be reduced by one and possibly two orders of magnitude. A value of $10^{-3.5}$ or 3×10^{-4} is our guess at a mean best-estimate value. This best-estimate could increase or decrease depending on the results of both ongoing and future investigations. The methodology used in making these estimates was that formulated by T. G. Theofanous and C. R. Bell in assessing CRBR CDAs. We have no opinion on BWR core melt accidents because of lack of time, available manpower, and funding.

2. We believe that the wrong interpretation has been made of the results from the SNL Monte Carlo study. The study mainly provides a convincing plausibility argument that if you accept the authors' assumptions regarding potential mixing of melt and water, transmittal of energy to the head, and bolt failure leading to head missile production, insufficient evidence exists to construct an objective probability density distribution for the consequences of a steam explosion at this time. Large amounts of subjective engineering judgment must still be used for any estimate. The results of the calculations should not be interpreted as pointing towards a significant likelihood that the probability of containment failure resulting from a steam explosion should be unity, only that this upper limit possibility must be considered with the assumptions made. It is our opinion that some of these assumptions are excessively conservative. However, the NUREG/CR-3369 report, despite shortcomings in detail, correctly identifies important uncertainties associated with steam explosion assessments. The confidence level required for making decisions on degraded core issues is ambiguous. If increased confidence is required of our guesses, additional research must be performed.

DETAILS ON QUESTION 1

The method used to address the conditional probability of containment failure by a missile from a steam explosion (the alpha failure mode) is an expansion of the scheme formulated by the CRBR study[1] done for the USNRC on core disruptive accidents. A generic progression diagram is devised, and each branch is assigned a probability as defined in Table I. The scheme to assign the probabilities is then as follows:

(a) Assume that an integral accident analysis computer program exists with models that describe the correct meltdown phenomena to the precision expected from our current uncertainties.

(b) Do gedanken (or thought) computations on this program varying the input parameters.

(c) Based on the results of these computations, assign probability ranges from the definitions in Table I.

A preliminary accident progression diagram focused on the alpha mode of containment failure is given in Fig. 1. This diagram assumes a ZION-type PWR unless otherwise indicated. The potential use of the Indian Point-2 reactor as a specific model should have little impact on this diagram, although time did

TABLE I

DEFINITION OF PROBABILITY SPLIT LEVELS

1/10 Behavior within known trends but obtainable only at the edge-of-spectrum parameter values.

1/100 Behavior cannot be positively excluded but outside the spectrum of reason.

1/1000 Physically unreasonable behavior violating well-known reality and its occurrence can be argued against positively.

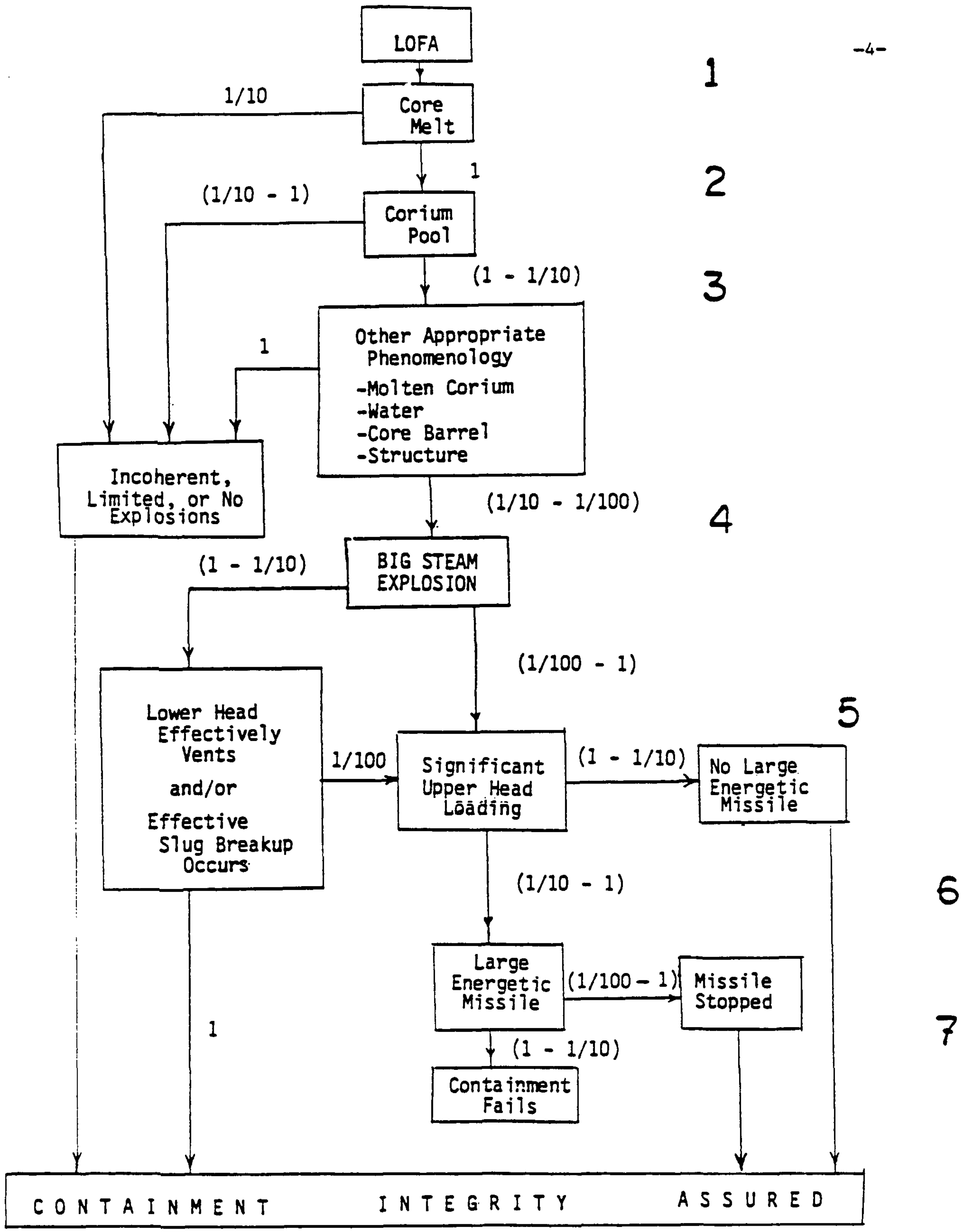

Fig. 1. Accident progression diagram focused on the alpha mode of direct containment failure by a steam explosion.

not exist to even consider modifications. The effects of differing accident sequences, for example, high pressure versus low pressure, are discussed where appropriate. No consideration was given to BWRs as a consequence of lack of time, available manpower, and funding. The branch points are annotated on the diagram for reference. The reasons for the indicated choices are as follows:

1. As suggested by the Oct. 2 letter of Dr. Ginsberg, a station blackout or pipe break accident is postulated. In this document such an accident is called a loss-of-coolant accident, or LOFA, and is assumed to lead to core melt. No credit is taken for operator intervention in stopping the accident progression.

2. A rubble bed leading to pool formation seems likely. This was the expected behavior in WASH-1400. No prototypic experimental evidence apparently exists that justifies use of alternative assumptions. Eventual pool formation is consistent with the behavior observed in the Three Mile Island accident, and the results reported for PBF tests SFD-ST, SFD 1-1, and SFD 1-3. From the Cambridge meeting proceedings (1983), the German best-estimate core meltdown code, KESS-2 with MELSIM-3, apparently slumps fuel rods in each zone once a slumping temperature is reached, assumes blockage formation in disrupted core regions, and then allows coherent downward motion into the lower plenum once failure of the core supporting structure occurs. IDCOR apparently assumes intact geometry and the consequences of conduction limited freezing to perform calculations with the MAAP code indicating incoherent fuel meltout. It is not possible to rule out this possibility. It may be reachable with edge-of-spectrum assumptions. However, our current opinion is that IDCOR's assumption, that as fuel rods melt they drop as blobs into the bottom of the reactor vessel, is based more on wishful thinking than scientific evidence on corium behavior. Pool formation is also consistent with LMFBR integral LOF tests with much shorter fuel pins at much higher power levels, although steel does not wet uranium dioxide while molten zirconium not only wets the fuel, but dissolves it. Access to studies done with ANCHAR (ANL/NSAC), CORMLT (SAI/EPRI), and MELPROG (SNL/NRC) should clarify the core meltdown picture in the future. At present, our gedanken program needs quite improbable assumptions, at least edge-of-spectrum, to <u>not</u> form a pool.

3. Several other characteristics in the meltdown sequence must be present besides pool formation for a dangerous steam explosion to be likely. In view of the uncertainties, these prerequisites can only be discussed qualitatively. They include:

(a) Molten corium in the pool. At commonly quoted corium temperatures, e.g., 2533 K in the CLWG standard problem #1, much of the uranium dioxide may still be solid. One reason speculated for weak or nonexplosive behavior of some FCI tests with corium simulants has been the presence of solid material. Theoretically a steam explosion involves rapid fragmentation in a liquid-liquid system, not a liquid-solid environment. A significant quantity of corium above the liquidus temperature appears essential.

(b) Sufficient water in the lower plenum. A small preliminary explosion must not blow the water away if the necessary condition is to be met. Some water must vaporize from downward heat transfer, but IDCOR's argument of extensive downward thermal radiation seems implausible if a solid crust is supporting a molten pool.

(c) Core barrel or molten corium openings to the downcomer must not lead to incoherent contact. Water would be expelled and any explosion would be small. Core barrel meltthrough is a possibility with the large thermal radiation heat transfer potential from molten corium. One consequence could be collapse of the core as shown in Fig. 2 from the SNL ZIP study, although this ignores the core secondary support system. A second, more likely, possibility is shown in Fig. 3 from NUREG/CR-3369. Escalation to a big steam explosion becomes far more difficult in these situations.

(d) The failure in structure holding up the pool must be sufficiently coherent to permit the initiation of large scale liquid-liquid contact. The explosion from a small pour is inconsequential to the alpha-mode of containment failure unless it can cause more coherent contact.

At present, we must guess at the probability of these conditions being met. It is our understanding that current MELPROG results are tending towards a large "semi-molten" pool with temperatures below the uranium dioxide melting

FROM SNL ZIP STUDY

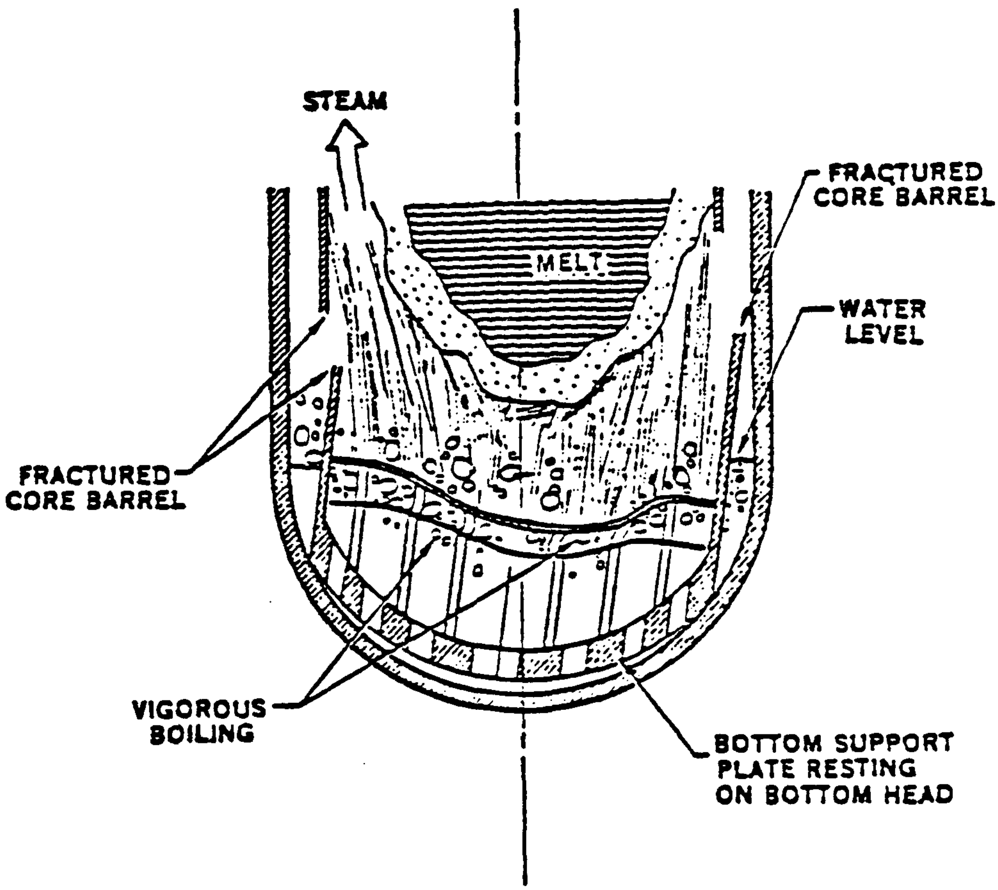

Fig. 2. Visualization of the state resulting from failure of the core barrel prior to penetration of a coherent molten mass through the below-core structure.

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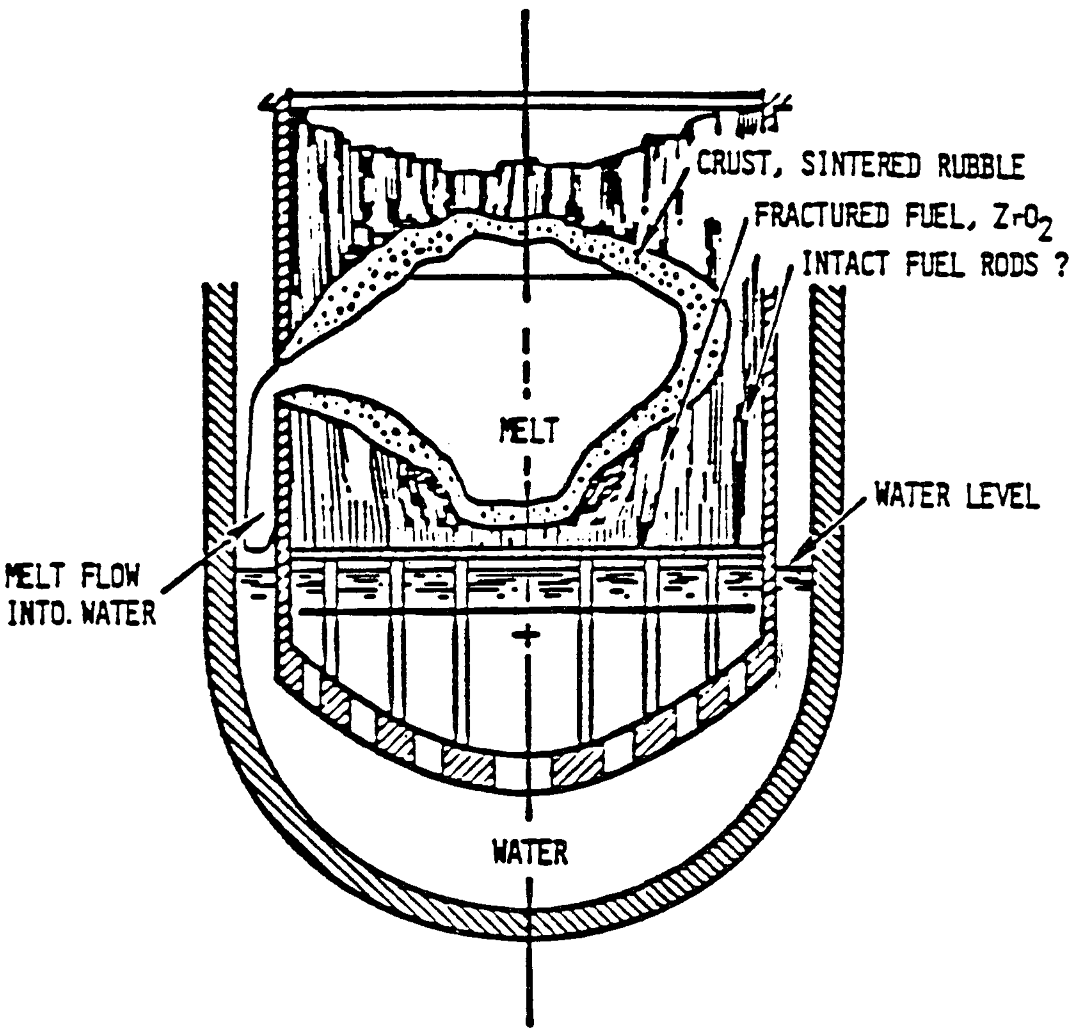

Fig. 3. Melt flow into the lower plenum by sideways penetration of the core barrel.

temperature but with a rather coherent failure of the lower grid plate. In our opinion, the WASH-1400 conclusion that up to 80% of the core may be molten before a massive pour occurs does not seem unreasonable at this time. Therefore, our current gedanken calculation would assuredly obtain satisfaction of the required conditions for edge-of-spectrum parameters, and perhaps more frequently. A 1 to 1/10 probability range was consequently selected for this branch.

4. Provided the initiation of a coherent pour or molten corium/water contact can be obtained, the standard arguments against a large steam explosion come from fluidization analyses. A big steam explosion is defined as one possessing greater than a 100 MPa peak pressure and sustained super-critical pressures driving the expansion until head impact. The reasons for this definition are as follows: The water critical pressure is 22 MPa. The potential expansion volume in the vessel is approximately 80 m^3. As stated by the Ross letter, a high energy explosion produces 2000 MJ. This can be obtained by expansion with a constant 25 MPa pressure differential. In practice, because of slug breakup and reduction of pressure as a consequence of expansion, higher initial pressures are required. The 100 MPa initial value is probably a lower limit of what is required, even for the case where the lower head is assumed not to fail. If fluidization furnishes the only arguments that can be made to limit coarse mixing, then a 1/10 probability would be appropriate.

A sample SIMMER-II calculation can be used to illustrate this point. In this calculation, the SIMMER-II mesh from the ZIP study (NUREG/CR-1411)[2] was used, but the corium and water were separated, as shown in Fig. 4. The corium temperature was 3100 K, the heat capacity was 0.54 J/(gm-K), the heat of fusion was 276 J/gm, and the corium melting temperature was assumed to be 2700 K. The amount of corium assumed was 131,760 kg. The 18,000 kg of water present was assumed to be saturated at 1 atm. Nominal SIMMER-II heat transfer assumptions were used with a droplet size of 20 mm in diameter. A partial blockage was assumed leading to a pour radius of one meter. Contour plots of fuel and coolant (water plus steam) densities over the first second of contact with these assumptions are shown in Figs. 5-10. The corium is seen to push the water away and up the downcomer. Not until 1 second has enough vapor pressure developed to

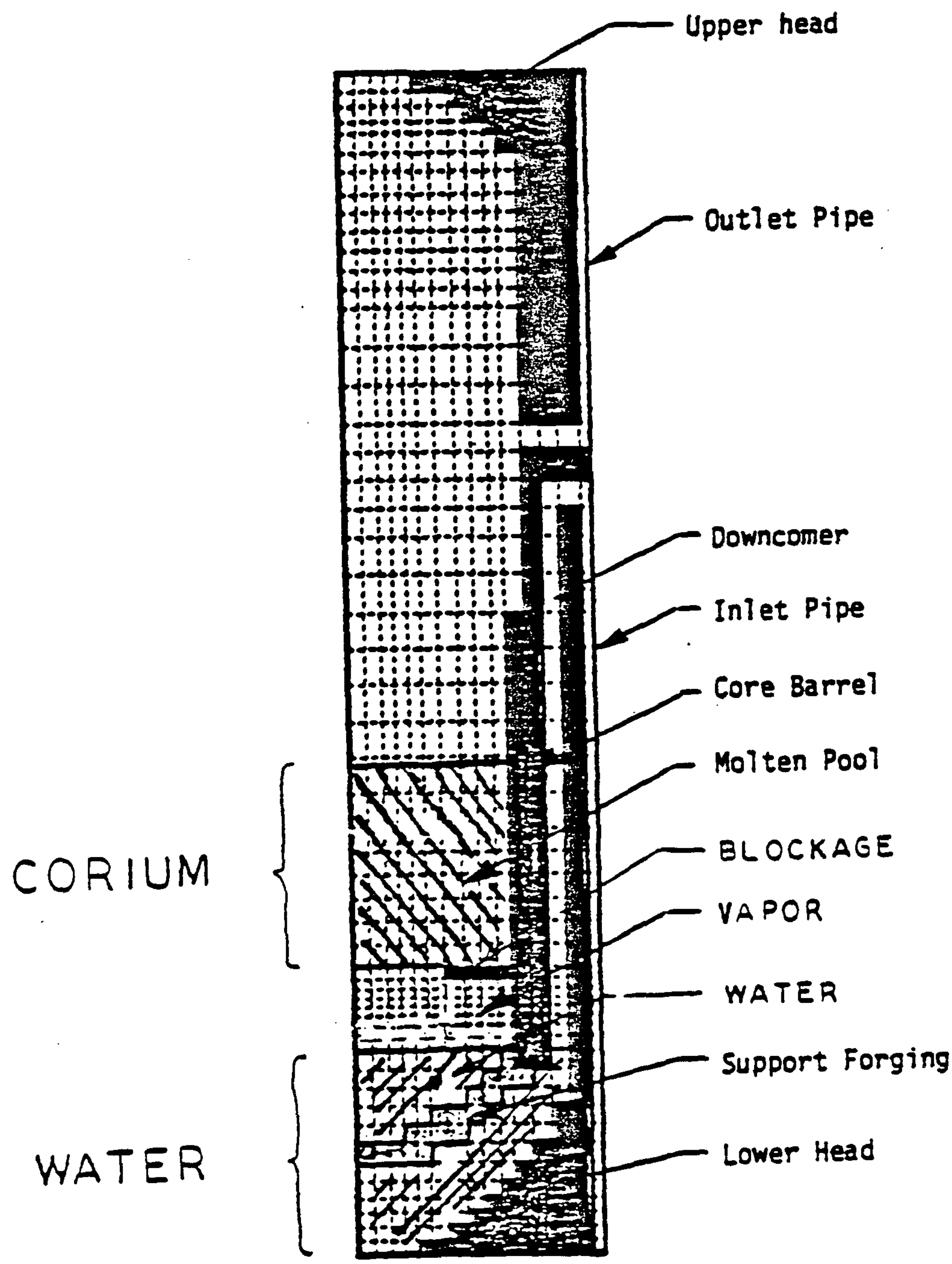

Fig. 4. SIMMER calculational mesh.

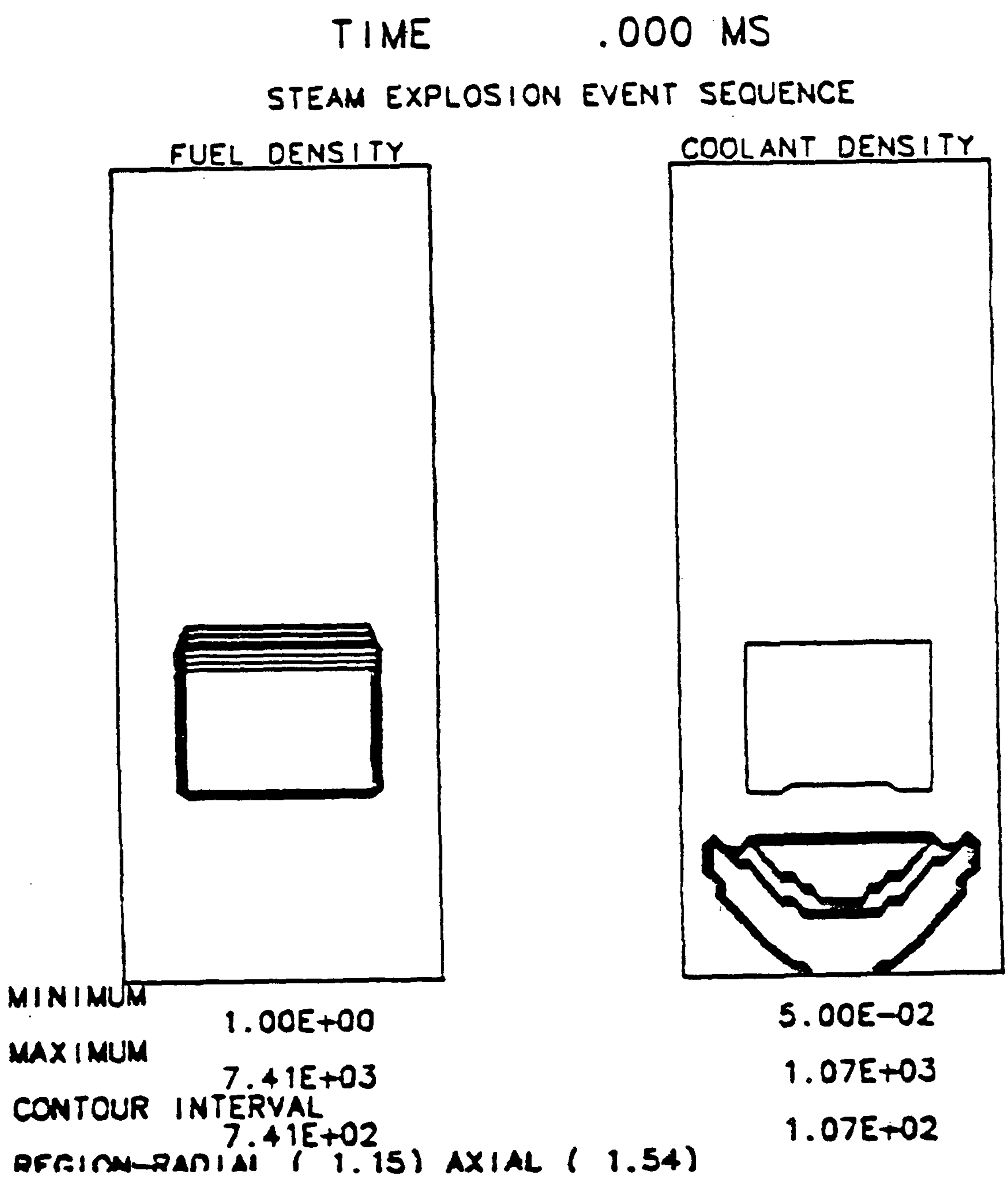

Fig. 5. Initial conditions, SIMMER-II edge-of-spectrum coarse premixing problem.

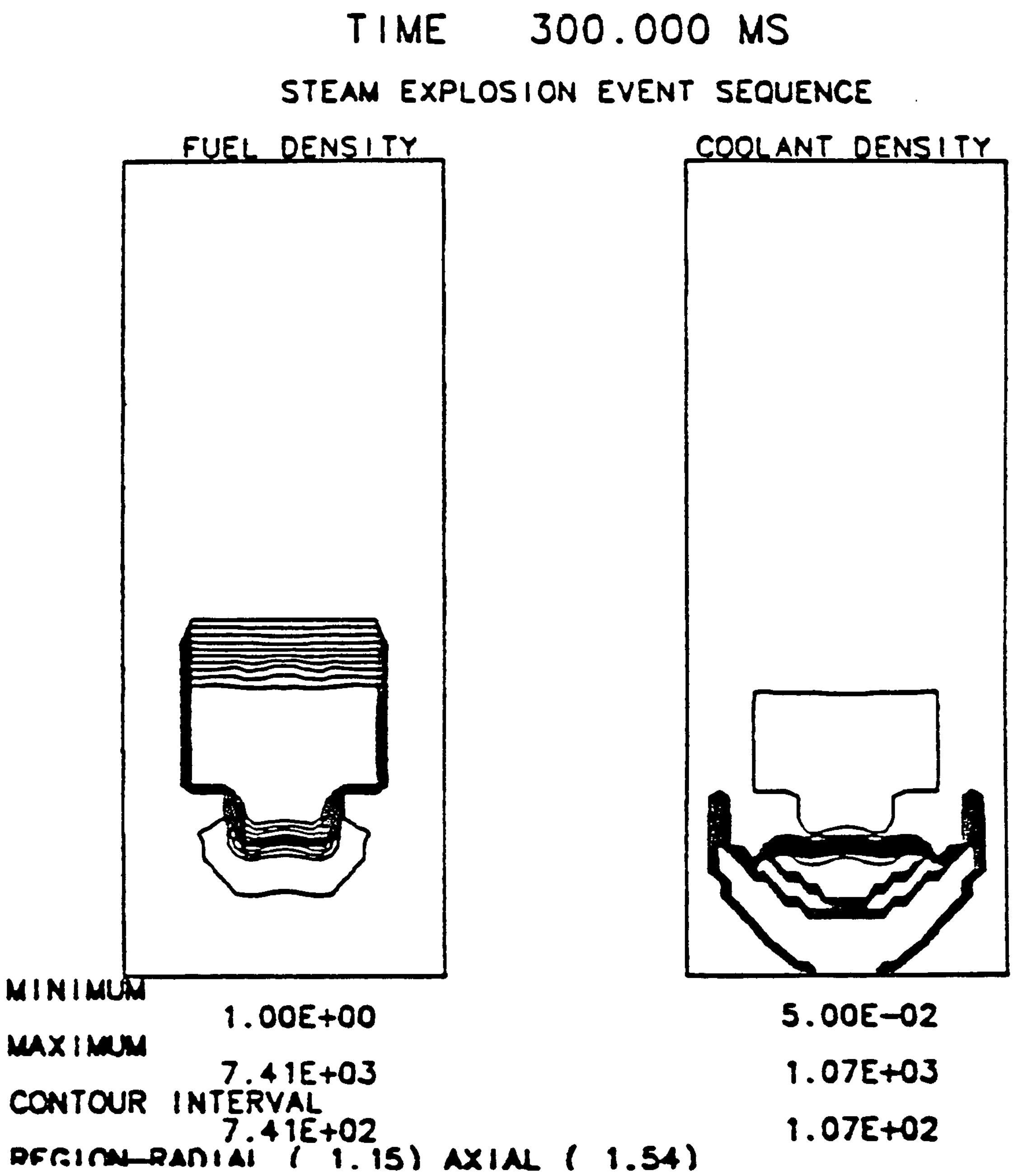

Fig. 6. Water-fuel contact, SIMMER-II edge-of-spectrum coarse premixing problem.

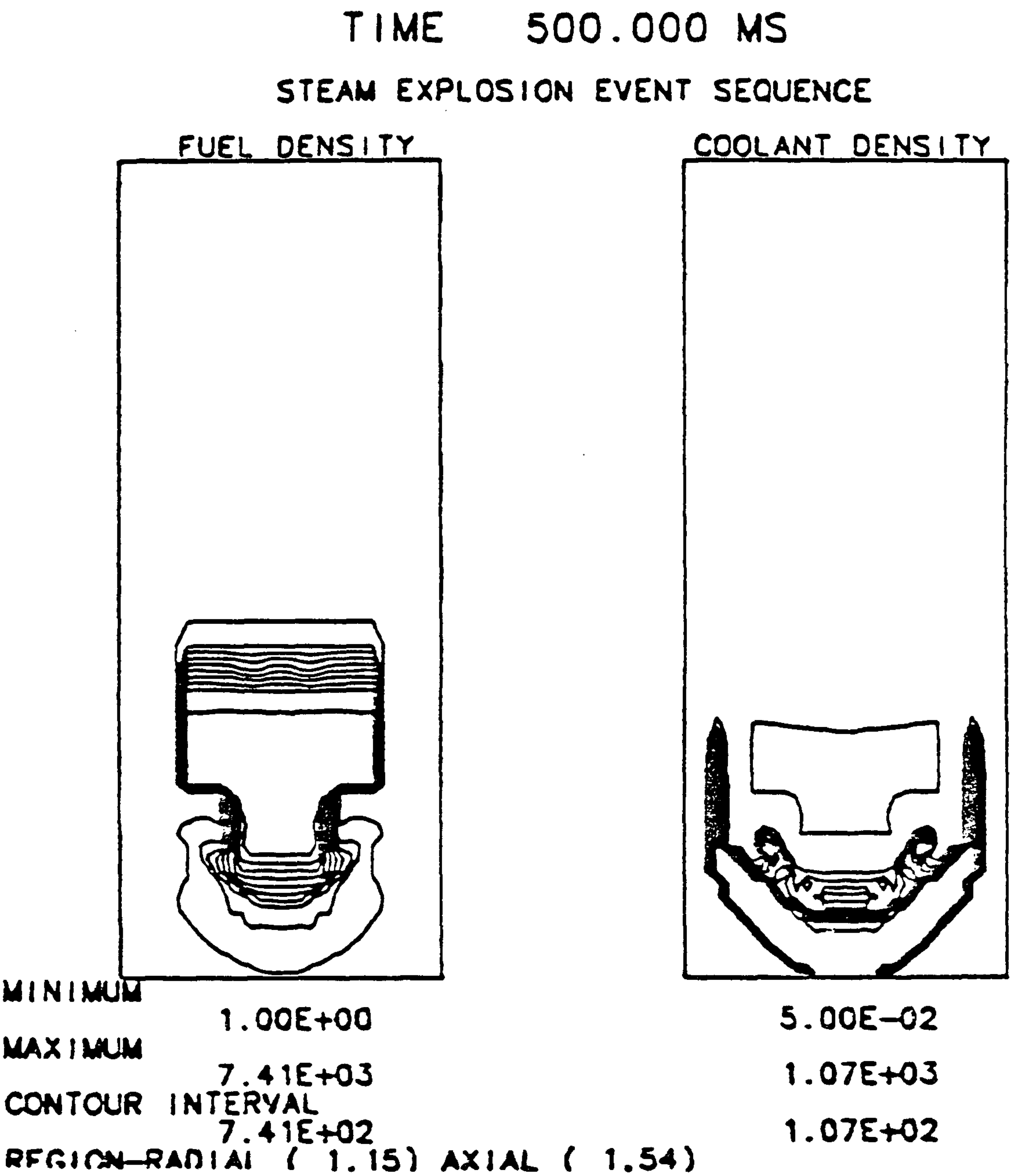

Fig. 7. Fuel contact with the support forging, SIMMER-II edge-of-spectrum coarse premixing problem.

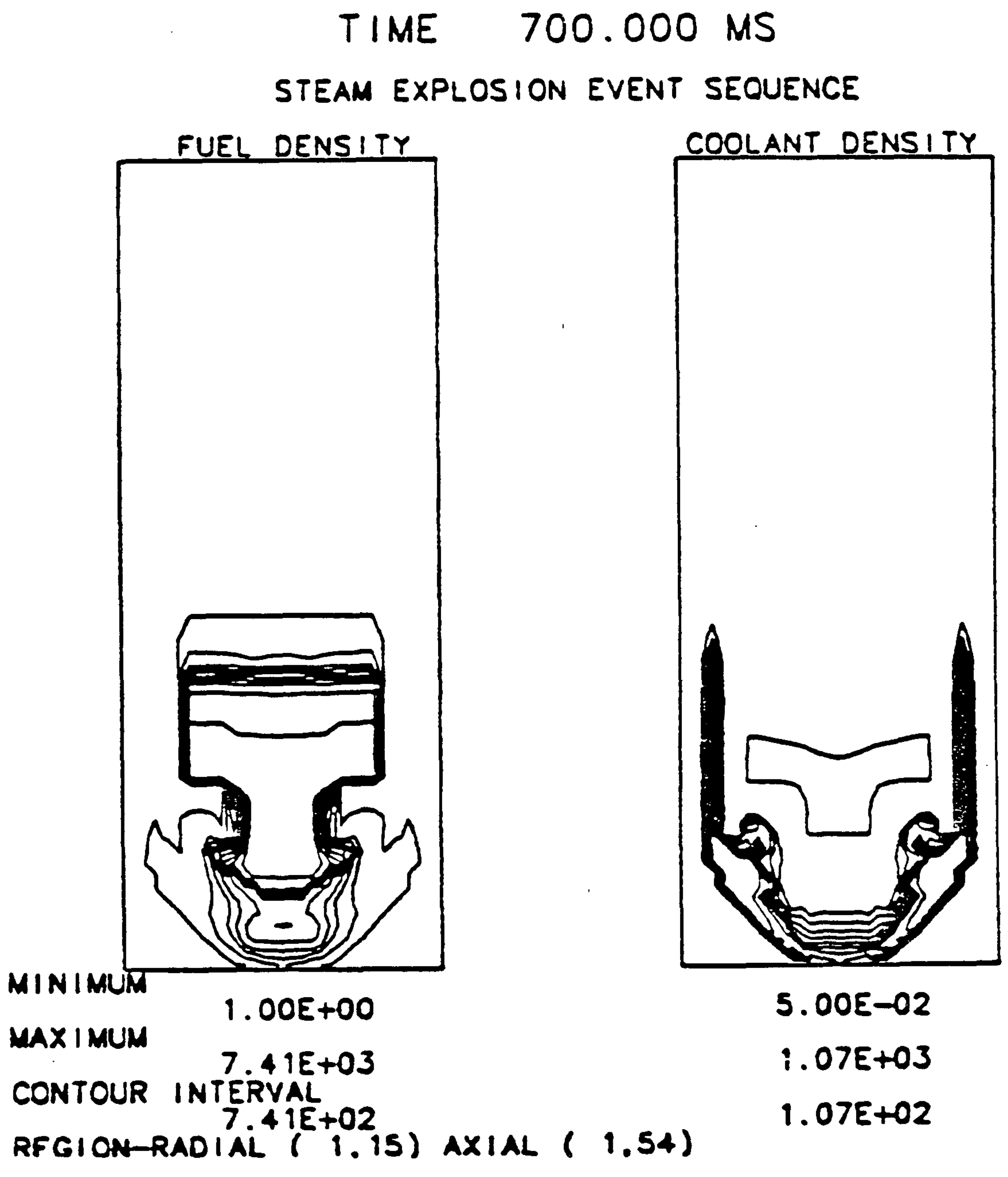

Fig. 8. Initial light contact of fuel with the lower head, SIMMER-II edge-of-spectrum coarse premixing problem.

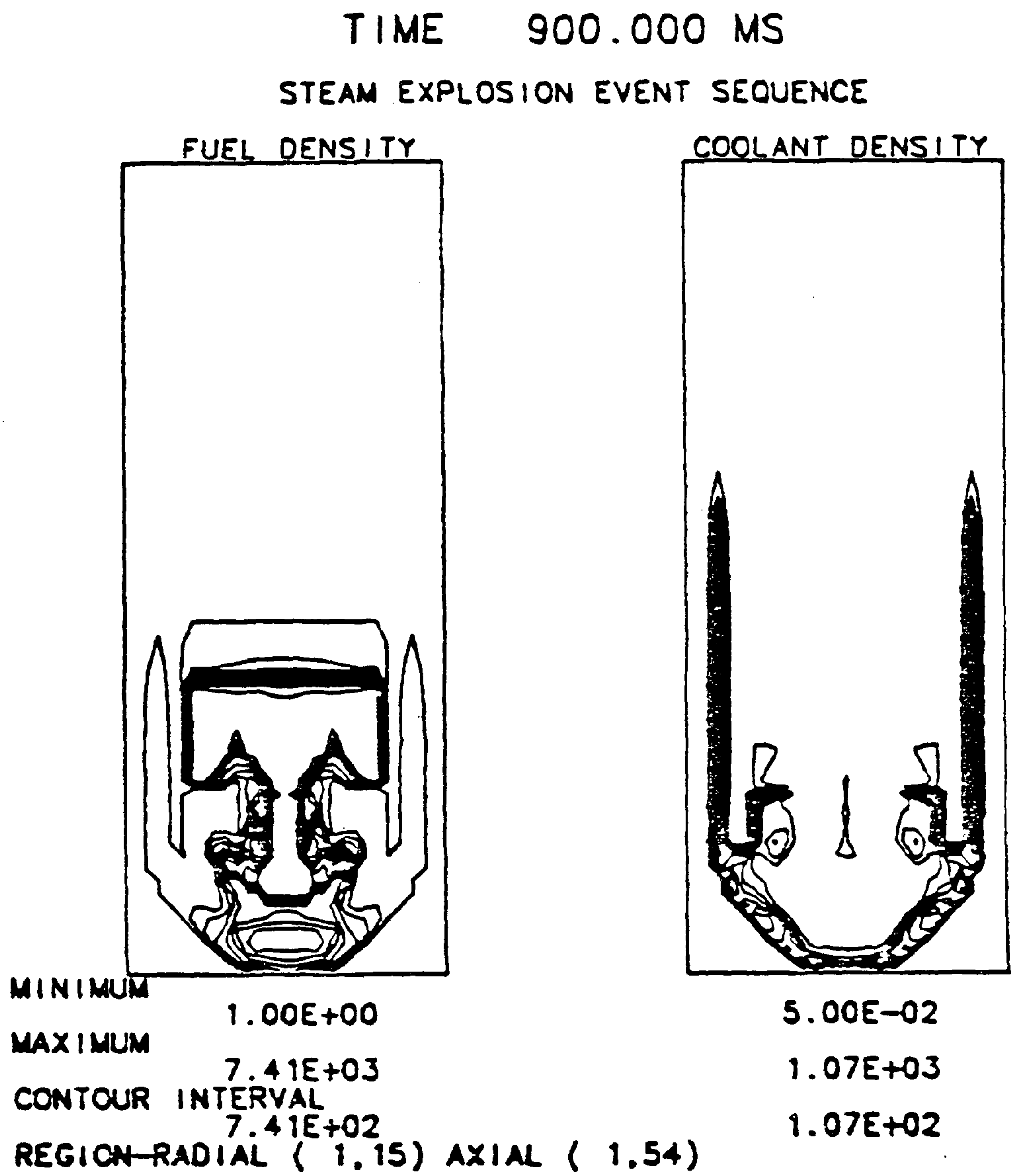

Fig. 9. Beginning of fuel pool breakup, SIMMER-II edge-of-spectrum coarse premixing problem.

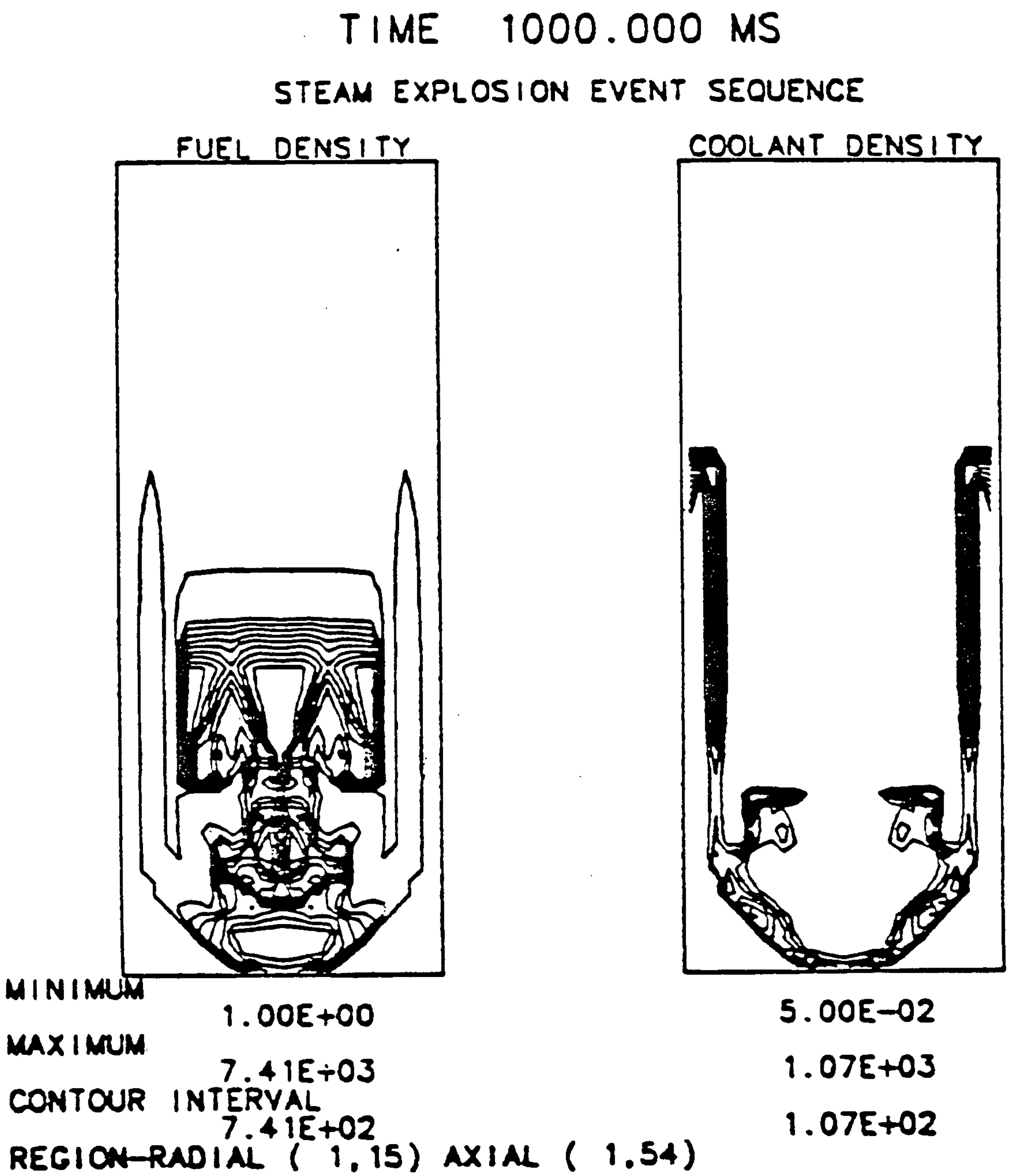

Fig. 10. Two-phase fuel pool formed, SIMMER-II edge-of-spectrum coarse premixing problem.

cause counter-flow through the corium. In addition to the limited heat transfer and the downcomer escape path, this time delay is a consequence of the inertial constraint posed by the corium pool. Assuming an explosion to occur at 1 second, the resulting pressure at the inlet plenum bottom is shown in Fig. 11. (The explosion segment of this case uses the equation of state, heat transfer, and vaporization-condensation model modifications developed for the present Los Alamos steam explosion program. Draft documentation on these changes can be made available if required.) The peak force on the head (head impact) is reached at about 54 ms after the start of the explosion (see the discussion on branch point 5); consequently, this can be defined as a big steam explosion. Additional calculations giving comparisons to the SNL experimental data base, giving the consequences of alternative assumptions, and comparing SIMMER-II heat transfer assumptions to the expected film boiling thermal radiation mechanisms are available. This calculation is admitted to use and undoubtedly requires edge-of-spectrum parameters. A best-estimate calculation is beyond currently available technology. However, in our opinion the present SIMMER-II calculation is not outside the spectrum of reason.

An alternative scenario can be considered. Recent SNL tests have shown a tendency towards early detonation. While little is known about how to model triggering phenomena in the context of a integral computer program, if confirmation of this early triggering tendency could be obtained from larger scale tests, achievement of a 1/100 probability would be possible. Indeed, our gedanken program suggests that the most likely outcome is a series of incoherent explosions. Although the possibility that a little explosion may be just the correct magnitude to be the precursor of a big explosion cannot be ruled out, modeling such a sequence with consistent assumptions in a reactor configuration seems difficult. A big steam explosion may well lie outside the spectrum of reason.

Finally, we observe that the more probable accident sequences (TMLB′ and the small break LOCA) may be speculated to lead to a somewhat elevated ambient pressure at the time of a steam explosion. If the resulting film boiling stability means that a steam explosion cannot be triggered, the problem is eliminated. However, because the volume of steam is reduced at high pressure,

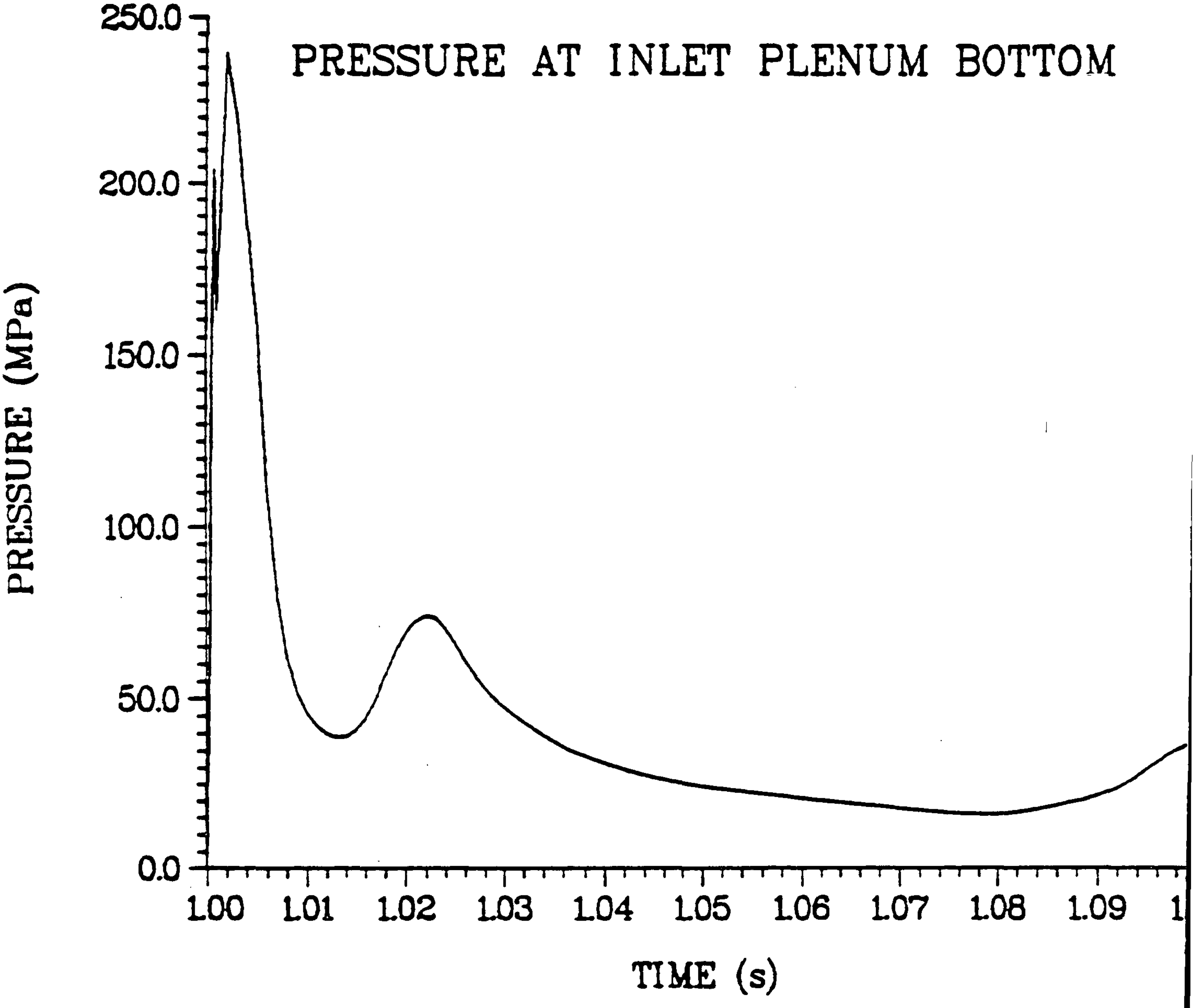

Fig. 11. Results of steam explosion at 1 second in the SIMMER-II edge-of-spectrum coarse premixing problem.

higher concentrations of fuel and water can exist at lower vapor volume fractions. If a steam explosion is triggered, it could be more efficient. In any case, the requirements for a trigger at high pressure are still not clearly understood, and maintaining a probability range of 1/10 to 1/100 seems a reasonable best-estimate at this time.

5. Significant head loading is defined as an averaged force over the head of greater than 1000 MN. Some minimum impulse is also required, but this is small given the time duration of the expected two-phase loadings. The value of 1000 MN comes from the following:

(a) The NUREG/CR-3369 document gives a bolt failure tension of 1170 MN, corresponding to a vessel static failure pressure of 80 MPa.

(b) The Los Alamos ZIP study (NUREG/CR-1411) using less conservative assumptions found a hydrostatic pressure capability of 100 MPa before bolt failure would occur.

(c) In a computer simulation, the Los Alamos ZIP study also obtained substantial plastic deformation of the upper head at a uniform pressure load of 70 MPa.

(d) Because the thermal conditions of the upper head are so uncertain and depend on the accident sequence only a rough number is possible. At 1000 MN, failure should be possible in some cases, but without the excess momentum present to form an energetic large missile.

The current Los Alamos program is oriented to obtaining a confident upper bound on the damage potential of a large steam explosion. One possible outcome of this program is the conclusion that effective slug breakup and/or effective venting of the lower head will preclude significant upper head loading for all but end-of-spectrum parametric choices. Figs. 12-18 give the expansion dynamics for the previously discussed SIMMER-II edge-of-spectrum coarse mixing case. Effective slug breakup is seen to occur. As shown by Fig. 19, the peak force on the head is less than 1000 MN. The peak kinetic energy was 1680 MJ. (The concentration of fuel towards the center node is mainly the consequence of the

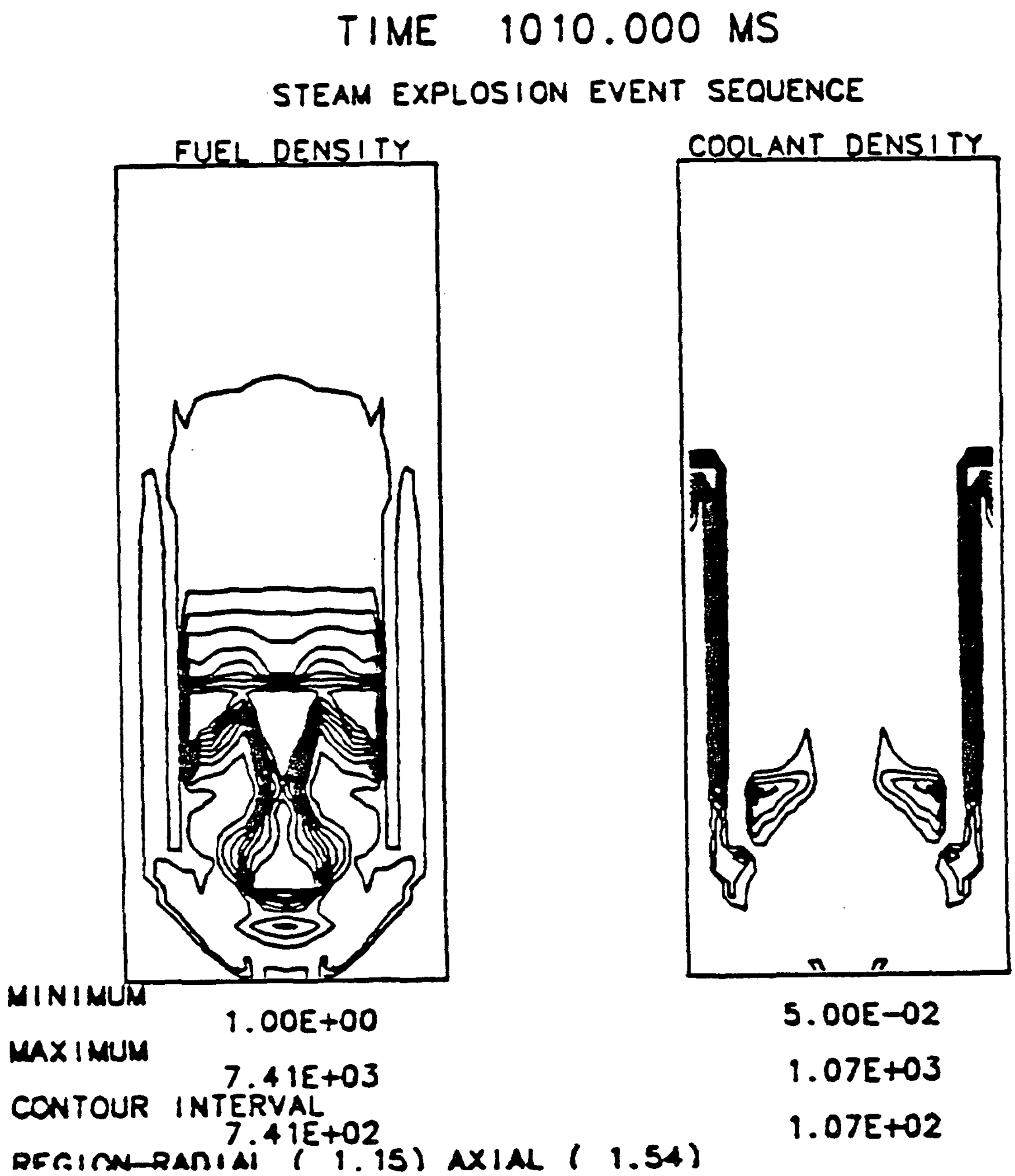

Fig. 12. Initial expansion instability, SIMMER-II edge-of-spectrum coarse premixing problem.

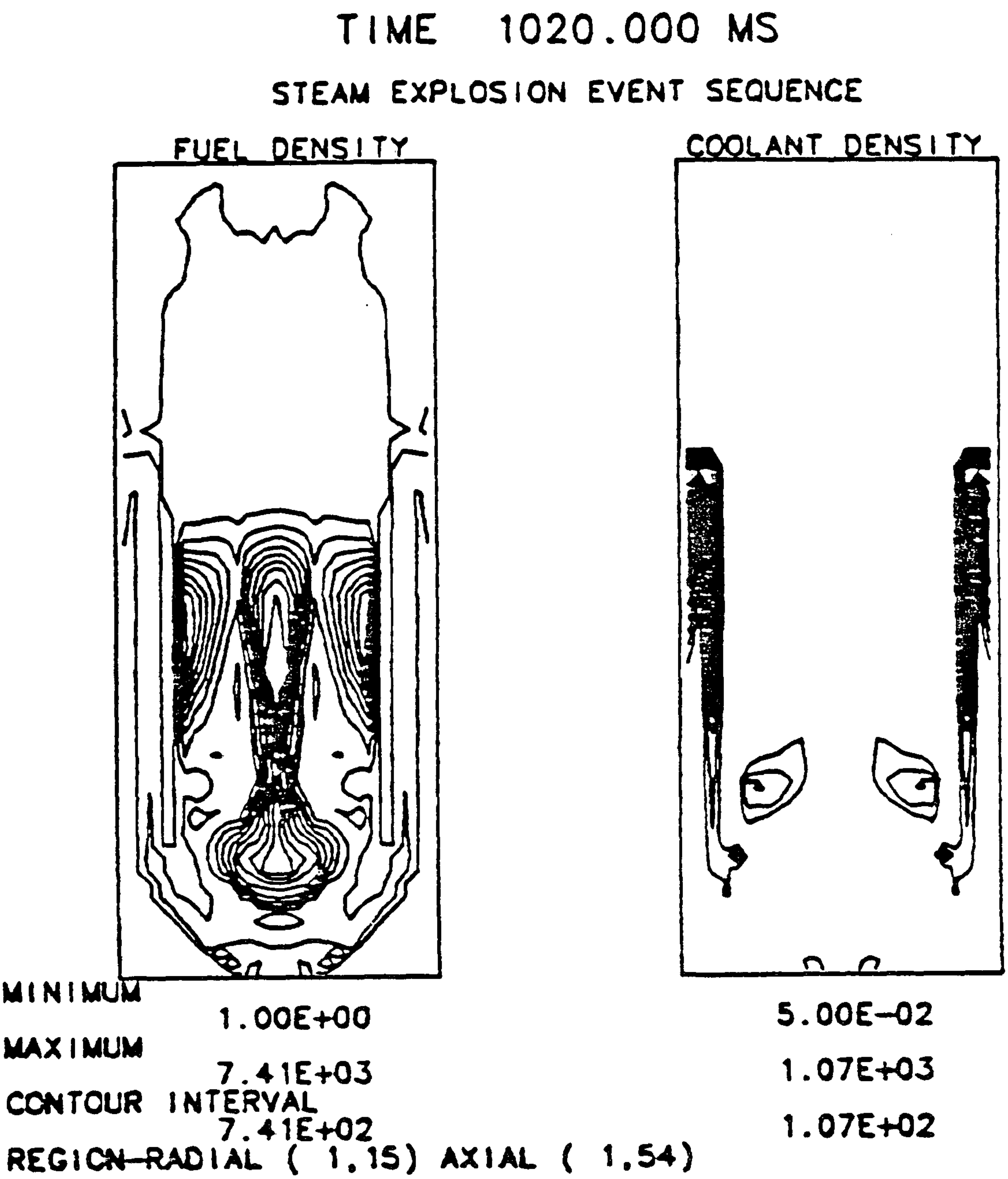

Fig. 13. Instability development, expansion phase of the SIMMER-II edge-of-spectrum coarse premixing problem.

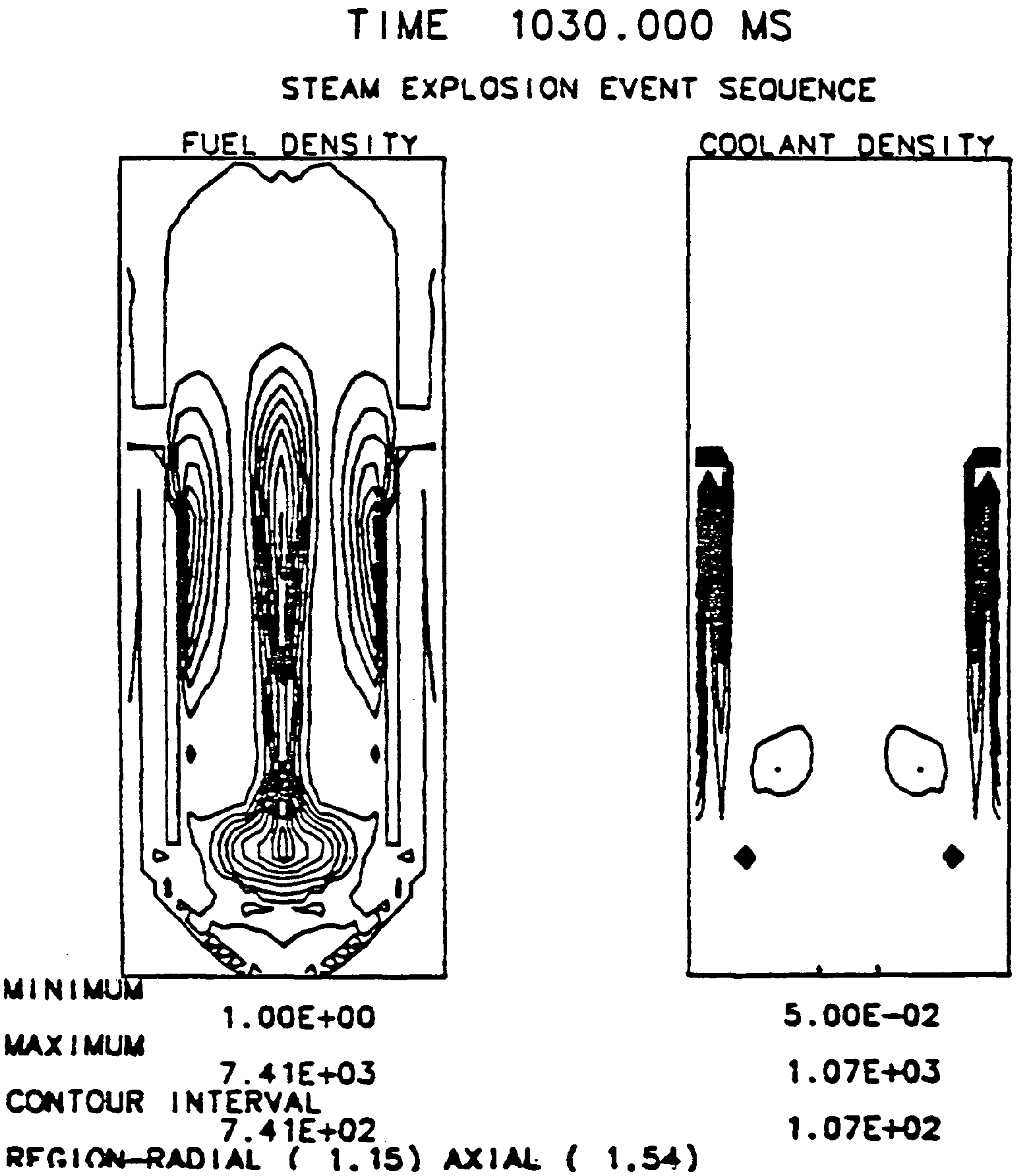

Fig. 14. Venting beginning in the expansion phase of the SIMMER-II edge-of-spectrum coarse premixing problem.

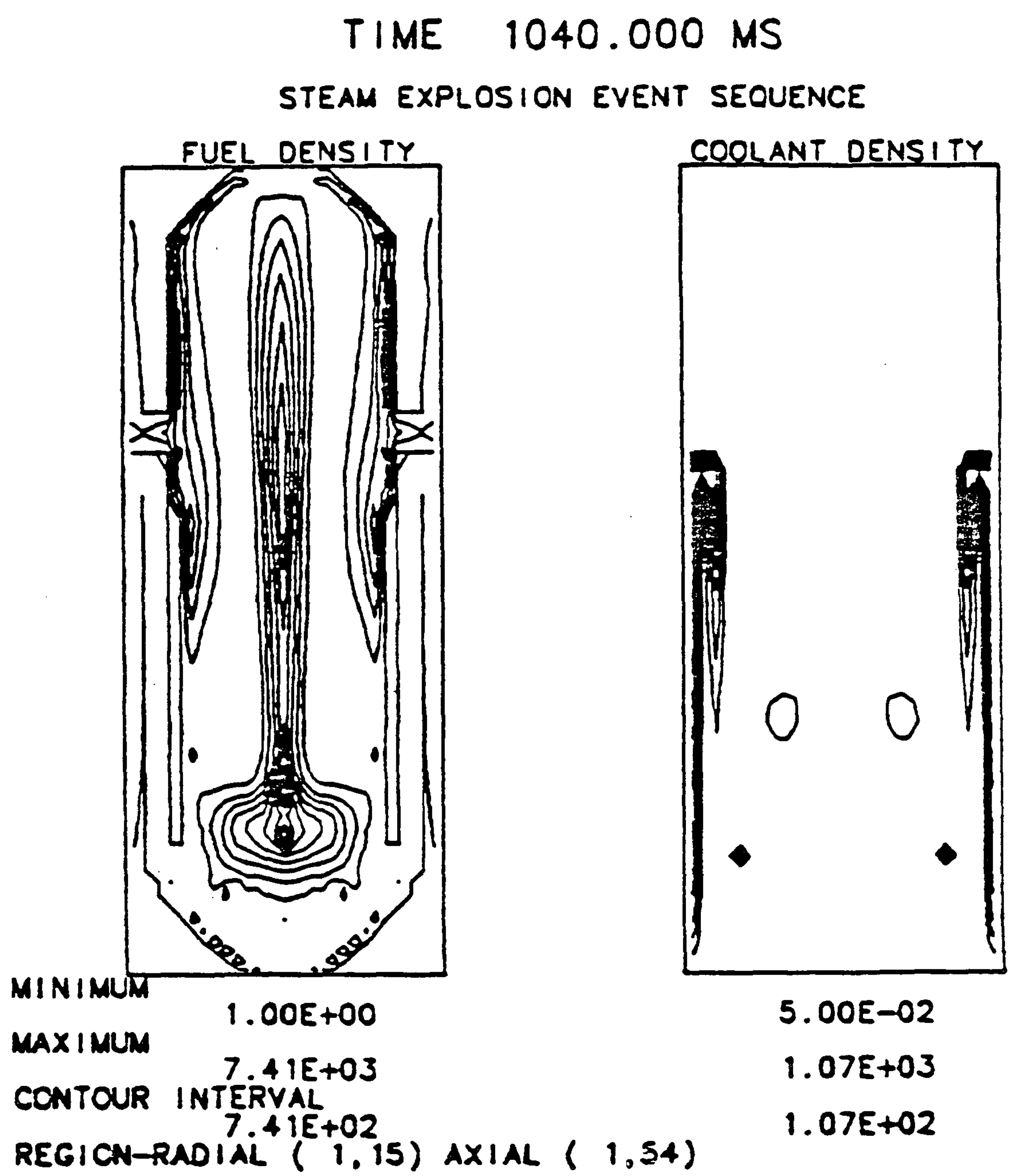

Fig. 15. Configuration just before impact in the expansion phase of the SIMMER-II edge-of-spectrum coarse premixing problem.

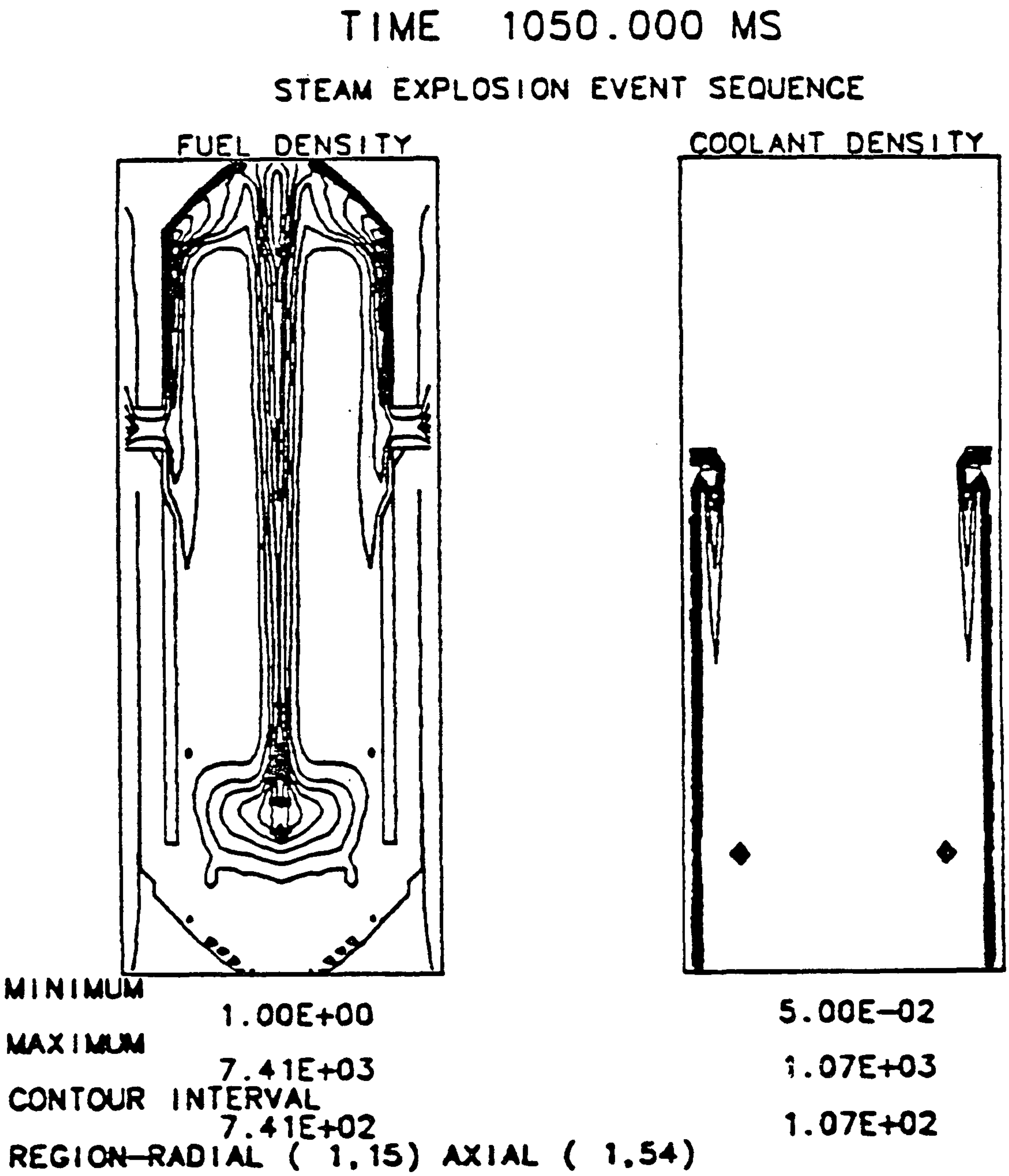

Fig. 16. Material impact beginning in the expansion phase of the SIMMER-II edge-of-spectrum coarse premixing problem.

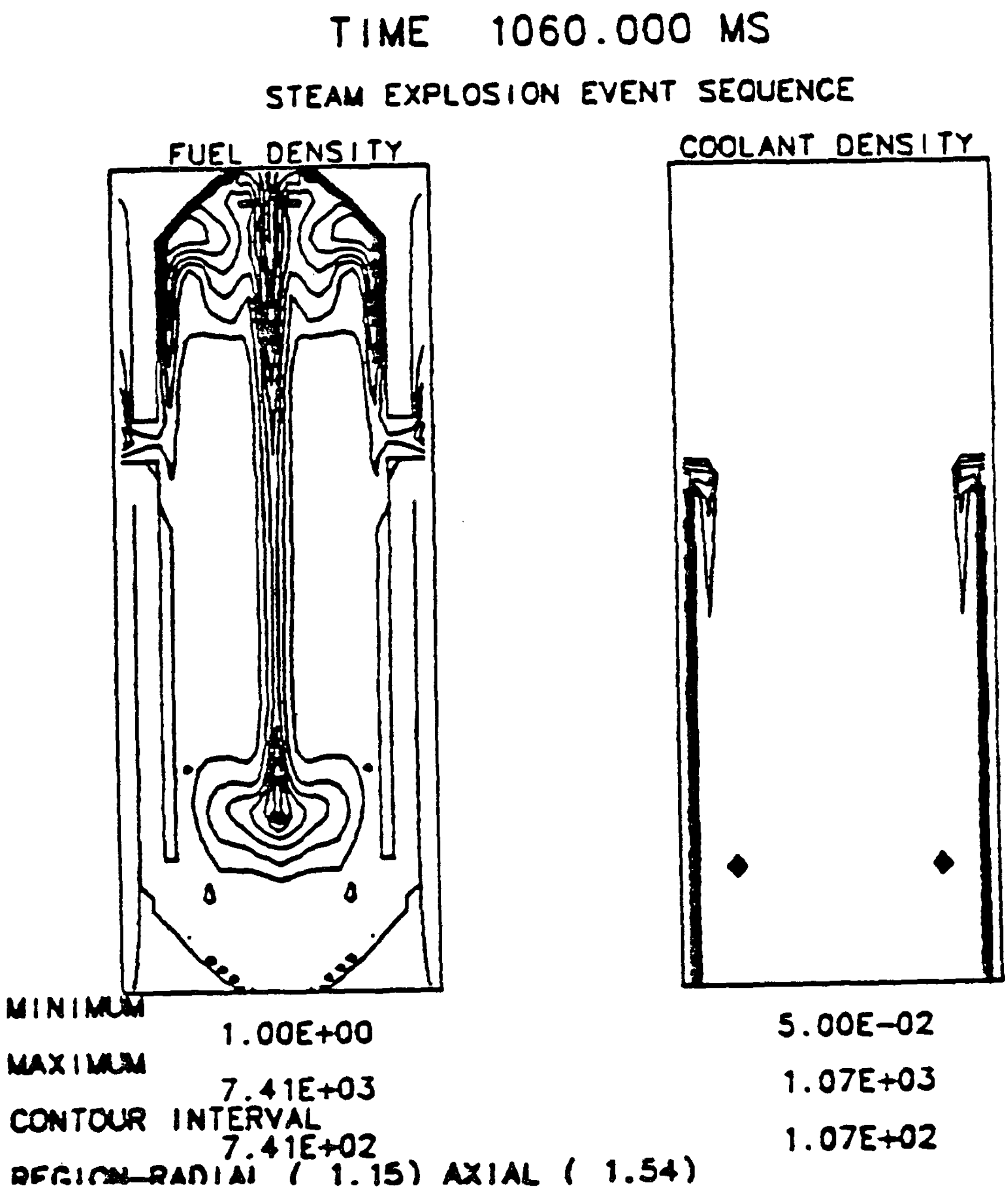

Fig. 17. Material impact complete and rebound beginning in the expansion phase of the SIMMER-II edge-of-spectrum coarse premixing problem.

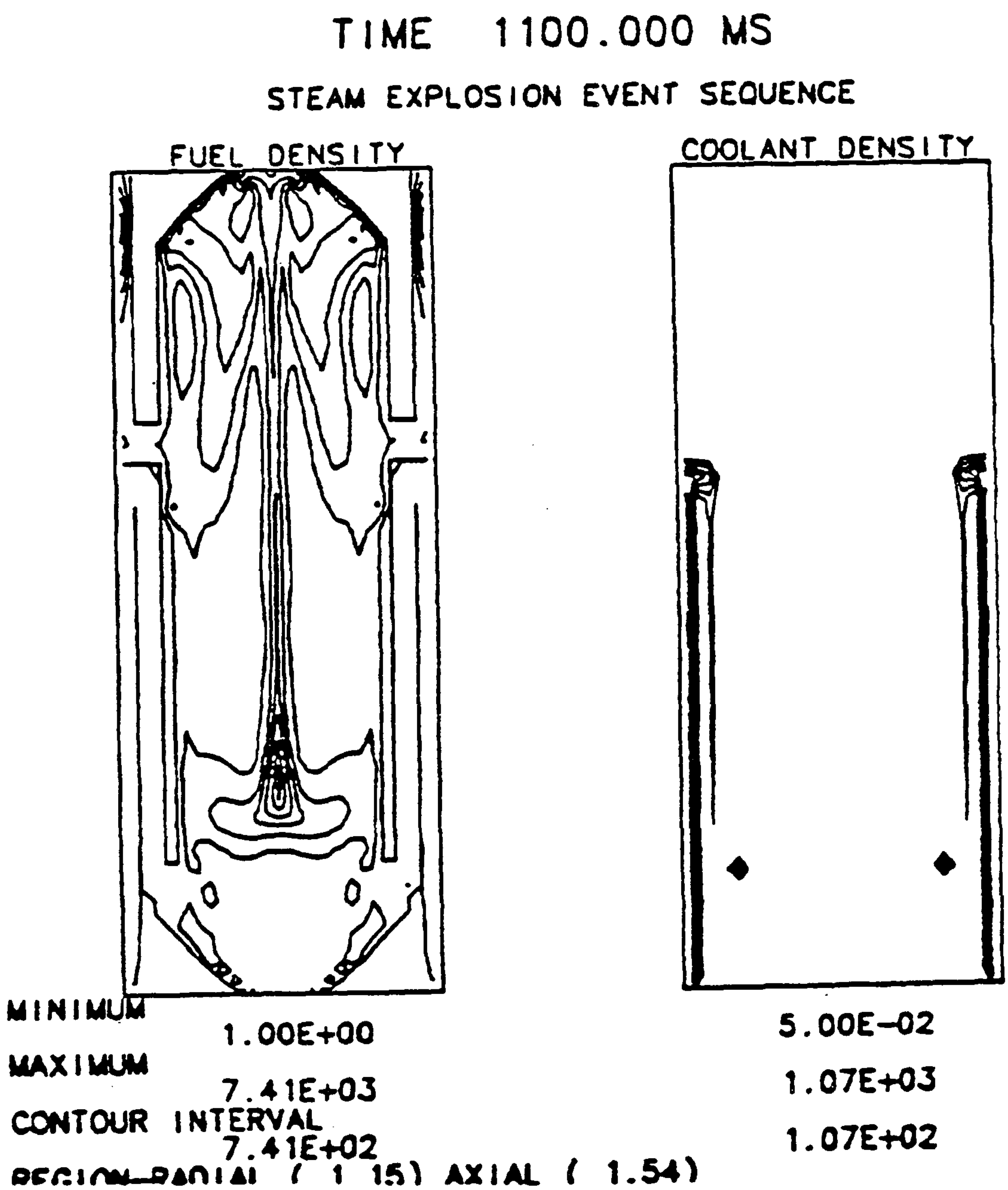

Fig. 18. Final conditions in the expansion phase of the SIMMER-II edge-of-spectrum coarse premixing problem.

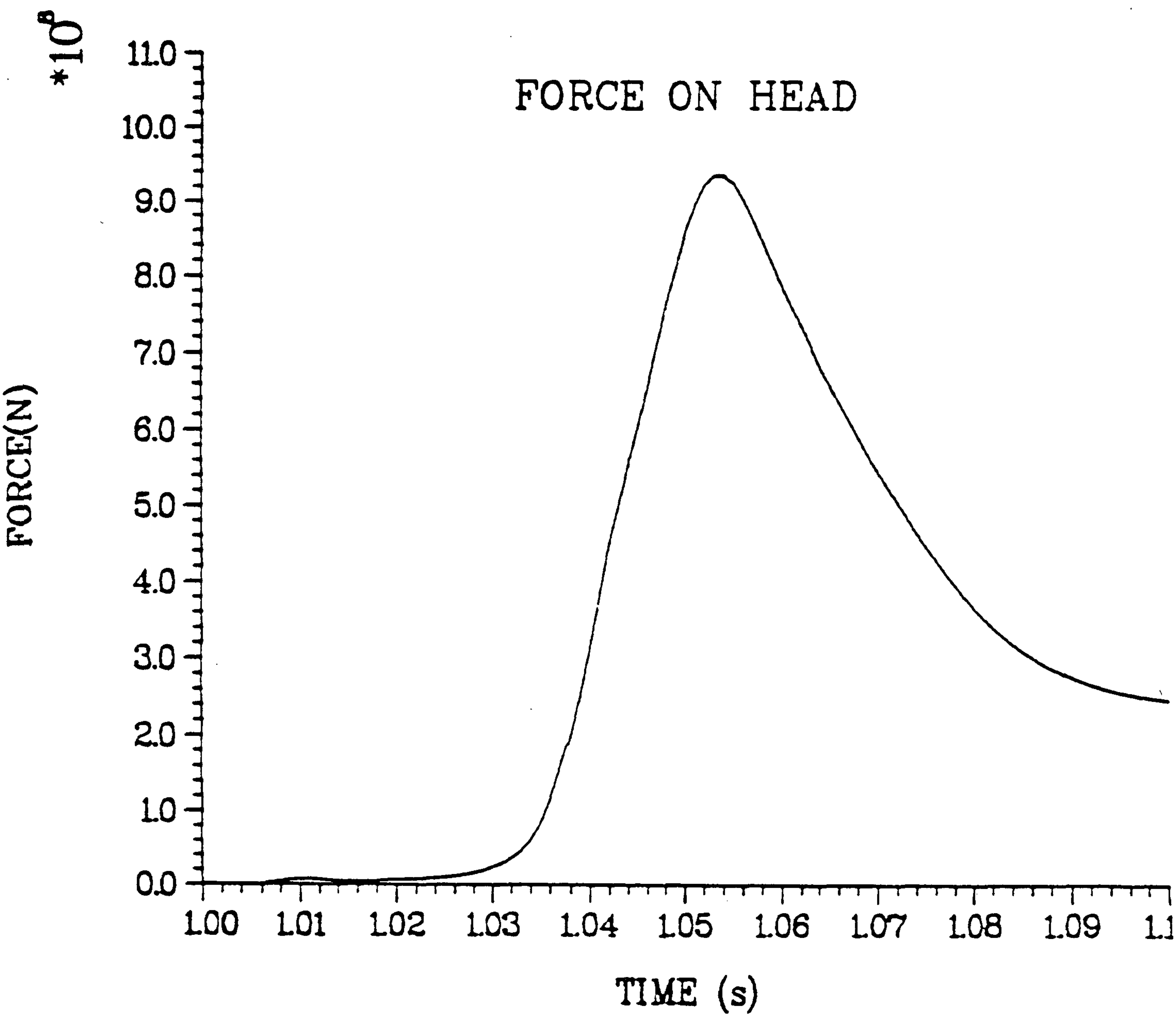

Fig. 19. Head loading force for the explosion at 1 second in the SIMMER-II edge-of-spectrum coarse premixing problem.

initial conditions for the explos.on from the premixing stage; however, experimental results also suggest SIMMER-II may be nonconservative in an idealized expansion starting with non-uniform initial conditions.) If we assume an explosion to occur at another logical time, 0.7 s, before the fuel pool has become two-phase, higher loading forces can be obtained. The force shown in Fig. 20 goes above 1500 MN, although the peak kinetic energy is reduced to 1490 MJ. (The effect of failure of the lower plenum is not considered in these calculations, because a lower plenum failure model has not yet been formulated for the SIMMER-II code. Also, in a best-estimate case part of the core may still be solid debris. Besides a reduction in pressure generation, acceleration of coarse solid debris may be mitigative in allowing more venting of driving pressures and in reducing the explosive kinetic energies through inelastic collisions. Finally, the mitigating effects of the upper core structure have been ignored. Their inclusion is difficult because of uncertainties in their thermal state, but clearly some differences should be expected as a consequence of the significant steel mass, or about 35,000 kg in the SURRY reactor UIS, for which data was available to us. A first approximation for a future calculation is to assume impact with such structure is ideally plastic, with the mass of initially stationary structure joining that of the moving liquid.) Analysis of the spatial and time loading distributions from the 0.7 s explosion case gives a failure time of the lower head as 2.1 ms after the start of the explosion. Clearly some mitigation from lower head venting should be anticipated. At present, gedanken computations suggest that edge-of-spectrum parameters may be required for significant head loading. However, the results of the Los Alamos steam explosion program are still incomplete. Significant head loading may well prove to be outside the spectrum of reason; alternatively, the possibility of a big steam explosion always leading to significant head loading must be allowed for. Consequently, a 1/100 to 1 probability is assigned here as a best-estimate. If effective lower-head venting or effective slug breakup does occur, a steam explosion must possess highly unusual properties to obtain significant head loadings. For example, an extremely large corium/water premixture must exist. This is considered outside the spectrum of reason and given a 1/100 probability.

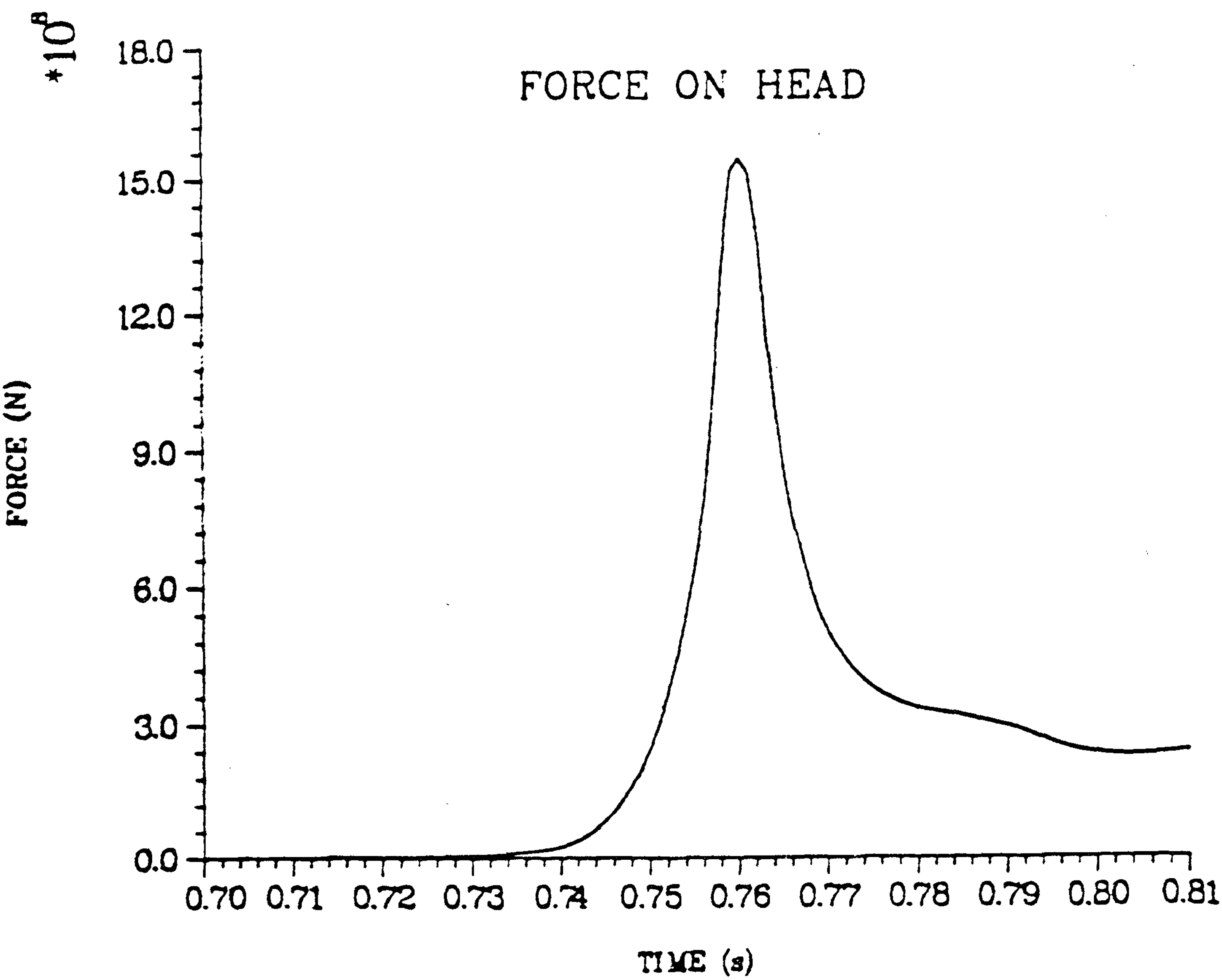

Fig. 20. Head loading force for an explosion at 0.7 s in the expansion phase of the SIMMER-II edge-of-spectrum coarse premixing problem.

6. Except for the first 1200 MJ case, the general result of the cases in the old Los Alamos ZIP study was produce a head force that exceeded 1000 MN. Also, preliminary estimates indicate that the head does not fail for the case shown in Fig. 19, while failure is predicted for the case shown in Fig. 20. However, the larger the steam explosion the more mitigation should be possible from lower head failure, and significant head loading and even head failure does not guarantee a large energetic missile. A large energetic missile must meet both mass and momentum requirements. The missile must be massive if the missile shields are not to be effective, and consequently a significant fraction of the head plus associated corium must be involved. Continuing spray from the vessel may not be very effective for missile acceleration after the head has entered the main containment volume, so an "initial" velocity of at least 25 m/s is required. This velocity is the minimum to allow a missile to reach the top of the containment if the missile shields, polar crane, and other impeding structures are neglected.

Both the old Los Alamos ZIP study and the current calculations indicate a concentration of loading towards the apex of the vessel. Peak pressures are higher by more than a factor of 2 at the top of the vessel in comparison to values near the bolts. Such a loading pattern favors production of heavy shrapnel rather than the intact head becoming a missile.

On the other hand, if head failure does occur, the energy requirements to produce 25 m/s are not large. A 1.3×10^5 kg head and associated structure with about 40 MJ of kinetic energy will possess this velocity. How the momentum will be partitioned in head failure is beyond the scope of a hand calculation; however, simply the initiation of upper head venting will not eliminate fluid axial momentum. In our discussion of Appendix B of NUREG/CR-3369, the failure mode of the head is discussed in more detail. In any case, the goal of the Los Alamos program is to investigate whether production of large energetic missiles is physically unreasonable given a big steam explosion. While calculated loading patterns suggest that formation of a large missile should not be within the best estimate range of behavior, with present knowledge our gedanken calculation is indeterminate in deciding which path requires edge-of-spectrum parameters. A 1 - 1/10 estimate was consequently chosen for both branches.

7. Once the vessel head or vessel head bolts fail, all or part of it (different pieces of the failed head) will move vertically and strike the missile shield assembly. (The term vertically is used rather loosely here because, as discussed in NUREG/CR-3369, differences in bolt failure times will cause some asymmetries). The equipment on top of the head (control rod drive mechanisms, cooling system, etc.) will contact the missile shield first and absorb some energy as it crushes. The head itself will contact the two, shield-supporting 36-in. wide flange I beams before it contacts the concrete missile shield sections. Again, deformation of these structural members and the head at the contact area will absorb some energy in plastic deformation. Connection of the concrete missile shield slabs (three 6 ft by 18 ft by 3 ft thick slabs with a 1-in. thick steel plate on the bottom) to the I beams is with relatively small bolts that ensure the slabs do not move during seismic events. These will rapidly fail and allow the two outside slabs and the I beams to slip off of the upward moving head. The middle slab could stay in place until the head/slab combination contacts the polar crane structure. The missile shield structure probably varies considerably from reactor to reactor but almost all PWR and BWR designs include a polar crane that will be positioned directly above the center of the reactor vessel. The mass of the polar crane is typically two to three times the mass of the reactor vessel head. This structure may deform considerably but could bring the velocity of the head down to levels that will preclude significant damage to the containment. Because of the potential stoppage of even a large energetic missile by the polar crane, a 1 to 1/10 probability was adopted for the path of containment failure. A realistic estimate requires knowledge on by how much the initial head velocity exceeds 25 m/s. For high enough velocities actual stoppage may prove to be outside the spectrum of reason, so a 1/100 to 1 probability was selected for this alternative.

In conclusion, our suggestion is that with current uncertainties the conditional probability of direct containment failure from a steam explosion ranges from essentially zero to 0.1. The higher number is obtained by assuming that only achievement of a big steam explosion requires edge-of-spectrum parameters in the gedanken code. The unlikelihood of a big steam explosion is based primarily on coarse mixing difficulties. Many possibilities exist, that if verified, could reduce the 0.1 estimate. The most likely is that venting from lower head failure, inelastic collisions from remaining structure, and fluid breakup would mean that even a big steam explosion obtained with edge-of-spectrum parameters would not produce significant head loading. Anticipated results from current investigations at Los Alamos should allow the 0.1 bound to be reduced to 0.01. Reduction to the 0.001 level, or that of physically unreasonable behavior, is possible depending on the outcome of a head loading analysis and a determination of what magnitude of steam explosion can truly be accommodated, and the examining of what assumptions are required to obtain that large a steam explosion. However, in scaling a phenomenon by more than three orders of magnitude (or from the SNL data base up to reactor scale), characteristics can change completely. In our opinion, more knowledge is required to truly justify any expectation that a reduction below a zero to 0.01 range of values will prove to be correct. If one number must be proposed as a best-estimate, an anticipated 0.01 bounding estimate on the containment failure path might be expected to be reduced at one or two branches by an additional order of magnitude. Some care is required here, because the combination of three edge-of-spectrum calculations does not necessarily imply physically unreasonable behavior. We must be careful about our probabilities becoming too low simply through branch proliferation. However, if we take a log average of this expectation, the median best-estimate value is 3×10^{-4}.

DETAILS ON QUESTION 2

This review of NUREG/CR-3369 is separated into three parts. These are as follows:

(a) Opinions as to whether the conditional probability of containment failure should be one.

(b) Opinions as to the usefulness of this methodology for doing a risk assessment.

(c) Opinions on details in the subject document.

A. Comments on the conditional probabilities proposed by the SNL document.

The SNL document provides a delineation of major aspects of steam explosions and associated effects, and then provides descriptions of the uncertainties that SNL believes to be associated with these aspects. The assignment of uncertainty ranges is a highly subjective process particularly when attempting to extrapolate the current, limited data base to reactor scale and to a variety of postulated accidents. Thus, perspectives of these ranges should be expected to differ. We offer our perspectives in this section on the appropriateness of the "high-end" ranges.

First, we suggest that the conditional probability should not range up to unity although this is really a question of the confidence level that should be employed. The SNL formalism does bias the conclusion toward containment failure when they setup distributions to

(a) assume all the molten corium can potentially mix with water and generate a steam explosion,

(b) assume a significant fraction of the explosion energy is transmitted to the upper head,

(c) assume the head to fail at the bolts and become a large missile.

We suspect that none of these assumptions will eventually prove to be valid in the extreme sense used in the SNL study.

On page 21, NUREG/CR-3369 states that for their study of steam explosion efficiencies, Corradini and Swenson assumed that in an initial coarse mixture, a 0.50 volume fraction of steam exists. Although this steam volume fraction may be excessive, evidently appreciable steam should exist in any coarse premixture. From page 40 of NUREG/CR-3369 we can infer that explosions strong enough to cause bolt failure will involve almost all the melt. If the initial configuration of water and corium is as shown in Fig. 21, most of the volume is occupied by single-phase liquid and/or solid material. If all the water and all the corium is to mix as assumed in NUREG/CR-3369, production of an appreciable steam volume fraction requires water moving upward through the molten corium in the coarse premixing phase, not just corium pouring into the water. This is not only different from the experimental situations treated by SNL, but also would seem difficult to realistically achieve. A water-steam mixture moving into a concentrated corium region is likely to receive far too much heat to continue in a quiescent film boiling process. Thus, mixing of large fractions of the corium with typically observed steam volume fractions does not seem reasonable.

If all of the corium does manage to mix with water in a two-phase coarse premixture, an explosion no longer even initially accelerates a slug. Instead we are looking at a spray. Significant head loading becomes more difficult to achieve. A SIMMER-II calculation was done to illustrate this point. The initial conditions were to assume 94,000 kg of corium was mixed with 20,000 kg of water. An extra 8000 kg of water was assumed to exist unmixed in the downcomer. The extra solid corium was ignored. The corium temperature was 2800 K. Its heat capacity was 500 J/(kg-K), with no heat of fusion such that its energy above the water temperature of 400 K was 1.2 MJ/kg, as assumed in NUREG/CR-3369. The initial steam volume fraction was 0.19 in the premixture This leads to an initial height of the mixture that is about the same as the top

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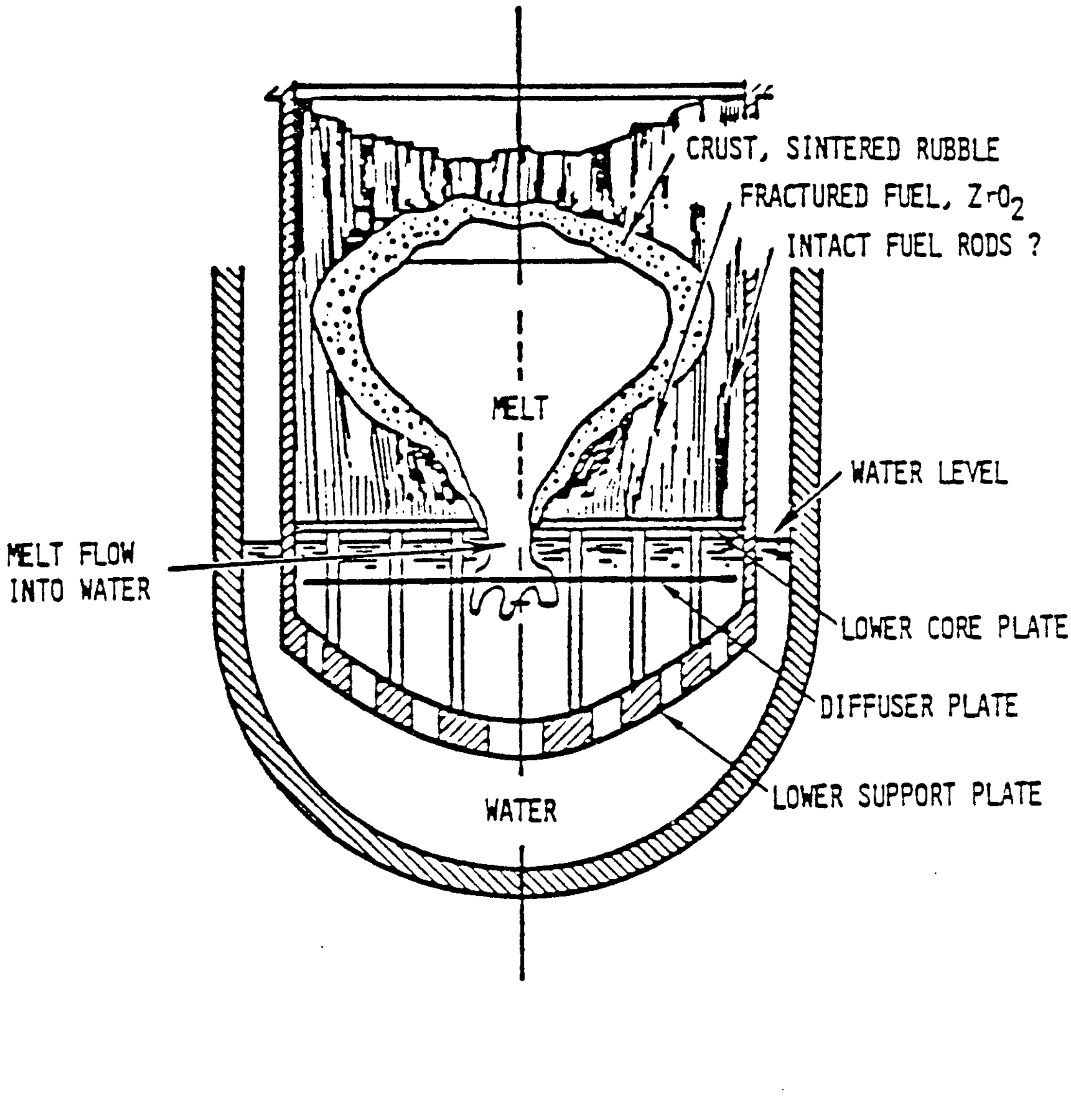


Fig. 21. Melt pour into lower plenum by failure of the lower core plate.

of the molten pool in the calculation performed for the premixing problem considered in response to question 1. The corium was assumed to exist as 0.1 mm (diameter) solid particles. A revised SIMMER-II heat transfer algorithm was used, which vaporizes steam from the water drop surface following contact with a corium drop. No heat transfer from the corium directly to the steam was calculated. These assumptions have the effect of vaporizing all the premixed water within the first millisecond, producing 194 MPa steam at 922 K with a steam volume fraction of 0.69. Adiabatic expansion of the steam then follows.

Corium density profiles for the expansion are shown in Figs. 22 and 23. Initially a spray does develop, then material is decelerated and collects near the head, and finally a smeared shock front delineating upward moving corium from downward moving material develops. Water in the downcomer limits the rate of corium penetration in that direction. The force on the head is shown in Fig. 24. It stays below 1000 MN, even though no venting from failure of the lower head was included in this calculation. Lower head failure should occur, as suggested by the pressure trace at the bottom of the inlet plenum, Fig. 25. The peak kinetic energy produced was about 2500 MJ, which gives a conversion ratio of 2.2%. Although the pressure loading on the upper head was fairly uniform, with some localized values around 80 MPa, the calculation suggests that the partition of energy to the upper head is insufficient for large missile production for this extreme situation.

Finally, we consider head/bolt failure. Here a review of Appendix B must be performed. The relevant points to be made are as follows.

Material properties:

The material properties in Table B-1 were checked by reviewing Refs. 3 and 4. The only significant differences found were the failure strain for the A-533 material and the fracture toughness for the SA-540 bolt material. The differences are important in determining whether a failure bias exists, that is, whether bolt failure or head failure would control the missile characteristics. Ref. 3 shows the total elongation for A-533 at 550 F to be approximately 0.14 while the average total elongation for 12 heats of material reported in Ref. 4 is 0.23. This is compared with the strain at failure given in Table B-1 as

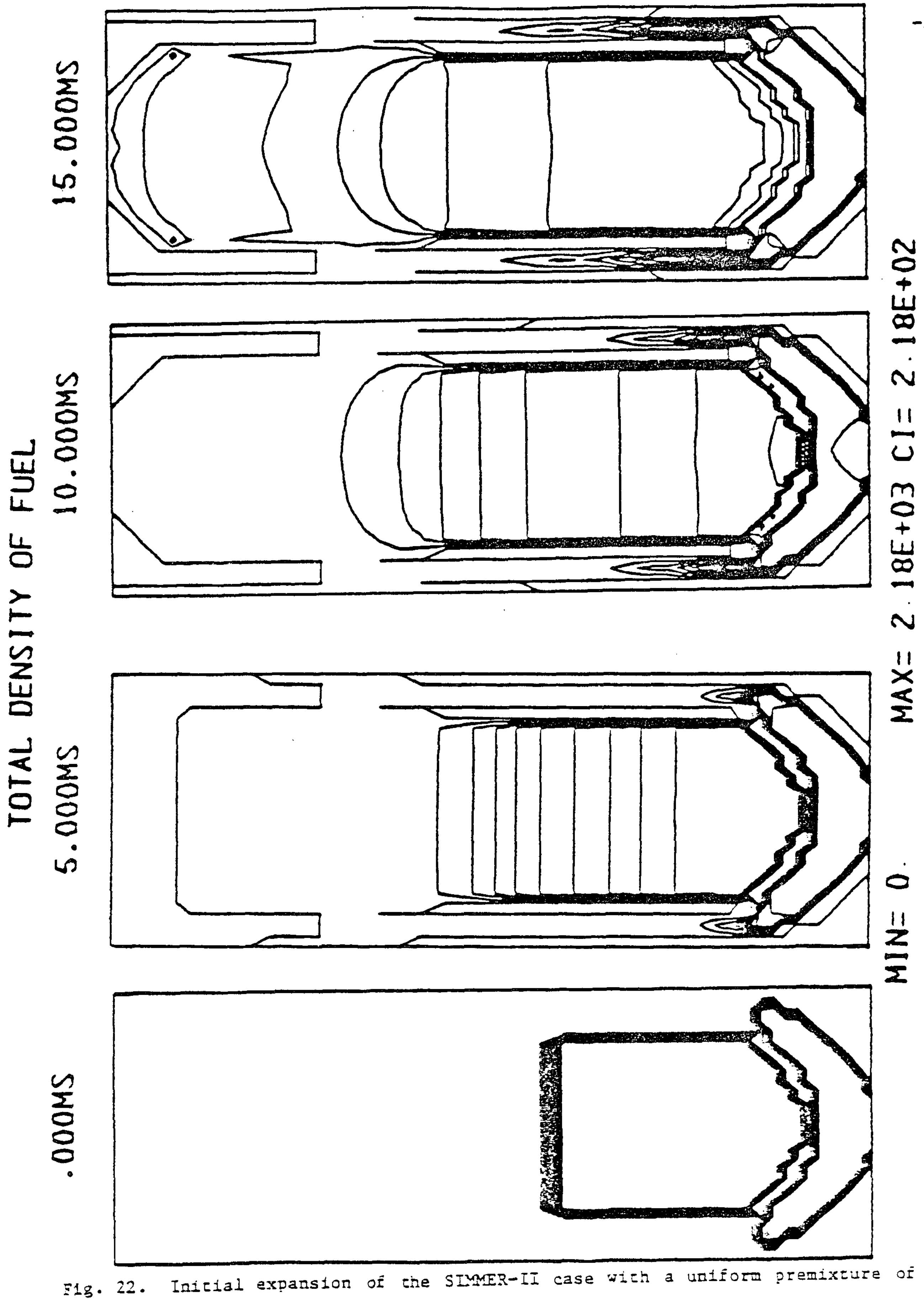


Fig. 22. Initial expansion of the SIMMER-II case with a uniform premixture of corium and water.

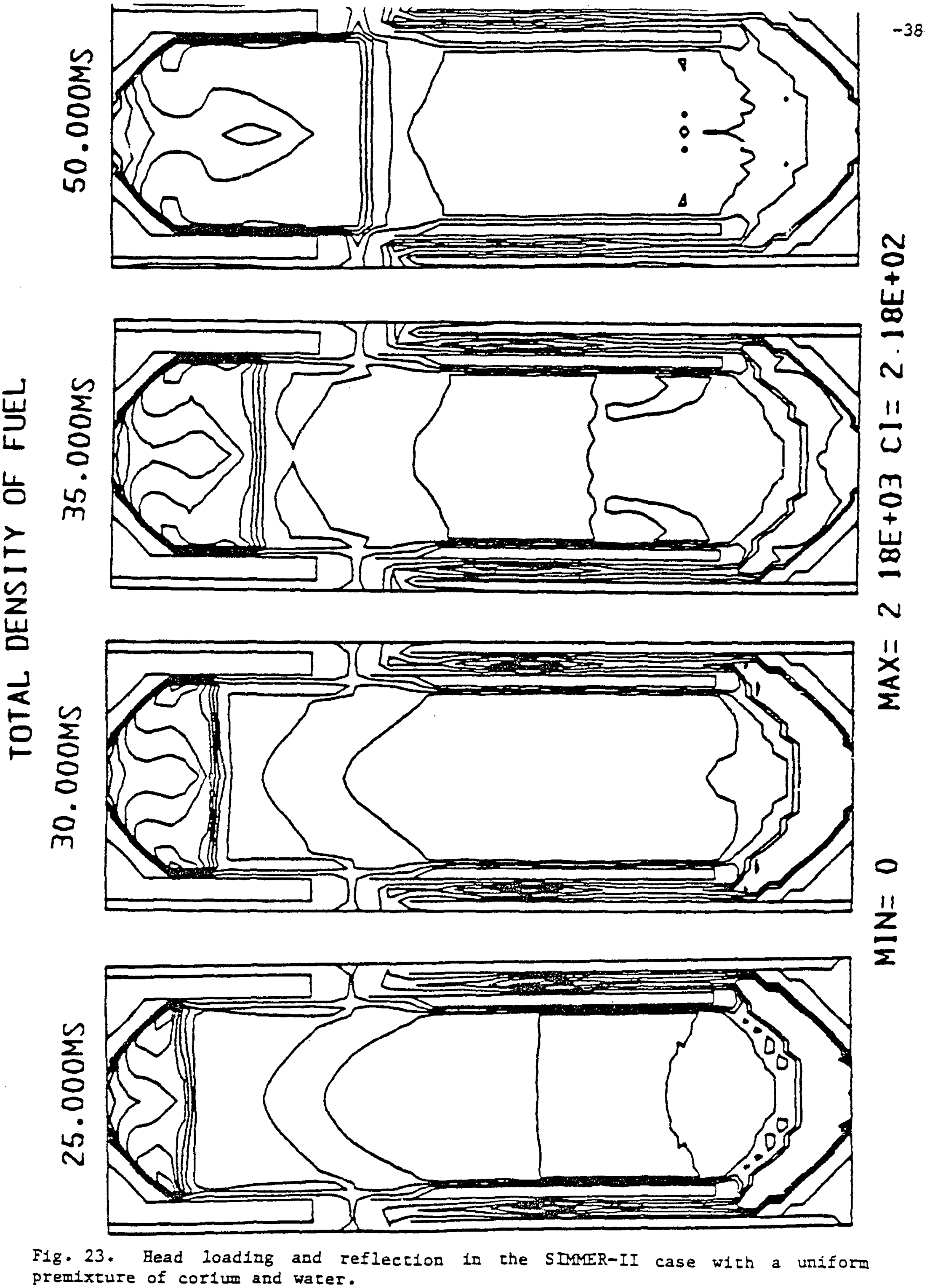


Fig. 23. Head loading and reflection in the SIMMER-II case with a uniform premixture of corium and water.

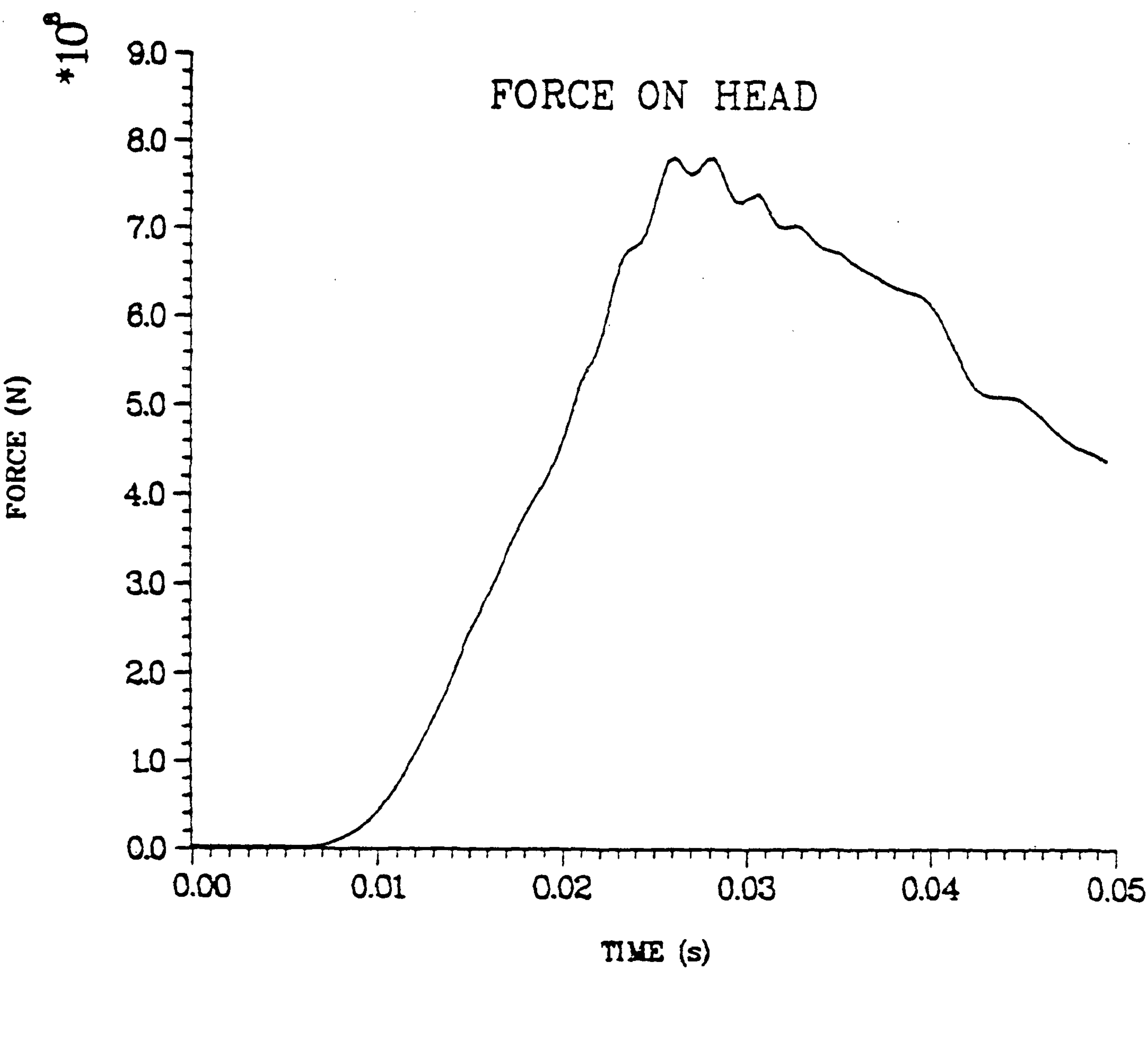


Fig. 24. Head loading forces for the SIMMER-II case with a uniform premixture of corium and water.

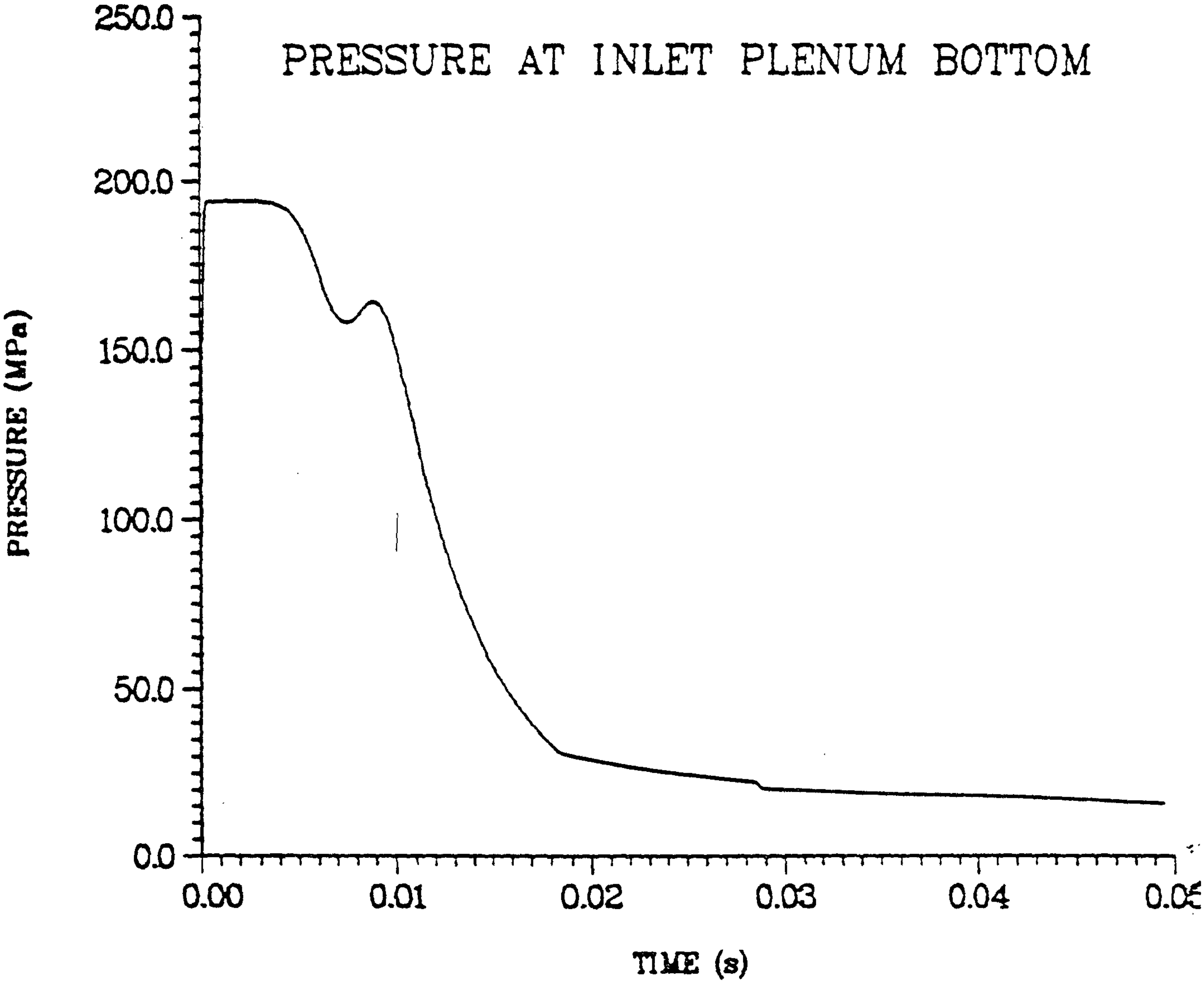


Fig. 25. Lower plenum pressures for the SIMMER-II case with a uniform premixture of corium and water.

0.20. The SA-540 fracture toughness is given as 175 $N\text{-}m^{-3/2}$ in Table B-1. Seventeen data points from four different heats of material were obtained from Ref. 4 and a mean value of fracture toughness was calculated to be 159 $N\text{-}m^{-3/2}$ with a standard deviation of 39 $N\text{-}m^{-3/2}$. Little faith can be put in statistics from such a small sample, but the statistics do show that, as would be expected, the fracture data are widely scattered. The materials properties discussed thus far are for 288 C (the vessel design temperature). If the accident scenario progresses such that the structures heat up before slug impact, higher temperature material properties would need to be considered. Figure 26 shows the trend of yield and ultimate strength as temperature increases. If the curves are extrapolated it can be seen that the yield strength of the SA-540 bolt material decreases faster than that of the A-533 head material. Similar data from Ref. 4 show that the total elongation is not very sensitive to temperature up to 300 C for either material. The data suggest that, even with highly refined dynamic response analysis and highly reliable failure criteria, a considerable range of failure characteristics may be produced by the scatter in material properties.

Failure criteria:

We believe that strain criterion proposed in NUREG/CR-3369 is not the most appropriate for failure analysis. Reference 5 describes a better criterion that has significant experimental verification, including data for A-533 and SA-540 (Fig. 27). This criterion makes use of a triaxiality factor defined by the trace of the stress matrix, or the sum of the principle stresses, divided by the equivalent stress. It is equal to two for a flat plate in biaxial tension, which approximates the stress state in the A-533 head material away from any discontinuities. For a triaxiality factor of two the ratio of the failure strain to the uniaxial fracture strain is approximately 0.3. Note that on the figure a data point representing A-533 steel exists at this triaxiality factor partially verifying the criterion. The criterion proposed in Appendix B of NUREG/CR-3369 results in an effective plastic failure strain of 0.18 while the criterion of Ref. 5 results in an equivalent plastic failure strain of 0.12 for a uniaxial fracture strain of 0.20 and 0.08 for a uniaxial fracture strain of 0.14. Changing the head failure strain to either 0.08 or 0.12 results in a somewhat different picture than presented in Fig. B-6 of NUREG/CR-3369.

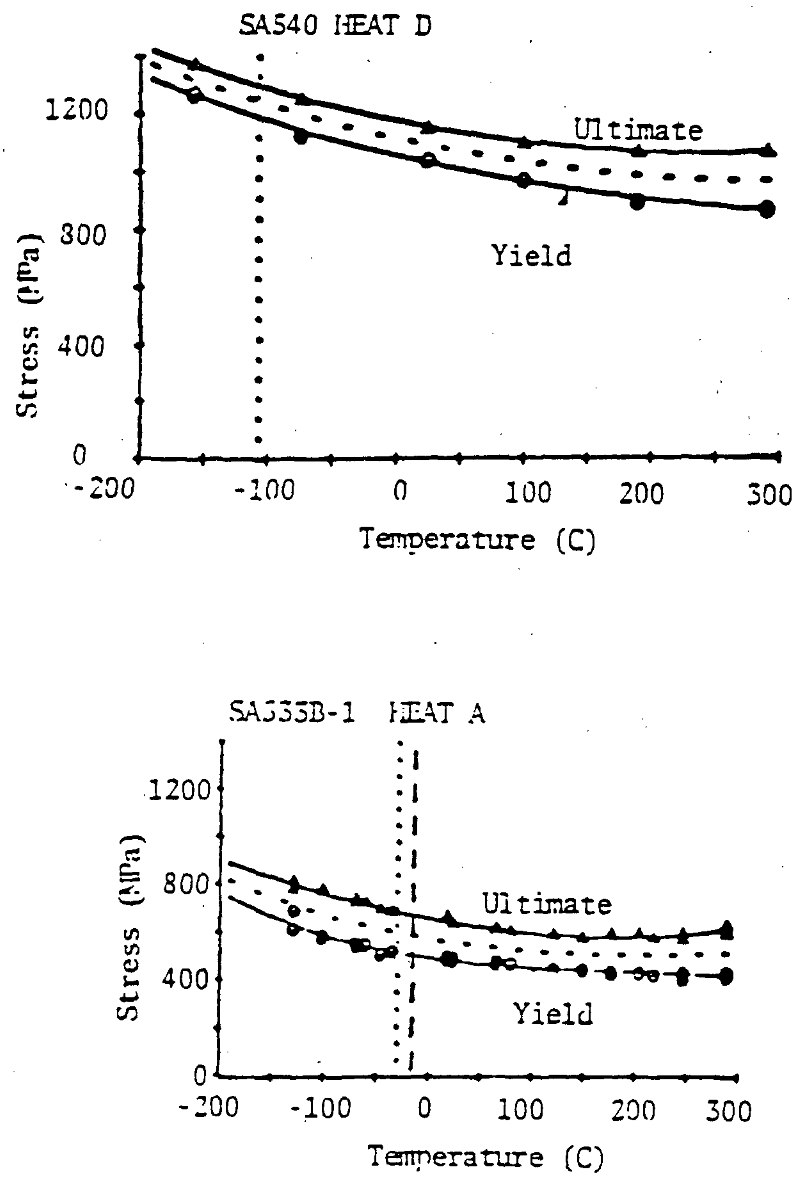
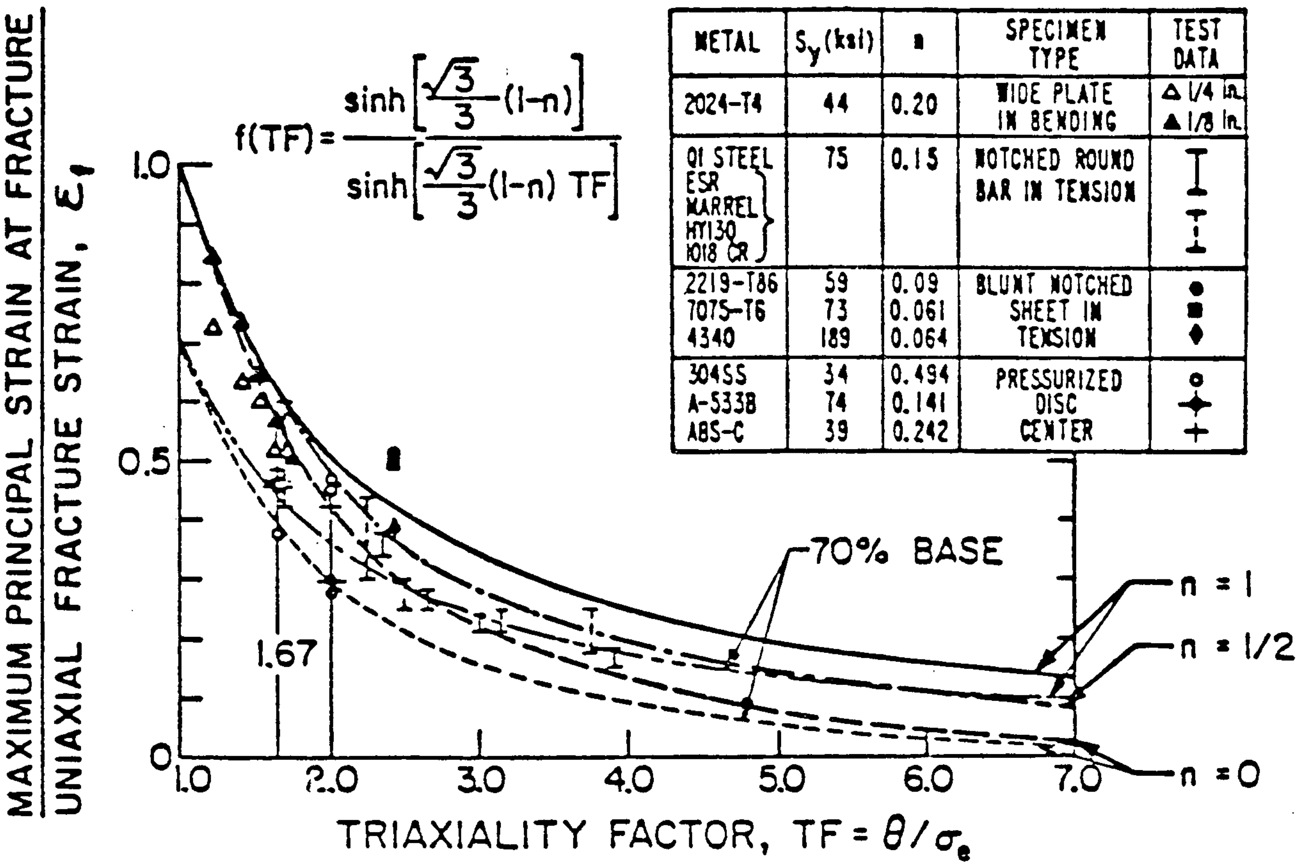


Fig. 26. Material strength variation with temperature.



METAL	S_y (ksi)	n	SPECIMEN TYPE	TEST DATA
2024-T4	44	0.20	WIDE PLATE IN BENDING	△ 1/4 in. ▲ 1/8 in.
01 STEEL ESR MARREL HY130 1018 CR	75	0.15	NOTCHED ROUND BAR IN TENSION	
2219-T86 7075-T6 4340	59 73 189	0.09 0.061 0.064	BLUNT NOTCHED SHEET IN TENSION	● ■ ◆
304SS A-533B ABS-C	34 74 39	0.494 0.141 0.242	PRESSURIZED DISC CENTER	○ +

Fig. 27. Strain failure criterion.

The criterion used for brittle fracture of the bolting material is acceptable except for the assumption that design flaws exist in all of the bolts. Considering the requirements for inservice inspection (Ref. 6), it is highly unlikely that very many significant flaws will exist and, if they do, they wouldn't occur in all of the bolts. Using a more realistic assumption of flaw depth equal to 0.08 in. (thread depth), the average stress needed to fail a bolt by fracture is 979 MPa. This is considerably higher than the average yield stress (892 MPa).

Numerical model:

The numerical finite element model used to predict structural response is adequate if two approximations built into it are considered. The first is that no structural discontinuities in the head structure are modeled. This approximation leads to overpredicting the head strain capability. The second approximation involves the boundary conditions on the model. Not including the complete mass of the vessel or modeling its actual support system will lead to overpredicting stresses and strains in the head area, including the bolts. In terms of momentum exchange, a considerable amount of momentum will go into upward motion of the complete vessel when the slug impacts the head. The model used in NUREG/CR-3369 does not account for this factor.

Results:

Plotting the new value of fracture stress on Fig. B-4 (Fig. 28) and realizing that the yield stress is 892 MPa, leads on to the conclusion that the bolts will probably yield before they fracture. If the bolts yield before brittle fracture, they would fail through plastic strain. The significant point these facts lead to is that, for loads severe enough to fail the bolts, the time to fail all of the bolts is much longer than if they all failed simultaneously in a brittle fashion. This allows more energy to be deposited in the radial deflection of the head leading to the potential of failure through plastic strain whether the bolts fail or not. If the loading on the head is concentrated toward the center, it will fail at this location before the bolts all fail.

For the uniform loading assumed in NUREG/CR-3369, whether the bolts or the head itself would fail first is difficult to determine and will depend heavily on load level and distribution. Referring to Figs. 28 and 29 of this review, for an 80 MPa step loading some of the bolts would fracture in a brittle fashion at approximately 2 ms. Calculations from a one-degree-of-freedom head model show that if one third of the bolts fracture at this time, enough strain would be accumulated in the remainder to cause them to fail at approximately 6 ms. In the meantime, the effective plastic strain in the vessel head material will have reached the failure strain of 0.12. The failure strain will probably be reached sooner at discontinuities where strain concentrations will exist. It is likely that even if the bolts fail first, there would be enough momentum in the radial direction in the head material that it would also fail. This could lead to multi-body impact dynamics with the missile shield, polar crane, and containment wall and possibly change significantly the ability of large steam explosions to fail containment. Considerably more work is required to ascertain if head failure and breakup is likely when considering uncertainties in material properties, failure models, and loading characteristics.

B. Comments on the usefulness of the SNL methodology

The next comments address the methodology presented in terms of performing a risk assessment. In brief, we believe this type of analysis is useful for highlighting research needs, but of less value for giving probability numbers. It is intuitively obvious to a casual observer that the conditional probability for containment failure must be between 0 and 1. To have value, the calculational approach should hold the promise of reducing that range. It is not clear how the NUREG/CR-3369 approach possesses that promise although we realize that the report was not intended to give probabilities but rather examine uncertainties.

There are stochastic aspects to the steam explosion question. Considering the spectrum of nuclear plant designs and potential accidents, essentially all potential core melt scenarios can be expected to possess differing initial conditions, boundary conditions, and forcing functions as a consequence of operator response. The steam explosion process itself seems quite sensitive to the conditions under which it occurs. Although some of this sensitivity as

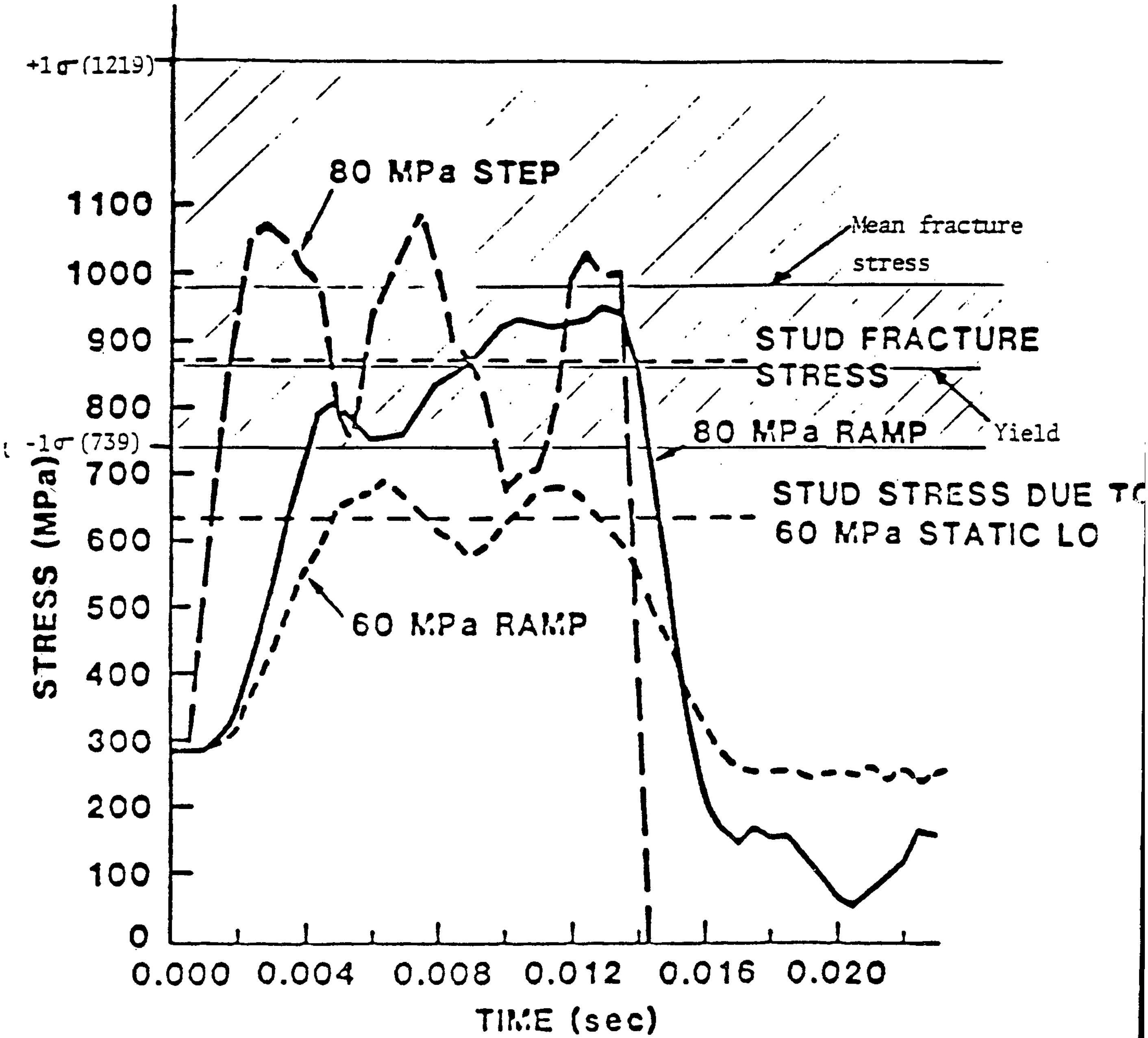


Fig. 28. Failure of vessel closure bolts.

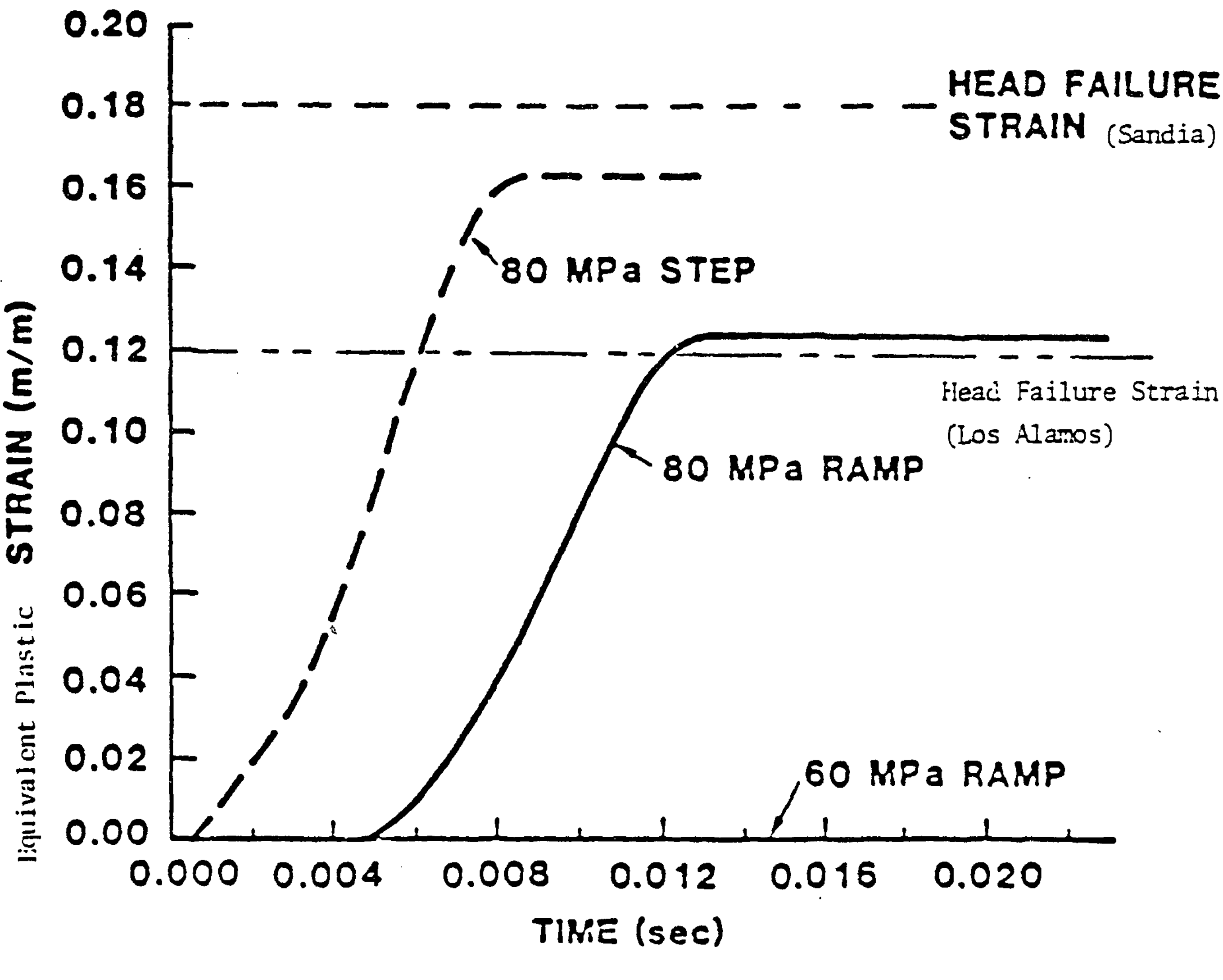


Fig. 29. Failure of vessel head through plastic fracture.

observed in SNL experiments is probably a consequence of randomness in the thermite preparation-delivery process, other vapor explosion experiments have also shown significant data scatter. Consequently, in constructing a model to simulate experimental steam explosions, we must deal with a mathematically ill-posed problem, or a situation where the results depend discontinuously on the initial conditions.

These stochastic aspects do not fit easily into theoretical arguments concerning the shape of the probability density functions. In other words, the stochastic aspect of steam explosions is unlike the situation in, for example, neutron cross section evaluation where the shape of the basic distributions for unresolved resonances can be based on theoretical arguments from quantum mechanics. Here the distributions are subjective. Experimental information can logically be expected to change the range of the probability density functions, but not their shape.

With these realizations concerning the nature of random behavior in the steam explosion phenomenon, the use of flat distributions is reasonable. Possibly the use of a logarithmic abscissas about median values might be justified for achieving output probability distributions; however, within the scope of the present methodology, we believe it is very difficult to justify other distribution shapes for performing additional calculations. If we now are to have concerns regarding the consequences of sampling from the upper thirds of these distributions, the problem essentially reduces to that of finding an acceptable conservative upper bound.

Therefore, we submit that what the NUREG/CR-3369 study implies is (a) that this methodology is insufficient for a risk assessment, and (b) that a rigorous upper bound does not yet exist on the steam explosion issue.

Other methods currently in use for determining probability numbers regarding steam explosions are of course subjective, and the Los Alamos steam explosion program is investigating the upper bound question. Additional research is required if a more justifiably objective method is to be developed for determining the probability of accident consequences, or if additional confidence must be obtained regarding upper bound steam explosion consequences.

C. Further detailed remarks on NUREG/CR-3369

In this part of our response to question 2, we provide comments on each section of the report for the purpose of highlighting additional or alternative considerations that should be addressed in the deliberations of the NRC expert group. We raise points for which there may not be definitive answers or definitive answers may not be required. We hope that paths can be found to address the points, if necessary, and that path can be found for decreasing the degree of subjectivity in addressing the entire issue.

Section 1.1 Background: It is somewhat disappointing that despite all the work since WASH-1400 and particularly since the TMI accident, even the initial phases of geometry disruption are judged by NUREG/CR-3369 to be "highly uncertain." For example, we believe the case for formation of a crust is a reasonable best estimate. Also, the statement that containment failure resulting from a big steam explosion may be possible 1 hour after accident initiation tends to focus attention on a time scale that may be overly short, except for the consideration of a large break LOCA as analyzed in WASH-1400.

Section 1.2 Previous Assessments: The brief review of other studies seems reasonable, although do all the opinions deserve equal weight?

Section 2.2 Monte Carlo Method: Some combinations of the independent variables seem unreasonable, such as mixing all the fuel with all the water. A "TILT" option appears desirable? Also, sometimes the use of the probability concept becomes confusing. Surely the "probability" computed with complete knowledge on an individual accident is different from the subjective probability described in Appendix A of NUREG/CR-3369.

Section 3.2 Fraction of the Core Molten: Some upper structure could melt into or join a postulated molten pool. A developing molten pool may contain more than simply core material.

Section 3.4 Pour Length (or Trigger Time): It is regrettable that solid arguments could not be found to limit the pour diameter to less than the full core value.

Section 3.5 Mixing Limitations: The limits to mixing is probably the key section of this document. If extensive mixing can occur, a high conversion ratio may be possible in a large system. Then the simple mechanistic models used by NUREG/CR-3369 predict containment failure (as modified by the condensed phase volume fraction, which is of limited effect in the model). We find it disturbing that the problem is felt to be only addressable by bigger experiments. Could not at least an analytical plausibility argument be presented showing the reasonableness of the extreme case of total mixing with the pour diameter being the full core diameter? This situation is almost one-dimensional with the water escape path being up the downcomer.

Section 3.7 Conversion Ratio: The conversion ratio may not be a very good concept for a large scale system, with the range of melt/water compositions expected to be present and the fact that even non-explosive energy transfer rates can lead to high kinetic energies with inertial confinement. The limited experimental data available certainly favors at least locally more efficient explosive interactions when confinement is present.

Section 3.8 Heat Content of Molten Fuel: The heat content of corium debris may be a stronger function of temperature than NUREG/CR-3369 assumes. For example, Ref. 7 gives a solid uranium-dioxide heat capacity as shown in Fig. 30. The value from 2670-3120 K is 167 J/(mole-K) or 618.5 J/(kg-K). The heat capacity for the type of molten stainless steel used in LMFBR analysis is 750 J/(kg-K) or greater. If high temperature melts are possible, high heat capacity at high temperatures could make a difference in explosion characteristics, disproportionately increasing the energy transferred.

Section 3.11 Energy Dissipation by Bottom Failure: Previous SNL work as well as calculations at Los Alamos have suggested what type of pressure-time history would be required for lower plenum failure. If a big steam explosion occurs, as defined in the response to question 1 above, it can be shown with a simple hand calculation that the bottom head will fail through plastic strain if

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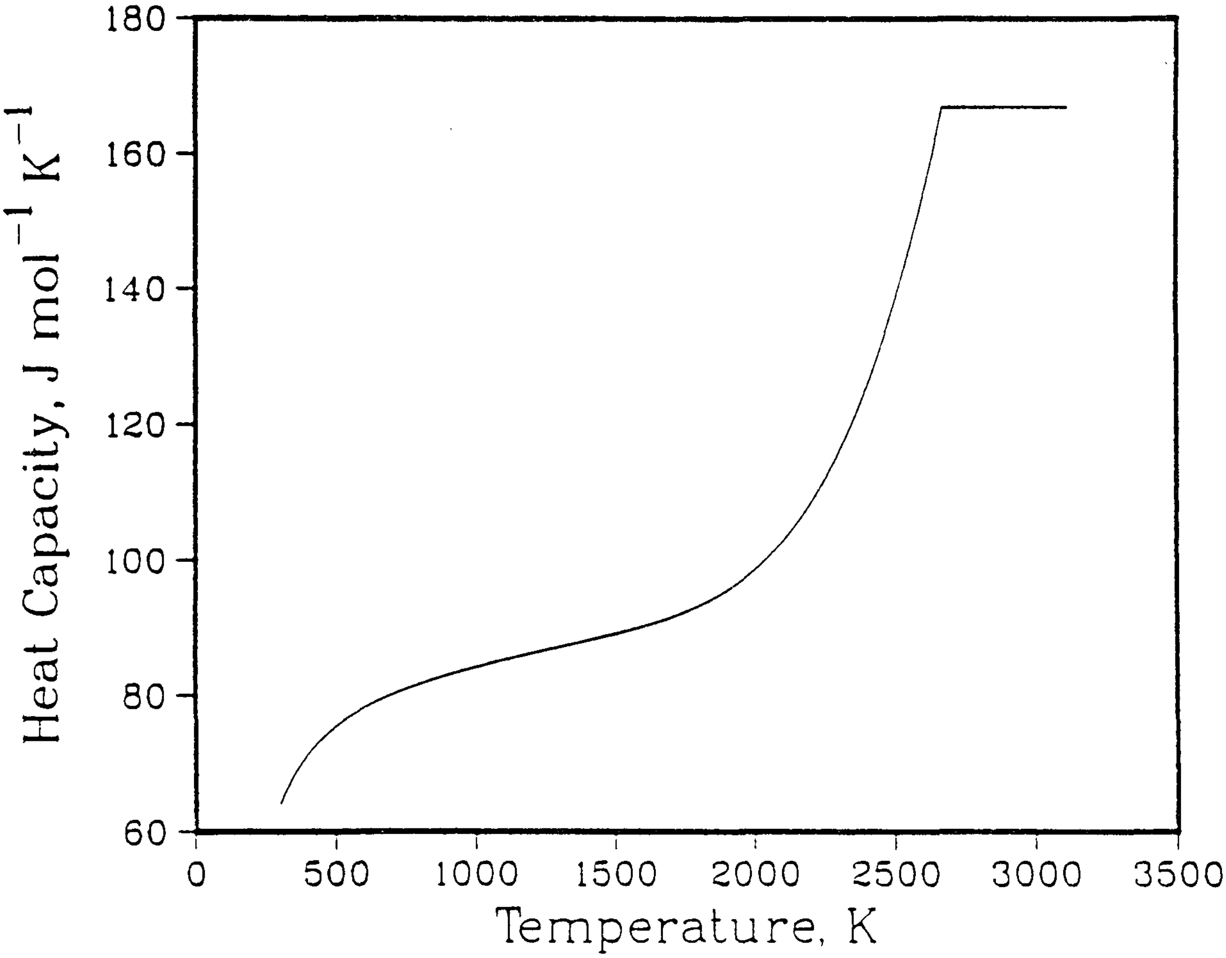

Fig. 30. The heat capacity of solid UO_2 from 308.15 K to the melting point, 3120 K.

0.125 MPa-s of specific impulse is delivered to the inner surface during the first 2-3 ms of the loading history. These calculations have been confirmed with a detailed finite element model of the bottom head. The SNL use of a 1000 MJ threshold seems reasonable, but neither the NUREG/CR-3369 study nor the previous SNL Monte Carlo study justifies this number. The formula for the upward and downward partition of kinetic energy may be a reasonable first approximation if an upper slug exists. The model does have the potential for a bias with the energy partition being on the basis of total mass, while bolt failure is based on energy per unit mass of the upward moving slug. This bias is probably small given the energy requirement for lower head failure. Further reduction in the potential for unrealistic cases might be achieved by adding to the upward moving "slug" some of the remaining solid core material or having an inelastic collision with the UIS? Also, the mass in the failed part of the lower head could depend on the loading and failure mode, and sideways venting of the driving pressures may be more important than assumed. This is under investigation in the Los Alamos program.

Section 3.12 Slug Composition: The slug volume parameter appears to model potential mitigation as a consequence of slug breakup. The range appears reasonable based on SIMMER-II calculations with postulated nonuniform interfaces. As suggested previously, a logarithmic abscissa might be more logical, i.e., a median value of 0.50 with a factor of 2 uncertainty in either direction.

Section 3.13 Energy Dissipation by Core and Upper Internal Structure: We agree that energy dissipated as plastic strain energy in the UIS is negligible for slugs that initially have enough energy to fail the vessel head. However, if the impact is simply modeled as an inelastic collision, momentum will be conserved and the kinetic energy can be reduced considerably depending on the relative masses. Also, there might be some effect on the momentum and energy from remaining solid core material.

Section 3.14 Slug Impact Model: Calculations performed at Los Alamos do not lead to the conclusion stated here that the loading can be assumed to be static. Loading rise times on the order of 10 ms are experienced. The natural period of the structure before yielding occurs is approximately 5 ms. When yielding occurs this increases by an order of magnitude.

Section 3.15 Containment Failure: Some comments on this point have been made in response to question 1 involving the missile shields and the polar crane. Additionally in NUREG/CR-3369 it is stated that the head loses 3/4 of its kinetic energy during a inelastic impact with the missile shield. The missile shield is not heavier than the head, but instead weighs less than 1/2 of what the head with attached equipment weighs. Using a weight ratio of 1/2 we calculate that the kinetic energy of the head/missile shield combination after impact is 2/3 of the initial kinetic energy of the head alone.

The use of standard missile penetration formulae for analyzing the effect of the head on the missile shield and containment dome is not reliable. These formulae were originally generated to study the penetration of military projectiles through concrete walls and have been successfully extended to investigate the effects of tornado-born missiles (Ref. 8). Tornado-born missiles (for example, telephone poles) are generally much smaller than the missile head structure. Similar work has also gone into investigating the hazards from missiles generated during accidental detonations of high explosives (Ref. 9). Again, the missiles considered here are much smaller and, in all cases, the ratio of the missile contact area to the target structure is small. In trying to extrapolate one of the plots in Ref. 9, it was discovered that the data point for the vessel head traveling at 50 m/s was too far off the plot to consider the results reliable. One extrapolation did show that when the head strikes the concrete missile shield, spalling but no perforation would occur.

Other problems involved with using the published penetration formulae include the material properties of the missile (mild steel versus concrete) and the fact that relationships such as the NDRC formula apply only to "hard" missiles. A more appropriate method for analyzing the effect of the missile on the containment wall is to look at the nonlinear structural dynamics of the system with a structural computer code. Such an approach has been used by the Europeans to investigate the problem of containment failure from impact by high speed aircraft.

In summary, several aspects of potential containment failure not considered in NUREG/CR-3369 should be addressed. The more important are to include the effect of the ever-present polar crane and to use more appropriate methods for estimating damage velocities. The most damaging geometry to containment in terms of penetration may be the vessel head with some small diameter and "hard" equipment on top of it contacting the containment wall. This possibility needs to be considered for those cases where the polar crane does not stop the missile.

Section 3.16 Summary of Modeling: The summary of modeling is good. There is one typo in the initial kinetic energy of the top head. The coefficient of 2 in the denominator should be an exponent of 2? Further justification for this type of energy partition could also be given.

Section 4. Calculations and Results: The sampling procedure does a reasonable job of finding the important parameters in the model—the pour diameter, pour length, conversion ratio, and the melt heat content. Its orientation towards merely demonstrating uncertainty is less useful. It is interesting that in sampling from the full width of the distributions, the probability of failing the containment is about $10^{-1.5}$. This is within the range of probabilities of having a big steam explosion in our response to question 1, despite the lack of credit assigned by NUREG/CR-3369 to mixing limitations. Again, we believe that the model used by NUREG/CR-3369 exaggerates the probability of containment failure given that a big steam explosion occurs.

Section 5.1 The Effects of High Pressure: A review of models on the effect of high pressure would be desirable, as well as the presented review of experimental information. It is difficult to conceive that funding for large scale tests at high pressure will become available in today's environment.

Section 5.3 Multidimensional and Geometric Effects: The discussion of multidimensional and geometric effects again reveals a key philosophy problem with NUREG/CR-3369. This is the implication that a phenomenon must be experimentally investigated before any mitigative credit is allowed. This philosophy assures unresolved issues, if the prototypicality problems present in any experimental program are noted.

Section 5.4 The Effect of Correlations: We disagree on the conclusion regarding correlations. Proper inclusion of correlations would be almost guaranteed to reduce uncertainty by reducing the range to be considered on some of what are now random variables.

Section 6. Discussion: If high ambient pressure and the failure mode of the vessel top head are key contributors to the uncertainty, why not include them in the Monte Carlo study?

We have no comments on Appendix A. Comments on Appendix B were given previously in the discussion of head/bolt failure. Overall, we regard NUREG/CR-3369 more as a statement of the consequences of a particular subjective view of initial conditions and physics uncertainties, rather than a cause to claim that new concerns should exist. It is always possible to make unrealistic assumptions that are sufficiently conservative that containment integrity becomes questionable. If the fuel and water are thermally equilibrated before the expansion in the calculation previously considered in the response to this question, the low vapor fraction of the coarse premixture leads to a mixture temperature of 1482.5 K and an initial pressure of 903.1 MPa, using the Los Alamos SESAME water properties and assuming the corium to be incompressible. Because a large, highly constrained system could lead to efficient heat transfer, constant volume temperature equilibration does become more plausible with such initial conditions. If the bottom head does not fail, preliminary estimates would be for fairly uniform upper head loading pressures in excess of 300 MPa.

Some additional considerations on this case are as follows: After initial equilibration the high pressure "steam" volume is about 30 m^3. An isentropic expansion of this steam by an additional 80 m^3 will reduce the density from 680 kg/m^3 to 185 kg/m^3. The relevant isentrope was computed using the SESAME tables. The change in internal energy, or 0.73 MJ/kg, can be equated to the work done along this path. With 20,300 kg of steam, the work done is 14,800 MJ. This is a potential conversion ratio of 0.13 for the 94,000 kg of corium, assuming no heat transfer during the expansion. Of course, this case should be considered in the context of lower head failure. As a first approximation, we can assume that with the extra support given to the lower head in the region of the vessel penetrations, vessel failure will occur where the penetrations stop, in other words, the core radius. A preliminary SIMMER-II calculation with this assumption only reduces the magnitude of upper head loading pressures by about 10%, as a consequence of the extent of the large premixed zone limiting communication with the lower head release path.

Consequently, if the Monte Carlo approach used in the NUREG/CR-3369 study is to eventually prove more useful in reducing any residual concerns on the alpha mode failure issue, we cannot allow the uncertainties in how water and fuel might mix to force the conclusion that mixing can be completely arbitrary. The use of NUREG/CR-3369 for guiding further research is thus meaningful only _if_ our level of confidence in existing arguments concerning mixing is insufficient.

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COMMENTS ON PROPOSED RESEARCH CONTRIBUTING TO THE RESOLUTION OF RESIDUAL STEAM EXPLOSION ISSUES

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T. A. Butler

SUMMARY

This report addresses question three asked by Denwood Ross in his letter of August 24, 1984.

The question is as follows:

Review and comment on SNL's proposed research program. In particular:

(i) Provide your opinion as to whether these experiments or other experiments and/or analyses will contribute to resolution of residual issues and to a further reduction of uncertainties.

(ii) Provide your recommendations on pre-experiment analyses or experimental procedures which will maximize the usefulness and effectiveness of the tests.

Our reply covers several areas.

1. The program proposed by SNL covers broader considerations than direct containment failure from a missile produced from a steam explosion. Our comments will be limited to the alpha-mode failure issue and specifically the SEALS and SHIP tests.

2. There are uncertainties. A key uncertainty is the type of scientific evidence required to resolve a degraded core issue, such as the question regarding the probability of alpha-mode failure. The SNL proposal does not truly offer an integrated plan combining analysis, experiments, and code development. For example, the proposed SEALS tests are to be run at 0.083 MPa. Some algorithm must eventually exist to extrapolate to large-scale reactor vessel pressures under accident conditions where only smaller-scale test results are available. The idea being proposed by SNL seems to be to list the important phenomena and perform exploratory experiments in which these phenomena will be present to some degree. Such an approach has not worked well in the past. We

might anticipate confusion will still exist after performing these new test series particularly from the standpoint of the controlling physics and the extrapolation of the results to the reactor situations.

3. We see the Los Alamos program as providing an additional means of obtaining increased confidence on a probability estimate of containment failure by a steam explosion, given core melt. The thrust of the Los Alamos program is on the post-explosion expansion phenomena, with further investigation of the thesis that only physically unreasonable steam explosions can lead to a containment challenge. An example calculation is provided in this report.

4. Initial conditions are required in the analysis of post-explosion expansions. With reference to Fig. 1 of our response to question 1 of the Ross letter, we are examining whether the probabilities below the big steam explosion box are so low that the only way they could be appreciable is to more restrictively define a big steam explosion. With such a redefinition, a big steam explosion might take on a 1/1000 or less probability. Large scale tests could be beneficial for obtaining increased confidence in our judgments concerning explosive configurations, if further progress toward that goal is believed necessary. They could be used for partial justification of a 1/1000 probability estimate on achieving a truly large-scale explosion. Available SIMMER-II calculations suggest that with 2000 kg of melt, useful experimental results might be obtainable on the extent of coarse premixing and the effects of confinement. These calculations also suggest the potential for a significant "explosion". If a large explosion does not occur with 2000 kg of melt, pessimistic assumptions could be relaxed in application to the reactor case.

5. Regarding experimental analysis, if large-scale experiments are to be performed, a best-estimate computational capability should be developed. Both SNL and LANL have programs that can contribute in this area. These programs should be accelerated, and analyses performed. This would help insure that the correct test conditions are selected (related to reactor geometry and initial conditions) and that the correct interpretation would be made of the tests. Also, uncertainties would be reduced in extrapolation to reactor scale (consistent physics would be applied). Regarding experimental procedures, our 1980 Zion/Indian Point study suggested that if a significant degree of coarse

premixing could occur, for example 10% to 20% of the core, the inertial constraint of the remaining fuel reduced sensitivity to heat transfer assumptions. Experimental support for this idea could reduce modeling difficulties and the level of understanding believed to be required for making decisions on alpha-mode failure issues.

6. Other comments and recommendations concern scaling, the conditional nature of SEALS3, analysis of tests with the CSQ code, performing SHIP tests, limiting large scale tests to water, the possible use of gamma- or x-rays for diagnostic information in SEALS1, and essential nature of energy quantification.

Each of these areas is treated in subsequent sections. We conclude that if an overall plan existed for resolution of steam explosion issues, large-scale tests should be considered as part of that plan. An overall plan was not referred to by the SNL proposal, which is ambiguous as to the value of the information that we can expect from these tests. Consequently, we must end with some questions. Will the information obtained from the SEALS and SHIP tests specifically resolve issues that are crucial to the reactor steam explosion concern? What are the backup approaches if current mixing theories that produce limited explosions are proved inadequate? What if the data indicates highly variable and unpredictable results? Is extrapolation to the reactor then possible?

1. Observations on Scope

We are under the impression that the SERG is to focus on the alpha-mode failure issue. The SEALS facilities do this, by addressing questions of scaling effects on coarse mixing, confinement, and the conversion ratio. The SHIP facility is also relevant, being focused on a very narrow question, the triggering of a single drop at high pressure. The FITSX and ELVIS facilities both appear to possess a broader charter, examining such questions as steam and hydrogen generation, debris characteristics, the characterization of ex-vessel steam explosions, and fission product release. In these comments, we will limit our remarks to the SEALS and SHIP facilities. Justification of FITSX and ELVIS, as currently proposed, should come from concerns regarding those features of the accident progression that these facilities address.

2. Uncertainties

The required technical information for achieving closure on various degraded core issues, such as steam explosions, is not well defined. Our former ANS president, Milt Levenson[1], apparently believes as an article of faith that operating reactors are inherently safe. No further technical support is required. The NRC tends to favor an approach that attempts to build a consensus of independent expert opinions, such as is to be obtained from the SERG. The IDCOR[2] report on explosive steam generation rates presents the position that simple models can represent the dominant phenomena, and are sufficient to assure physical incredibility. In the LMFBR area, simple postulated models have been interconnected in large computer codes. In 1983, such code calculations[3] were argued to provide sufficient support for resolving concerns on CRBR accidents beyond the design base. Similar code construction is underway for the LWR degraded core area. We interpret the SNL philosophy to be the obtaining of a convincing experimental demonstration before any issue of concern can be considered resolved.

Each of these approaches has its problems. For example, the SNL critique on IDCOR's models makes several worthwhile observations on the excessive optimism built consistently into the IDCOR assumptions. However, despite the fact that the level of evidence required is subjective, direct containment failure by a missile produced as a consequence of a steam explosion is generally felt to be highly unlikely. Research activities are thus directed at improving the level of confidence in this judgment.

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[1]Nancy Zacha Godlewski, "Levenson—Improving Technical Credibility," Nuclear News, 26, #9, p 78 (July 1983).

[2]IDCOR Technical Report 14.1A, "Key Phenomenological Models for Assessing Explosive Steam Generation Rates," Fauske and Associates, Inc., June 1983.

[3]T. G. Theofanous and C. R. Bell, "An Assessment of CRBR Core Disruptive Accident Energetics," Los Alamos National Laboratory report, NUREG-CR-3224, LA-9716-MS (1983).

Both the SEALS and SHIP tests potentially can increase our confidence. SEALS could demonstrate a scaling limit to coarse premixing at or before 2000 kg of thermite is reached, and the SHIP tests could show enormous difficulties to triggering at 17 MPa. The problem is that SEALS results are not assured, as we will suggest in Sec. 4, while conclusions from SHIP tests may face scaling questions, if triggering results are in any way ambiguous. At the level of analysis presented in the SNL proposal, the SEALS experiment can be interpreted as dropping the thermite into water to see what happens. If negative or ambiguous results are obtained, problems then exist on how much these results have been affected by non-prototypic effects, how to extrapolate to reactor scale, and what we now should conclude as to the required information to "resolve" the alpha-mode failure issue. Our suggestion is that there is insufficient evidence on how these experimental proposals tie together into an integrated package. In the last few years we have observed an increasing level of confusion from the FITS test results with surface explosions, multiple explosions, ambiguous explosions, alternating contact mode explosions, rigid container effects, water subcooling effects, and other random test results. Even the relevance of thermite as a corium simulant is presently under question. For example, does it stratify, is it prefragmented, or are its physical properties unrealistic compared to corium? Potentially adding another level of confusion will not increase our confidence. A effort must be made to understand clearly the role these tests are to play in resolving the alpha-mode issue and to generate some assurance that the role will be accomplished.

3. The Los Alamos Steam Explosion Program

The Los Alamos steam explosion program is based on the acceptance of broad uncertainties in the understanding of the magnitude of any steam explosion, given core melt. Assuming that a large steam explosion occurs, we evaluate the consequences on the reactor vessel, and the consequences on the containment integrity should a large missile be produced. However, our main orientation is to examine whether a large upwardly directed missile is possible. A special version of the SIMMER-II code is used for the fluid dynamics. Modifications have been inserted for heat transfer, vaporization/condensation, and the equation-of-state to better treat water. A lower head failure model with movable structure has been inserted to represent downward pressure relief.

Comparison to shallow pool acceleration experiments is used to examine the reasonableness of upwardly-directed fluid kinetic energy. The current status of the Los Alamos program can be explained with reference to an example calculation used in our response to question 1 of the Ross letter. This is a steam explosion triggered at 0.7 s following a calculated "edge-of-spectrum" pour of molten corium into the lower plenum water.

Four modifications were made for this run relative to the case presented in our November 2 letter. First, a model for lower head failure was inserted into SIMMER-II. Finite element calculations indicate that failure can be expected at the radius where the outermost vessel penetrations are located. A circumferential split would be expected to proceed around the head at this radius and leave the inner portion of the head as a free body. A simple,single-degree-of-freedom spring-mass model was correlated to these finite-element results. This model accepts the average pressure loading over the failure region, and furnishes the time and downward head velocity when the head disengages from the vessel. Second, a model for treating the failed head as a moveable free body was inserted. The force driving the head downward is integrated over the moving surface and the net acceleration is computed; the position of the head then acts as impervious but moving structure to the fluid motions. The fluids are permitted to escape through the peripheral separation area to the reactor cavity. Third, steel particles were inserted to represent the upper internal structure (UIS). Although the strength of the UIS may not be significant if large amounts of upwardly directed fluid kinetic energy are present, the mass and inertia of the UIS were believed to be desirable to include in a post-explosion calculation. Fourth, an oversight in the water drop size was corrected. This has limited effect because of the inertia of the corium.

A 15 by 66 node mesh was used for this calculation, as is shown in Fig. 1. The space below the vessel was sized to include the space in an equivalent 18 foot diameter cavity, with the cavity extending from the vessel supports to the surface of an assumed 5 feet of water in the bottom of the cavity. A continuative inflow/outflow boundary condition was inserted in the bottom cavity node to allow some escape to the keyway. Similar boundary conditions were applied in the ends of the inlet and outlet piping simulations. Other

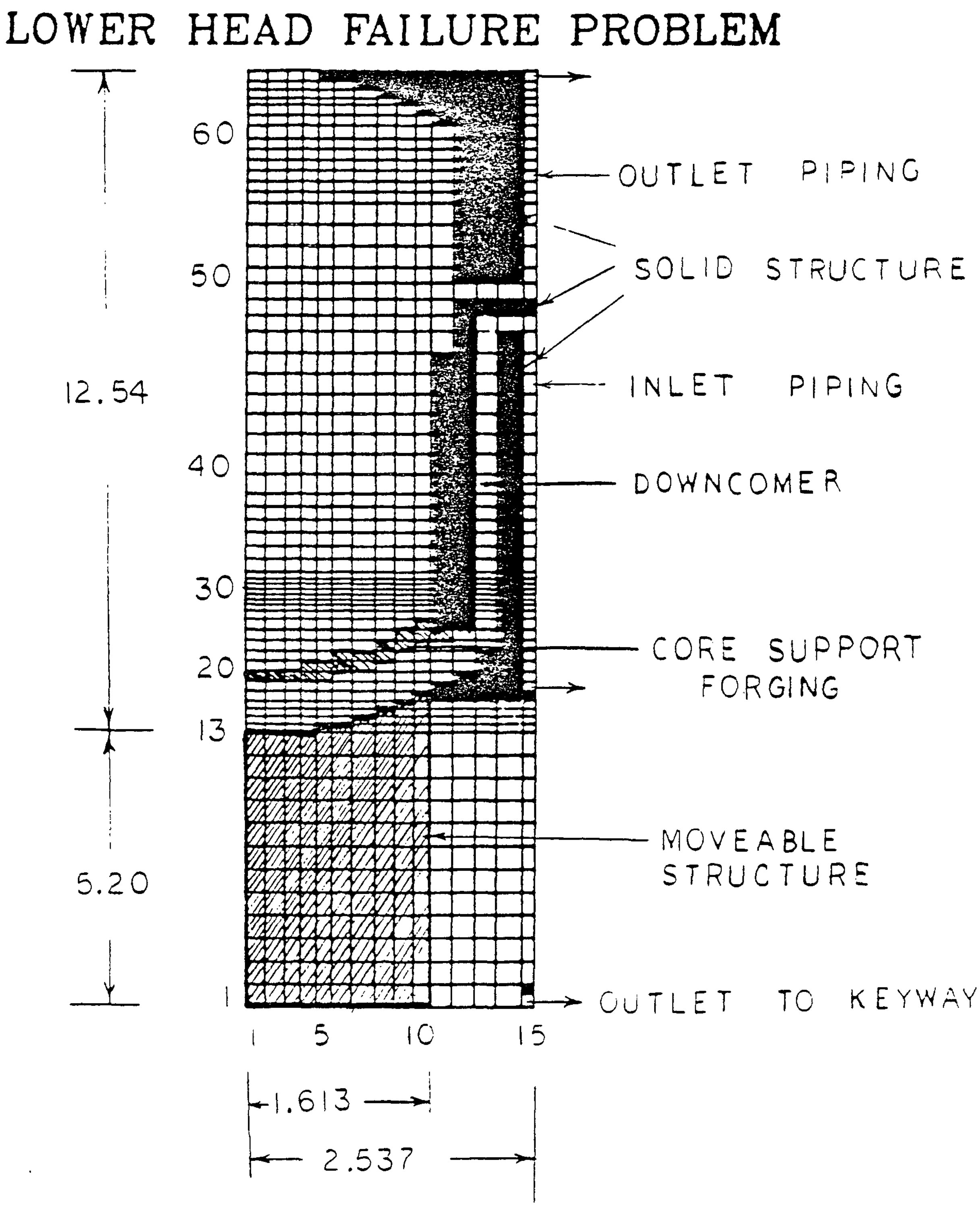

Fig. 1.
Mesh and structure geometry for the quasi-mechanistic reactor steam explosion problem. (Note expansion of the radial direction.)

boundaries were rigid. (The radial continuative inflow/outflow boundary conditions change the velocities at the boundary based on the pressure difference that develops over a time step. They allowed very limited pressure relief in this problem.)

The lower vessel head is calculated to fail at 2.7 ms after the start of the "explosion." The downward velocity at this time is 150 m/s. Head failure results in an immediate depressurization of the lower vessel inlet plenum. Fig. 2 shows the pressure for the bottom node in the inlet plenum. The pressures driving fluid upward are not relieved quite as quickly, however. Fig. 3 shows pressures in the node above the core support. A pressure of almost 65 MPa is seen 15 ms after the start of the explosion. The most appropriate indication of the fluid expansion is the liquid volume fraction plots. Each line on these contour plots represents a 10% change in the liquid volume fraction. Fig. 4 shows the initial expansion. Core-pool breakup is calculated around the edges of the slug. Fig. 5 shows the impact with the steel particles, which tend to reform the slug, and then impact with the upper head. Downwardly directed kinetic energy is mainly as a low density spray. Fig. 6 gives the total kinetic energy produced. The peak value of 1460 MJ is close to the 1490 MJ of the corresponding explosion in our letter of November 2. However, in this case significant kinetic energy is associated with the downward moving head structure. About 610 MJ are lost at 22.7 ms when the downward moving head is assumed to stop. The upward kinetic energy further decreases as steel particles are collected into the slug. The total system kinetic energy at impact with the upper head is slightly less than 600 MJ. Figs. 7, 8, and 9 indicate the spatial and temporal load distribution on the upper head. The peak pressure is biased to the apex of the head. Fig. 10 gives the force on the head. It is below the 1000 MN level, defined as representing significant head impact in our previous letter. Finally, pressures at the side in the bottom of the below-vessel cavity are shown in Fig. 11. A accurate treatment of water in the keyway would probably indicate the beginning of significant expulsion into the containment.

In conclusion, although the mixing conditions for this case are edge-of-spectrum, (with low heat transfer during mixing) both the magnitude of the upper head loading and the load distribution are not indicative of large-scale missile production. This calculation demonstrates that the approach

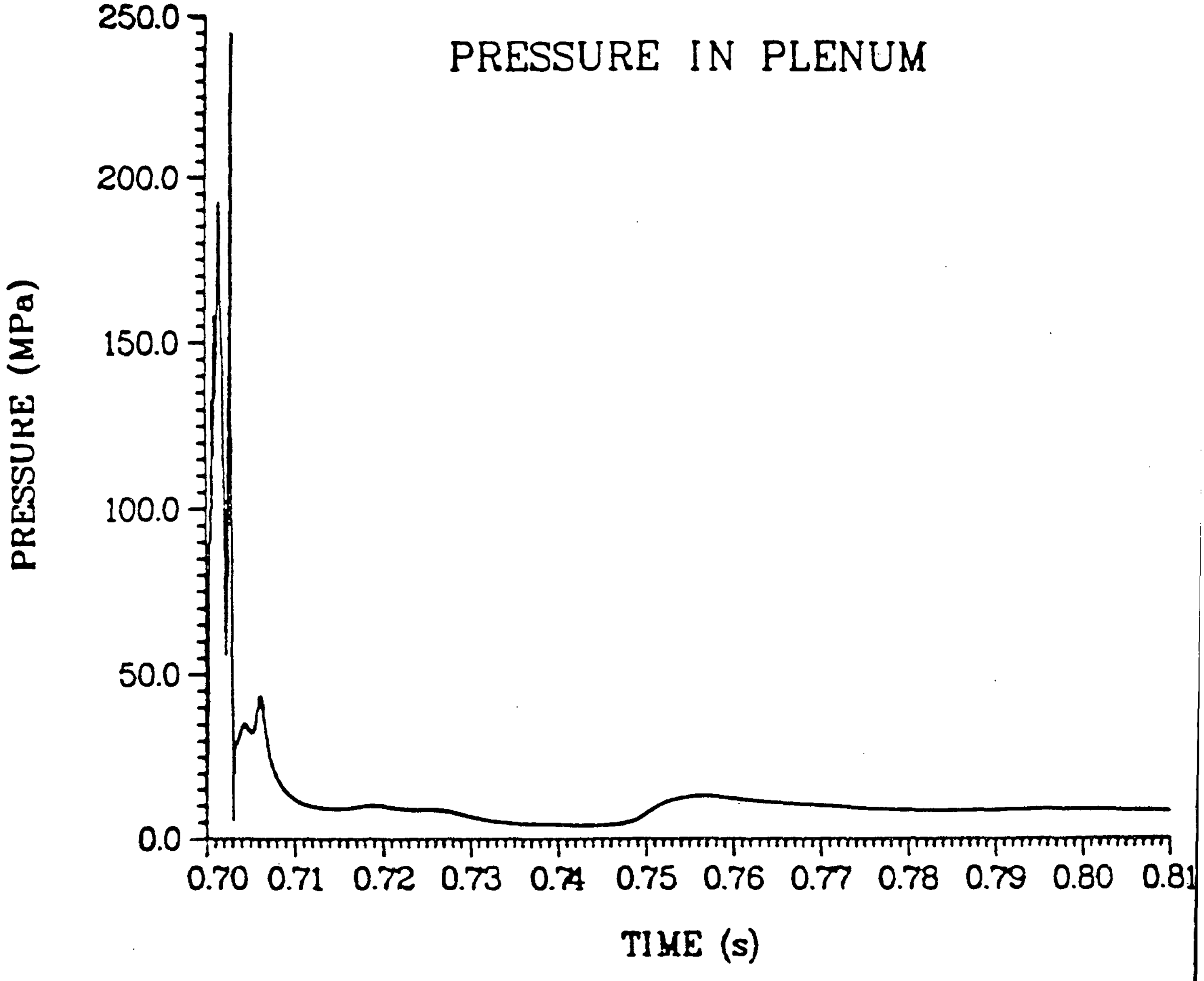

Fig. 2.
Center lower boundary node in the inlet plenum for the reactor explosion case.

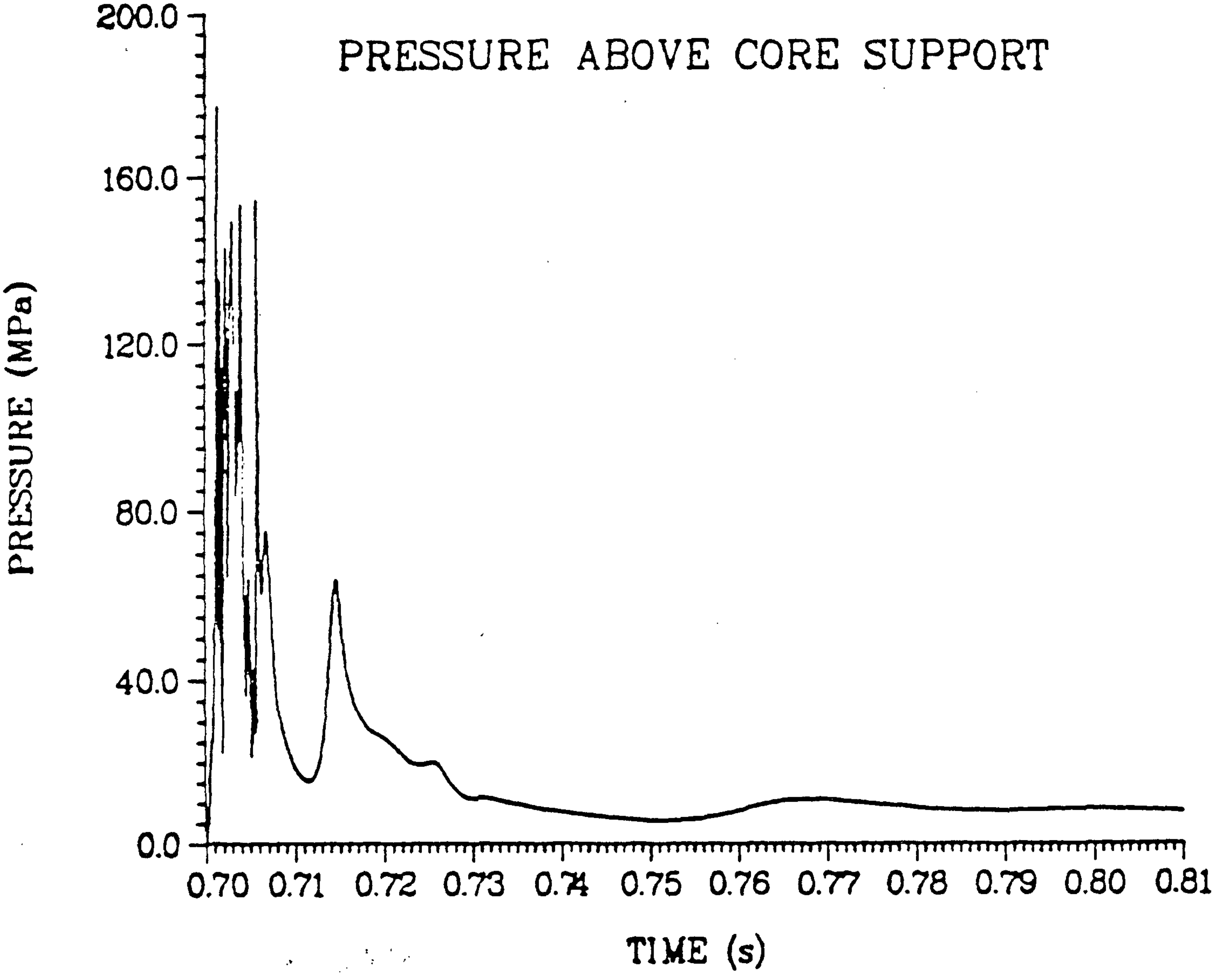

Fig. 3.
Rough approximation for driving pressure in the reactor explosion case.

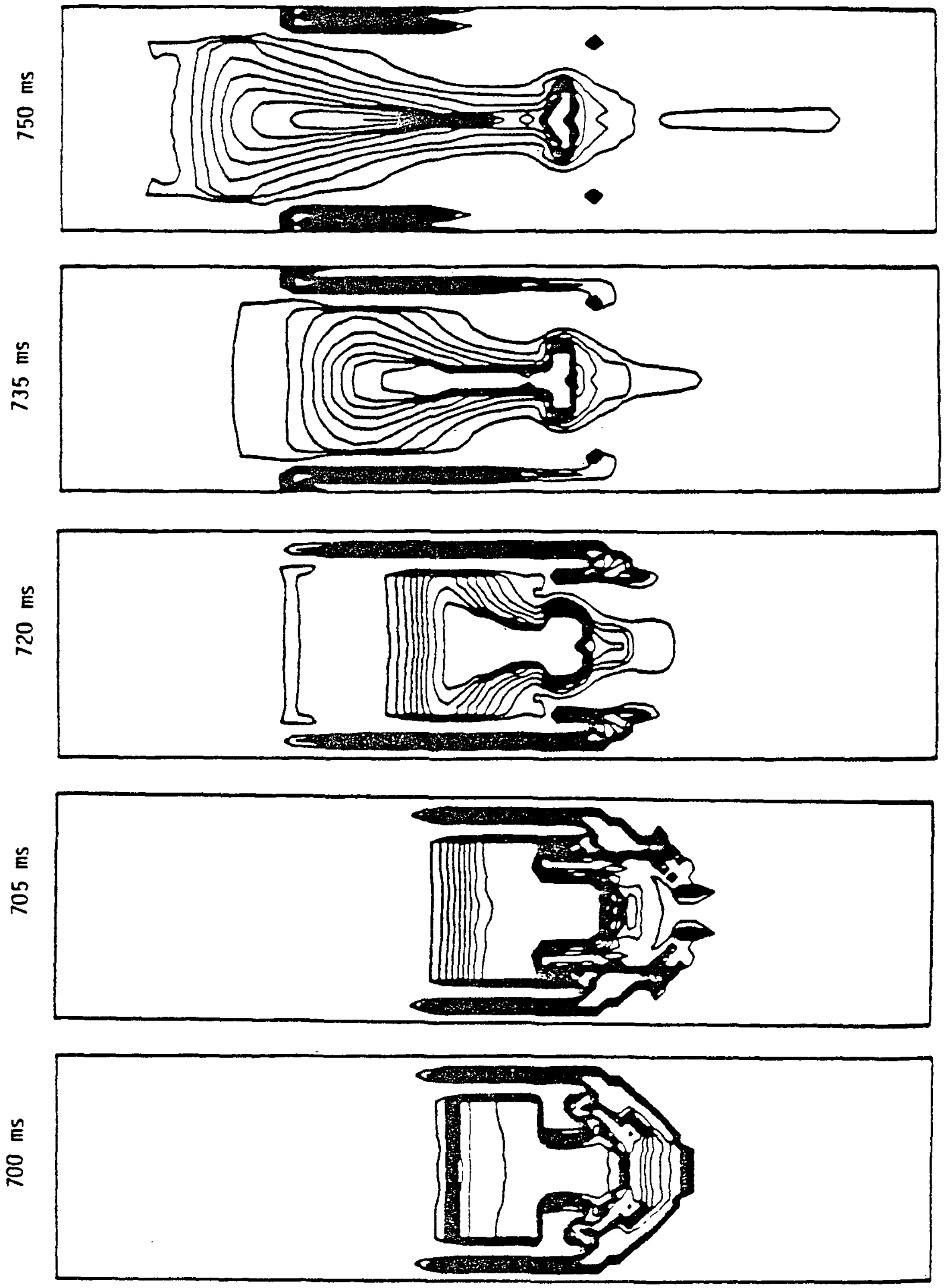

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Fig. 4.
Initial expansion represented by the liquid volume fraction in the reactor explosion case.

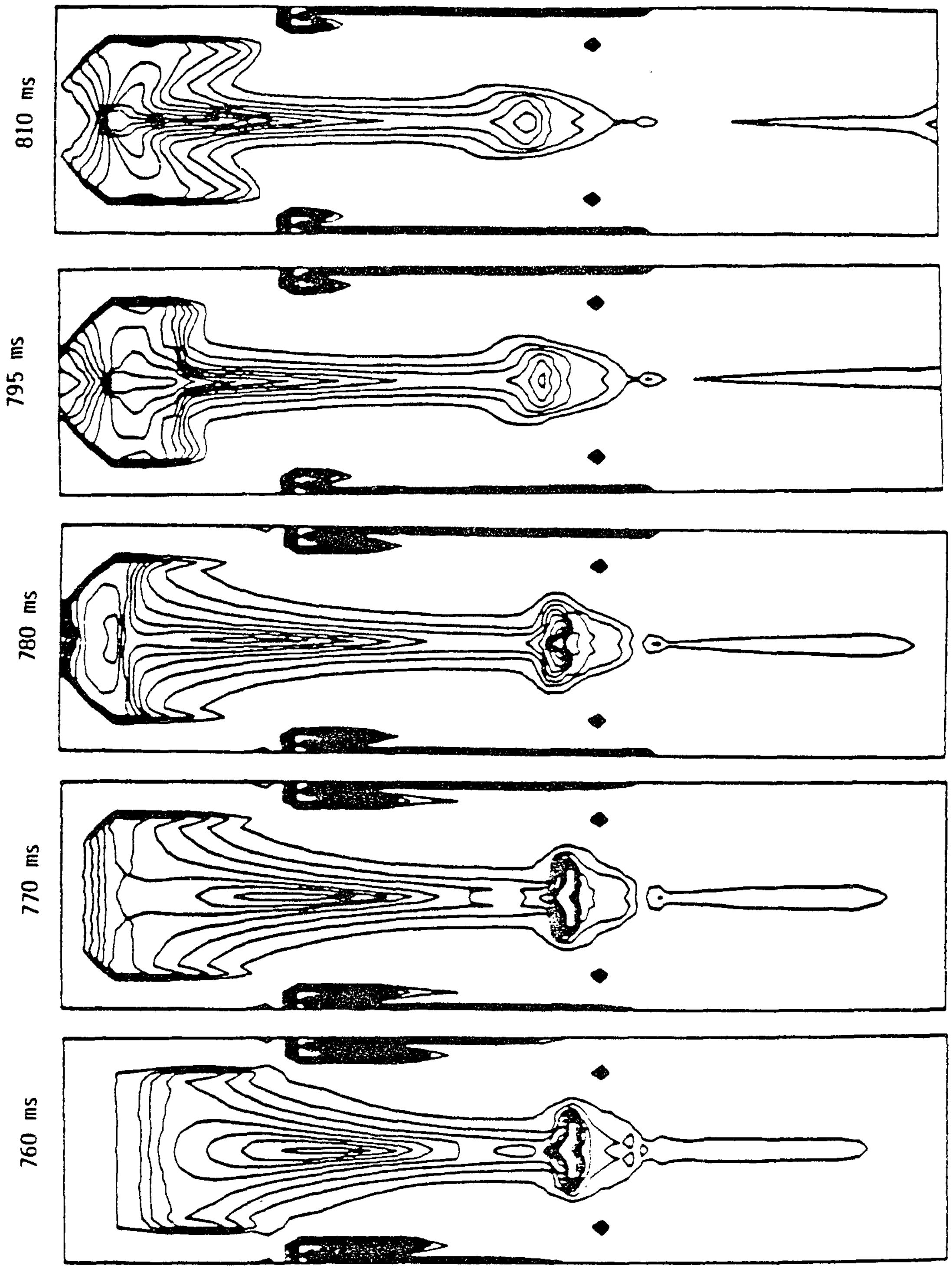

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Fig. 5.
Impact dynamics represented by liquid volume fractions in the

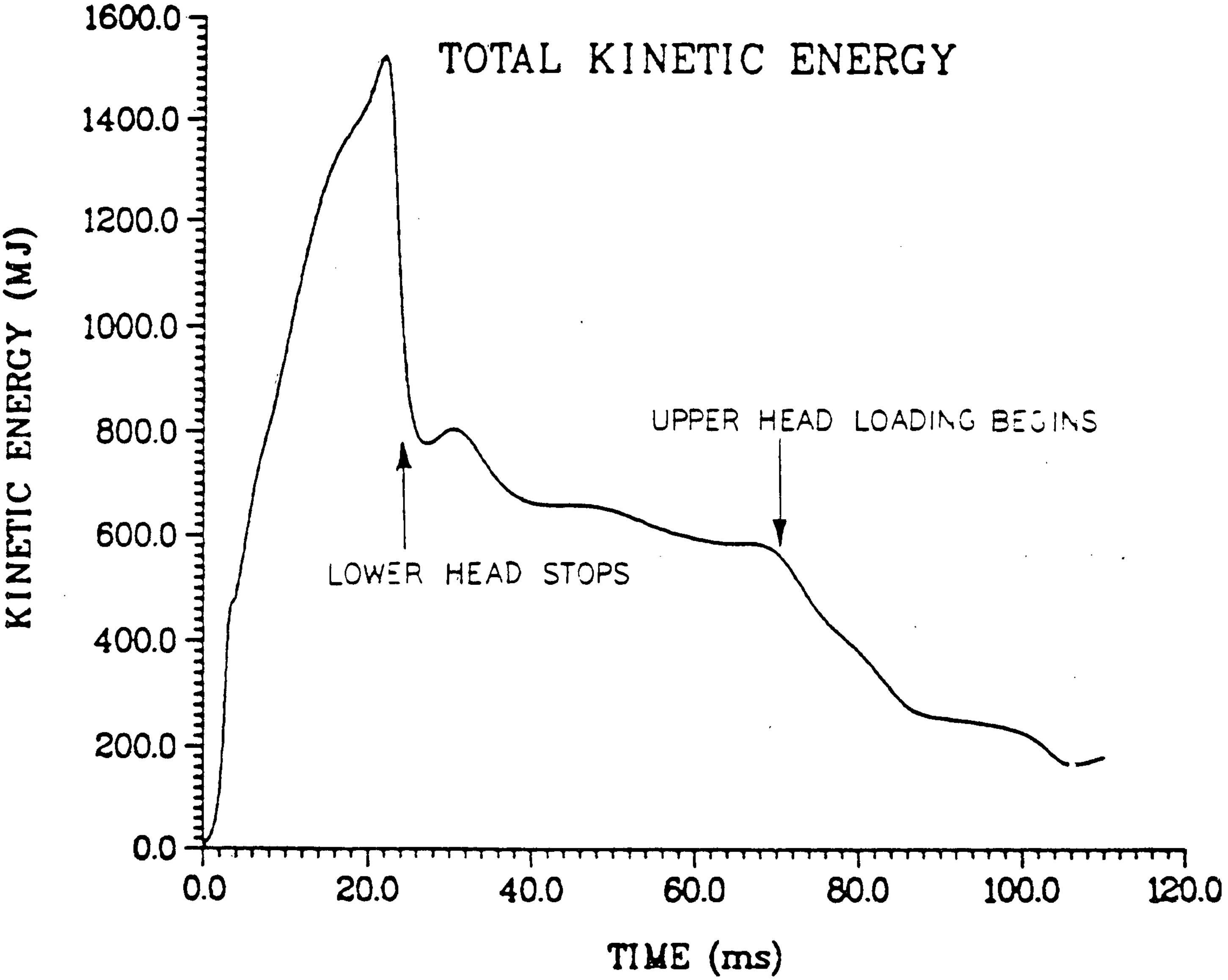

Fig. 6.
Total kinetic energy in the reactor explosion case.

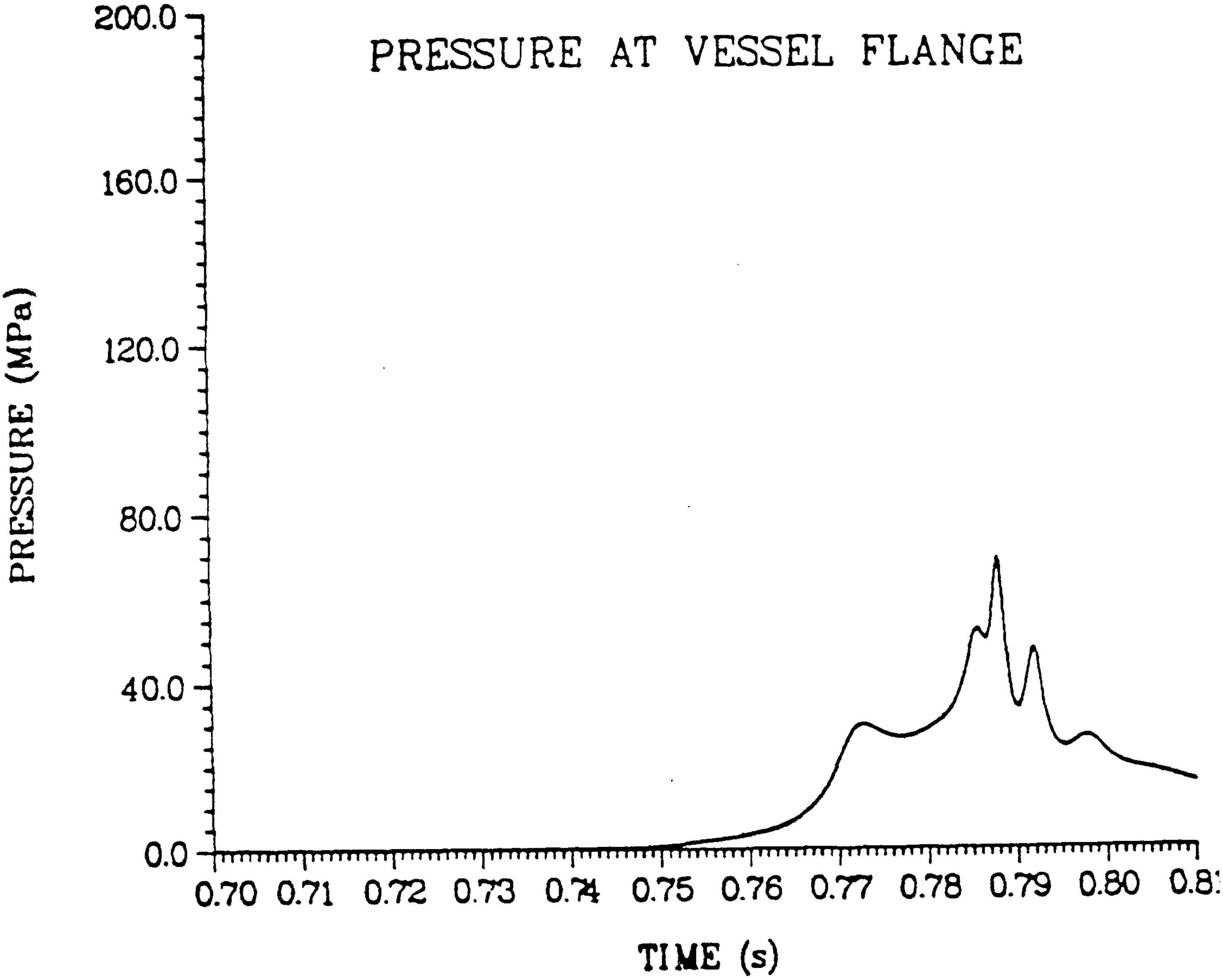

Fig. 7.

Pressure on the head, 70° to the vertical, reactor explosion case.

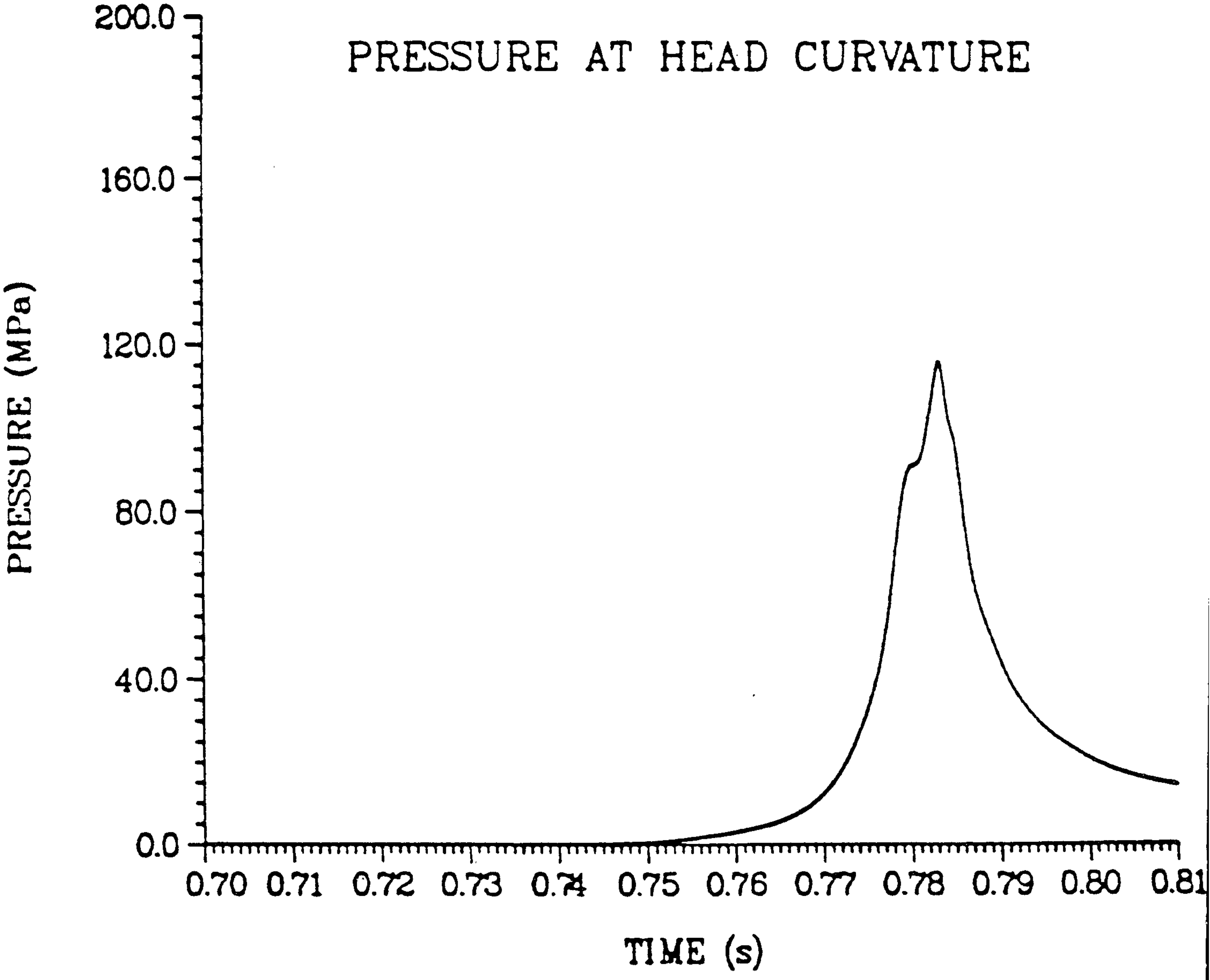

Fig. 8.
Pressure on the head, 30° to the vertical, reactor explosion case.

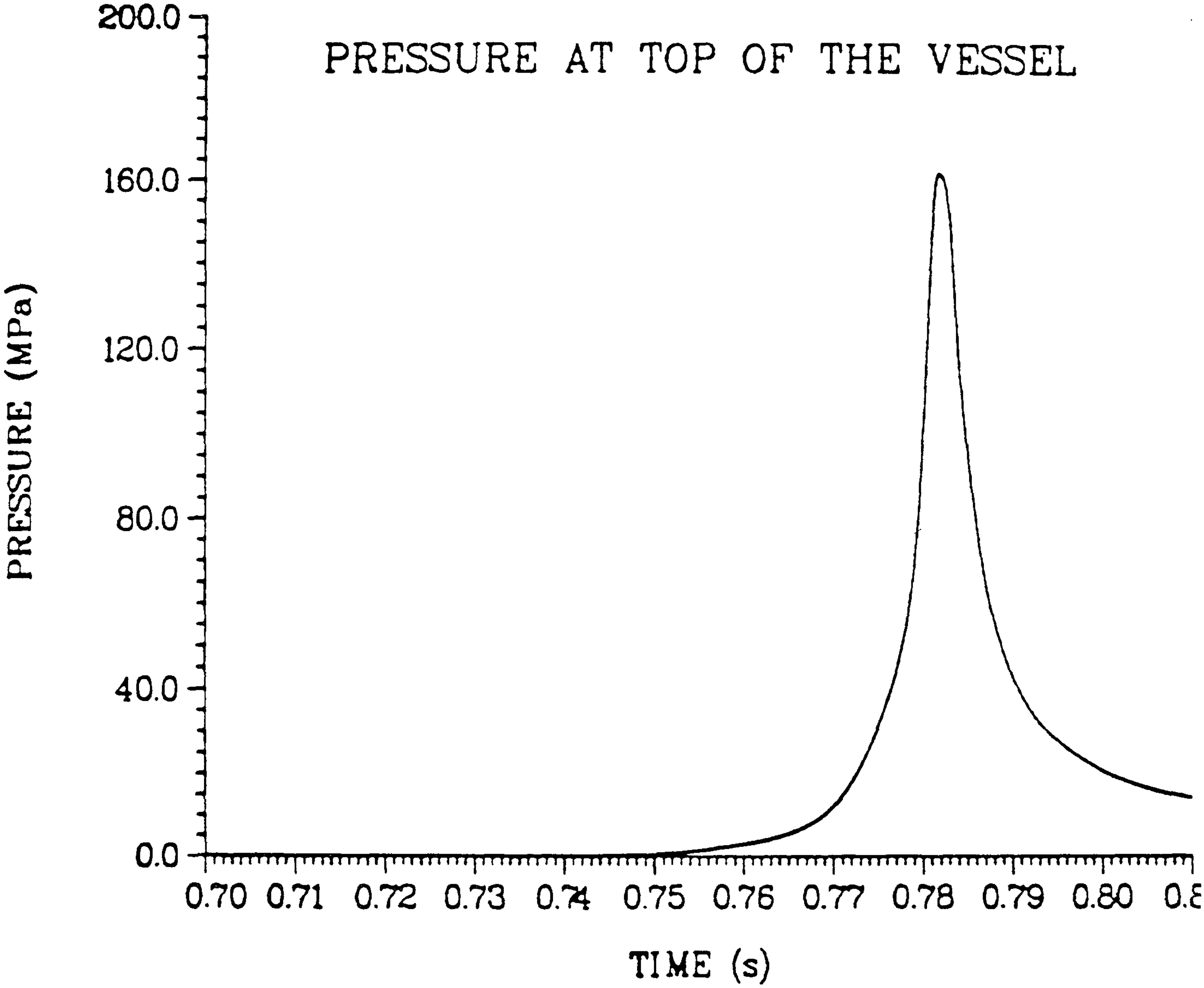

Fig. 9.

Pressure on the top of the head, reactor explosion case.

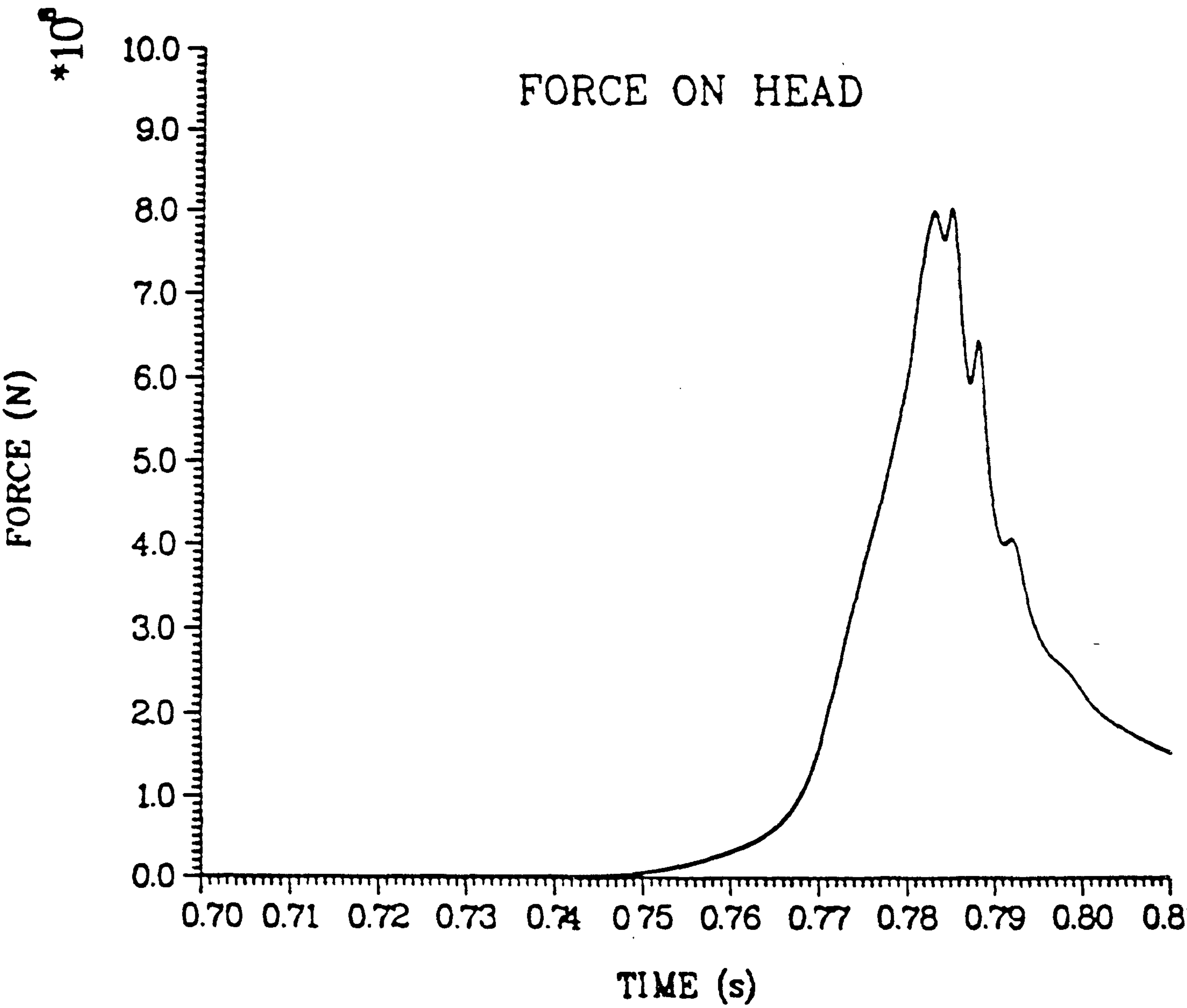

Fig. 10.
Integrated force on the head for the reactor explosion case.

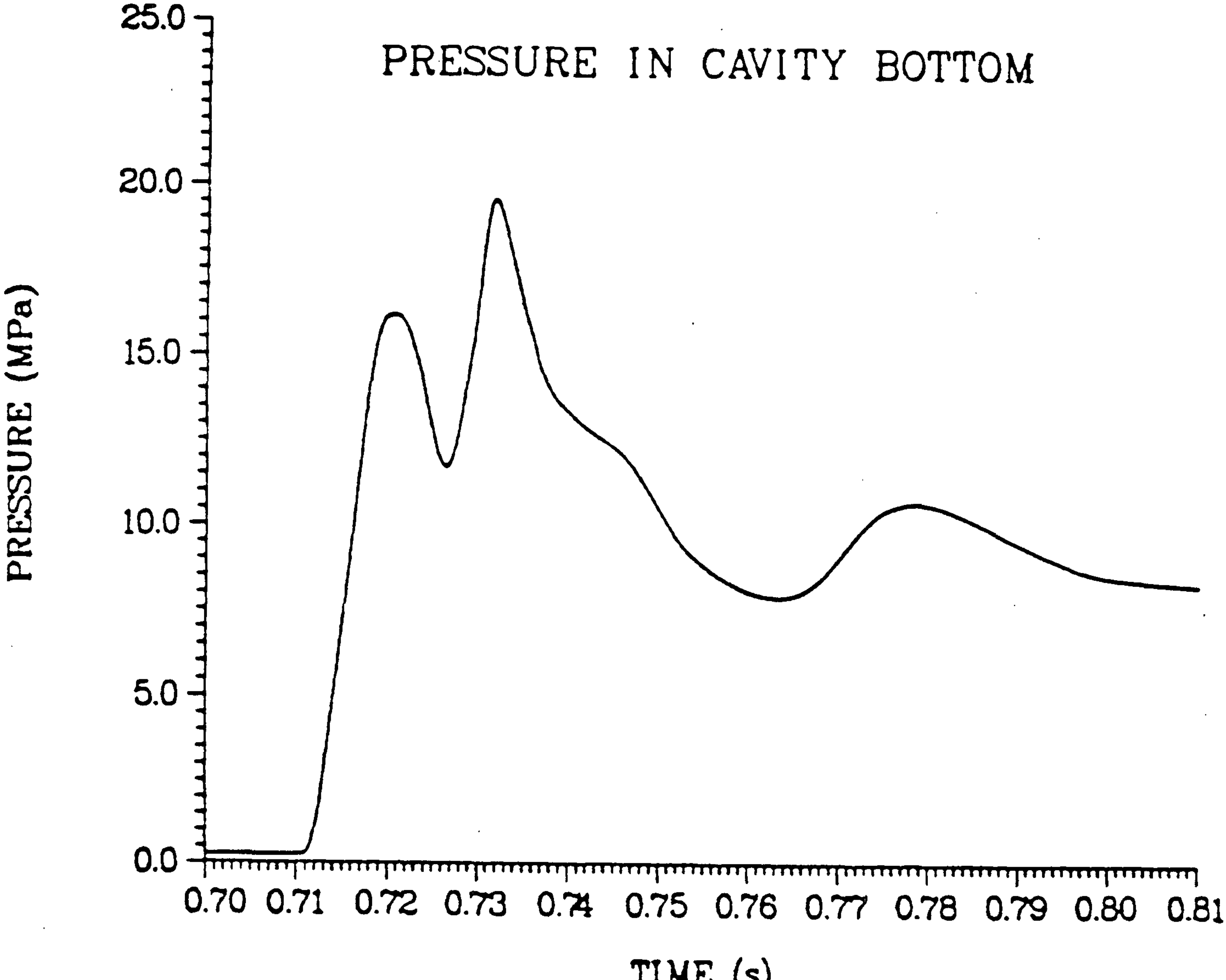

Fig. 11.
Pressure buildup at the side of the bottom of the cavity, reactor explosion case.

employed has promise, and that additional means exist, besides the SEALS and SHIP tests, to obtain increased confidence on this issue. However, the available margin is uncertain. At some level, steam explosions are expected to still produce a containment challenge. This brings us back to the need for large scale tests.

4. Large Scale Tests

One way to place the matter of potential damage from large-scale steam explosions on an objective scientific basis, and to find clear evidence that one element or set of elements is nearly impossible to achieve, is to look at the event sequence required for containment failure to occur. Preliminary results obtained from the Los Alamos program suggest that one requirement is an explosion over an extended premixed region with local pressures that are on the order of several hundred megapascals (kilobars) which are sustained for several milliseconds (such as that associated with mixing a large fraction of the core). This configuration negates the mitigation features of the calculation presented in Section 3. Such pressures are possible if arbitrarily mixed configurations of water, steam, and melt are allowed. We believe they will prove not to be possible if the configurations must develop from initially separated constituents in reactor type geometry. This extended, fuel-rich, premixed region, required to overcome the pressure relief from lower head failure, is significantly more energetic than in the definition of a big steam explosion from our letter of November 2. Highly energetic explosions do not represent phenomenology which can be straightforwardly extrapolated from the FITS tests because extreme pressurization is not possible in an interaction zone that is water rich, has a high initial steam volume fraction, has a fuel volume fraction of 0.01 to 0.03 and has minimal upward constraint.

Our present expectation is that on a reactor scale, mixing with coexisting water and fuel will occur only near the interface of separated melt and water regions. A uniformly mixed inlet plenum will not occur. Properly quantified and accepted limits using this picture, or better, a model which will allow such a development to be calculated in a postulated meltdown accident should be sufficient to eliminate concern over the alpha mode of containment failure when coupled to a damage assessment of a subsequent explosion.

An escalation process involving more of the melt is required if true containment challenging situations are to arise. On a large scale such escalation has been postulated as the mechanism of hydrovolcanic eruptions[4]. These are natural phenomena produced by the interaction of magma or magmatic heat with an external source of water, such as a surface body or an aquifer. Driving pressures of up to a few kilobars[5] may be estimated from the consequences of known explosions, although hydrovolcanic processes can lead to results of all sizes ranging from small phreatic (pertaining to ground water) craters to huge calderas. A schematic, Fig. 12, reproduced from Ref. 4, shows the hypothesized escalation process. Thus the required initial pressures ultimately leading to containment failure may be theoretically possible. However, a major difficulty is whether the type of inertial and structural constraints present would permit a time scale satisfying the mixing energy requirements involved in such a energetics "boost" to a large steam explosion following a reactor meltdown accident. Ultimately, we would expect that a 1/1000 probability can be associated with the likelihood of such an explosion. With proper experimental information, this large steam explosion would represent physically unreasonable behavior violating well-known reality and its occurrence should be arguable against positively.

Because some arguments must be made regarding the extent of water-fuel mixing, we believed SIMMER-II calculations in SEALS geometry would be useful. Two SIMMER-II calculations were performed simulating coarse premixing in SEALS type geometry. One of the resulting configurations was then used to perform two explosion calculations. The objective was to compare to similar calculations of previous FITS tests and with reactor scale simulations. Because SIMMER-II cannot represent a best-estimate mixing process with current algorithms, the two premixing calculations were chosen with parameters that appear to bound the

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[4]Michael F. Sheridan and Kenneth H. Wohletz, "Hydrovolcanism: Basic Considerations and Review," Journal of Volcanology and Geothermal Research, 17, (1983) pp 1-29.

[5]Kenneth H. Wohletz, private communication, Los Alamos National Laboratory, 1984.

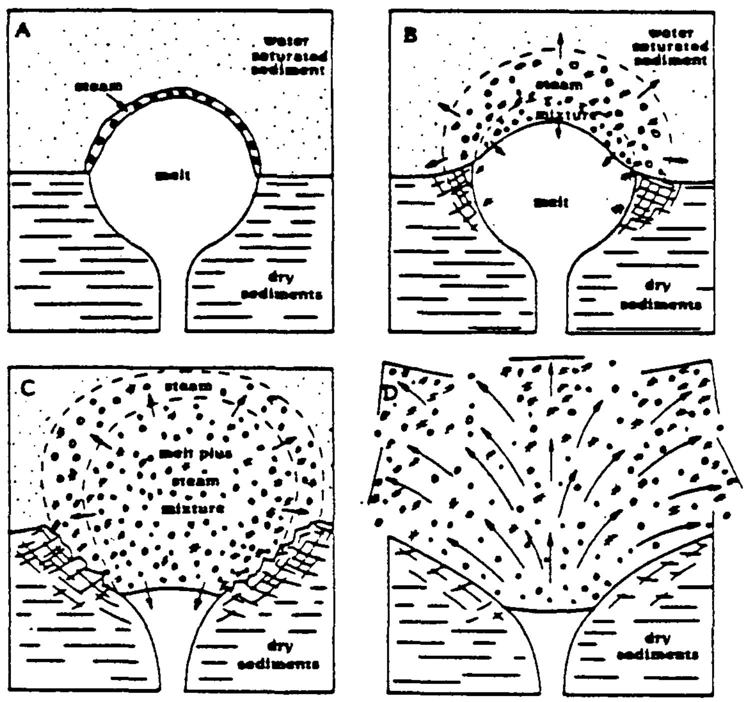


Fig. 1 Schematic diagram showing the stages of water/melt mixing within a multi-layered medium. A. Emplacement of melt into contact with water-saturated sediments. A thin vapor film develops along the contact. B. Pulsating increases in the high-pressure steam volume within the aquifer. Possible local brecciation of the country rock at this stage. C. Large-scale water/melt interaction. Mixing of country rock, steam, and melt. D. Explosive rupture of the confinement chamber.

(FROM REF. 4)

Fig. 12.

Hypothesized escalation process in hydrovolcanic eruptions.

amount of steam produced and consequently the influence of steam generation rates on thermite motion during the premixing part of the experiment.

The initial conditions assumed are shown in Fig. 13. At time zero, 2000 kg of 3000 K thermite is released into saturated water. An initial radial constraint but no axial constraint was modeled. Both the thermite and the water were assumed to be prefragmented to 15 mm diameter droplets. This size is similar to the coarse premixed melt sizes reported in FITS. The sides and bottom of the problem were presumed to possess rigid boundaries, although the top boundary was assumed to be at constant pressure.

Minimal steam production was obtained with the use of nominal SIMMER-II heat transfer and momentum transfer assumptions. Heat transfer in this case is limited by the water thermal conductivity. Density contours for the fuel (thermite) and coolant (water plus steam) are shown in Figs. 14 to 17. The fuel does remain largely separated from the water, with the water tending to close over the fuel around 800 ms. Sufficient steam is produced to preclude formation of regions with equal volumes of thermite and water coupled with low volumes of steam that could produce excessive pressures.

In the second calculation heat transfer to the water surface was only limited by heat conduction out of the thermite and the assumed water/thermite droplet contact dynamics. Any heat that could not be conducted into the water was assumed to produce steam. The density contour results are shown in Figs. 18-21. Here most of the thermite is blown away from the water and only a low density fuel/water mixture remains in the center of the picture at 800 ms. The results in this limit may be similar to those that might be qualitatively extrapolated from the IDCOR one-dimensional CHF steady-state model.

The influence of steam production on thermite dispersal is the biggest difference in the present calculations in comparison to previous FITS calculations. In the "intermediate" scale FITS simulations the high steam production assumptions merely caused excessive radial dispersal and lower than expected thermite fall velocities. In comparison to the reactor style simulations, the SEALS thermite experiments do not possess the inertia and continuous pour capability that would be associated with approximately 100 tons

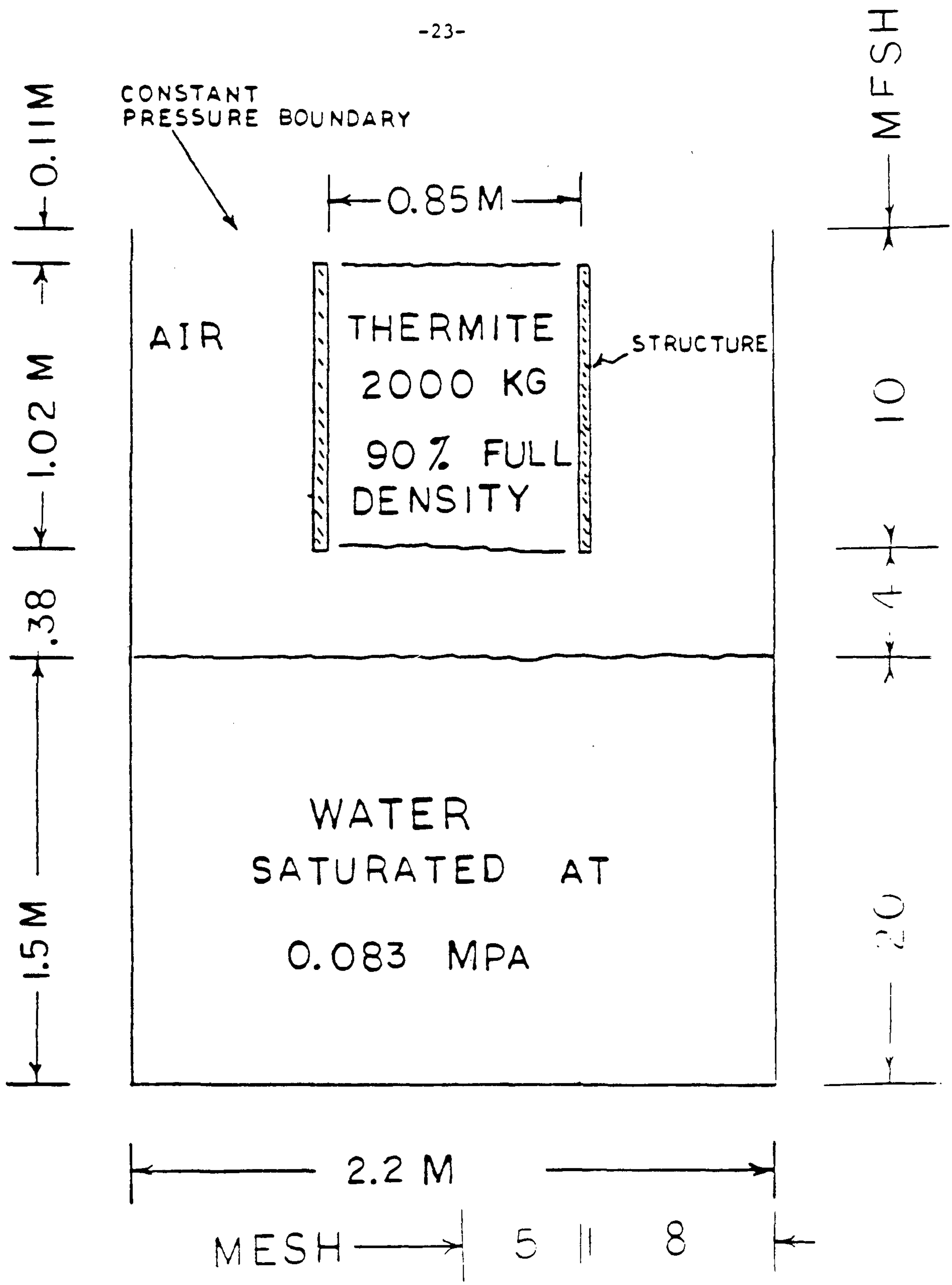
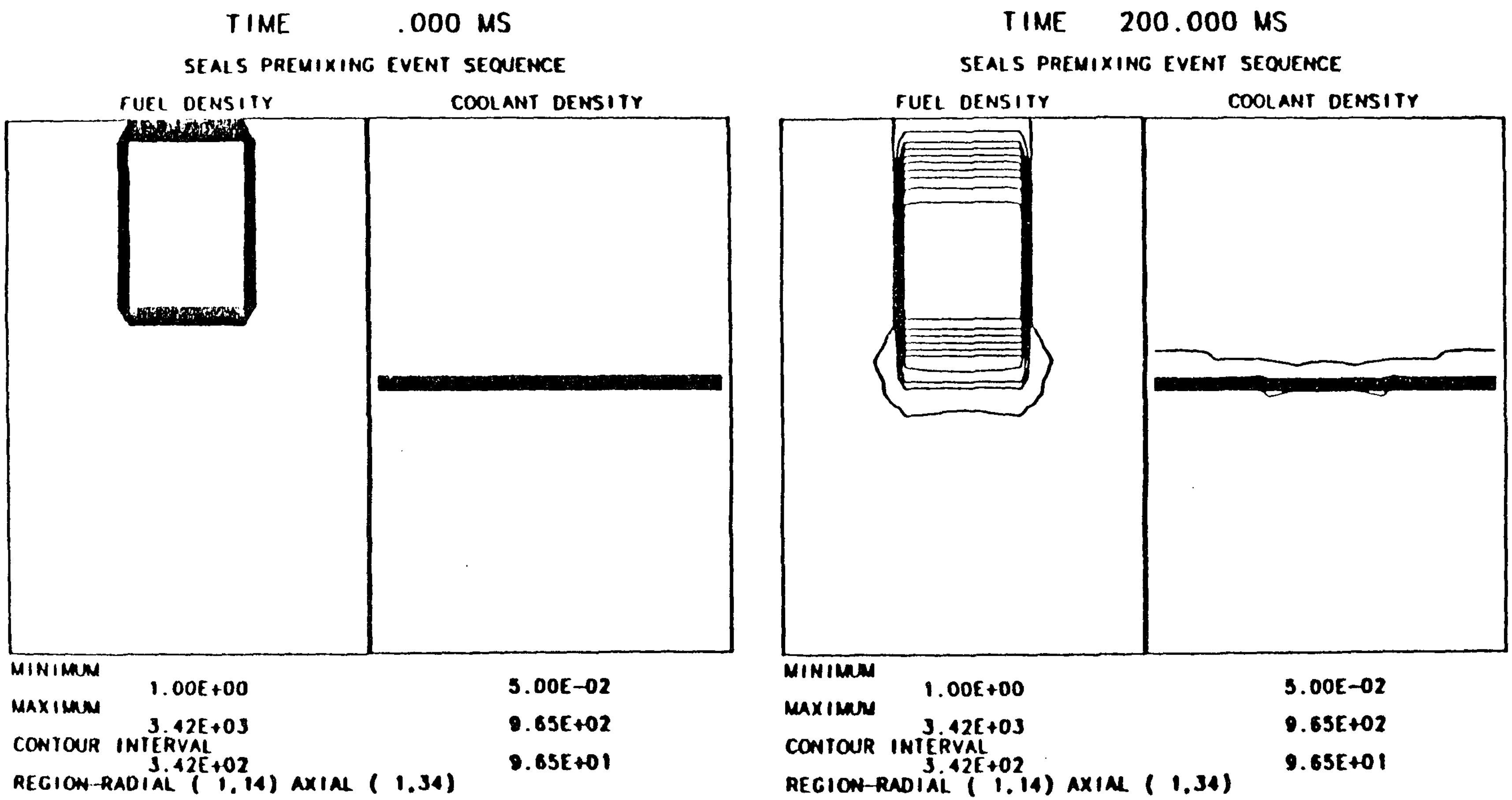


Fig. 13.

Initial conditions for SIMMER-II calculations of premixing in a SEALS test.



C-2.81

Fig. 14.

Developing density contours in SEALS test simulation, standard heat transfer - 1.

C-2.82

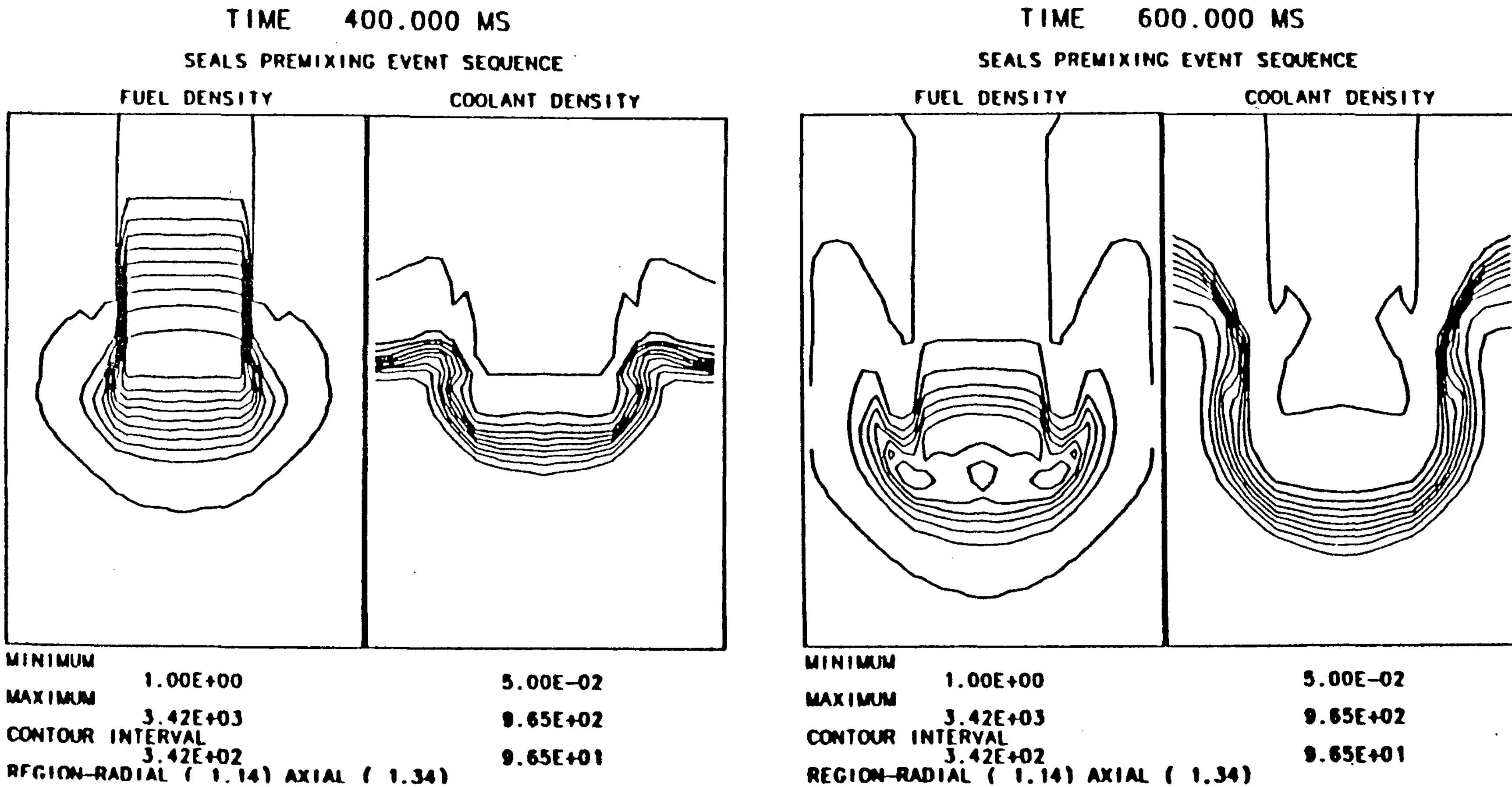
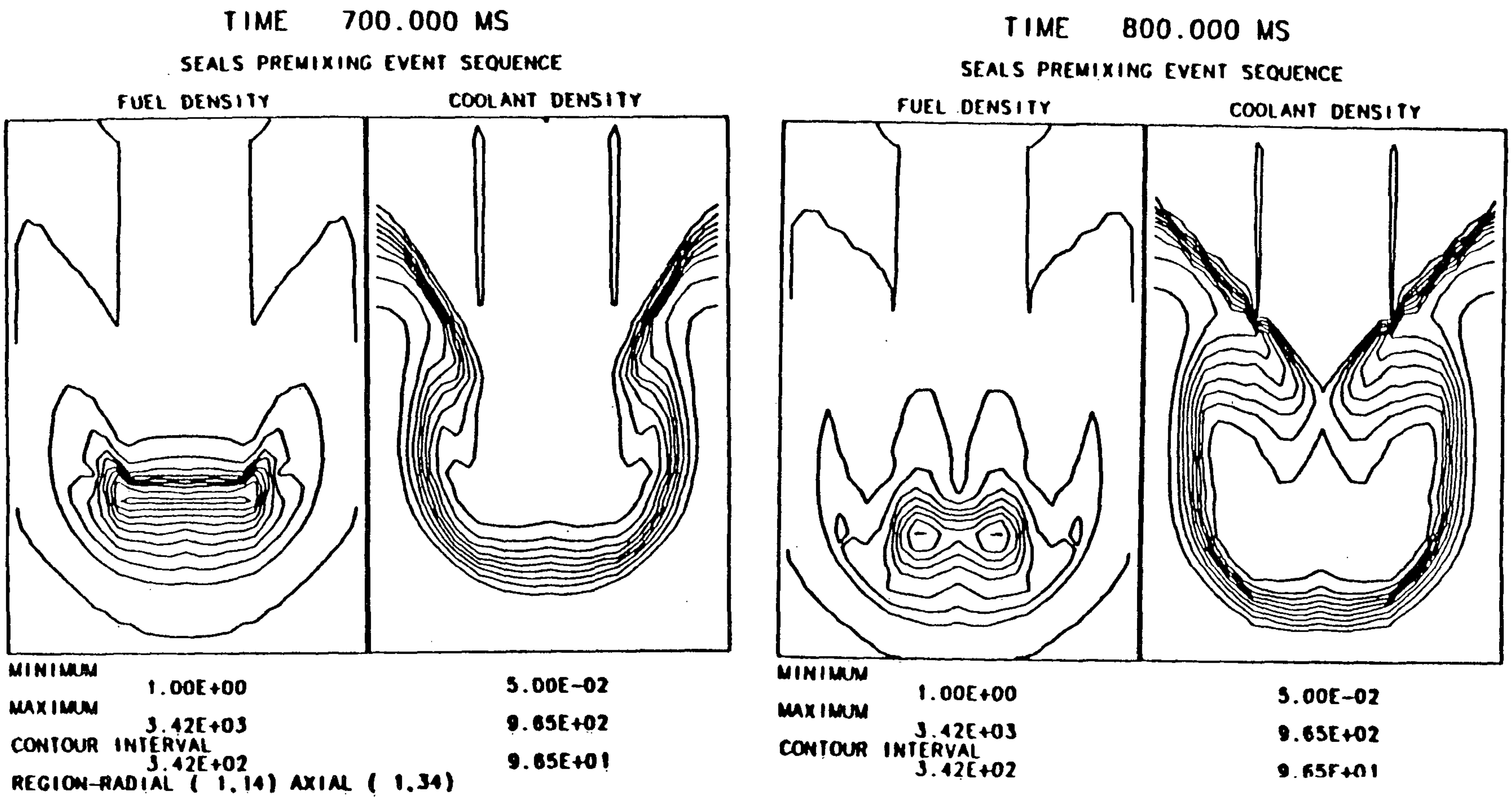


Fig. 15.

Developing density contours in SEALS test simulation, standard heat transfer - 2.



C-2.83

Fig. 16.

Developing density contours in SEALS test simulation, standard heat transfer - 3.

C-2.84

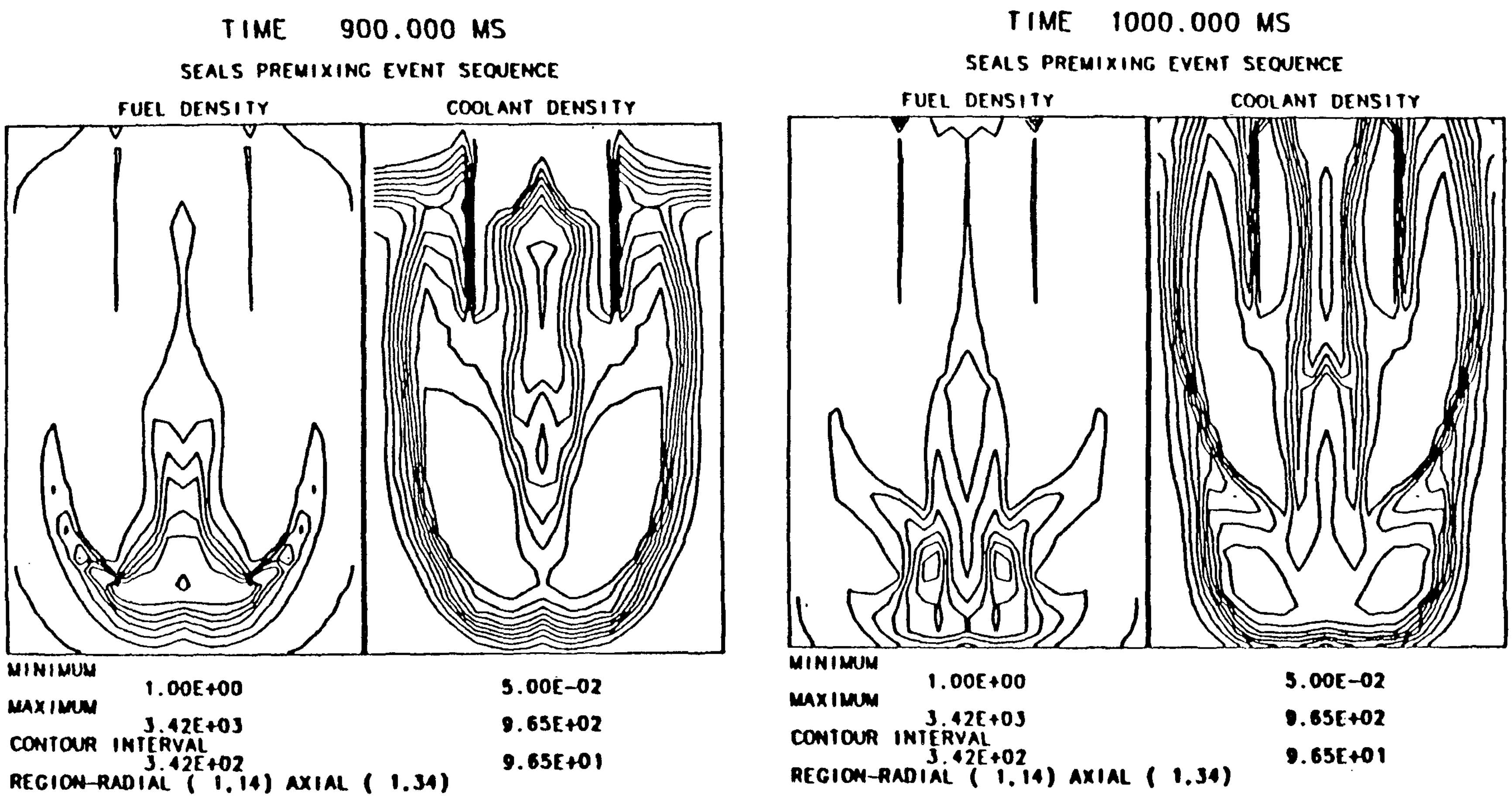


Fig. 17.

Developing density contours in SEALS test simulation, standard head transfer - 4.

C-2.85

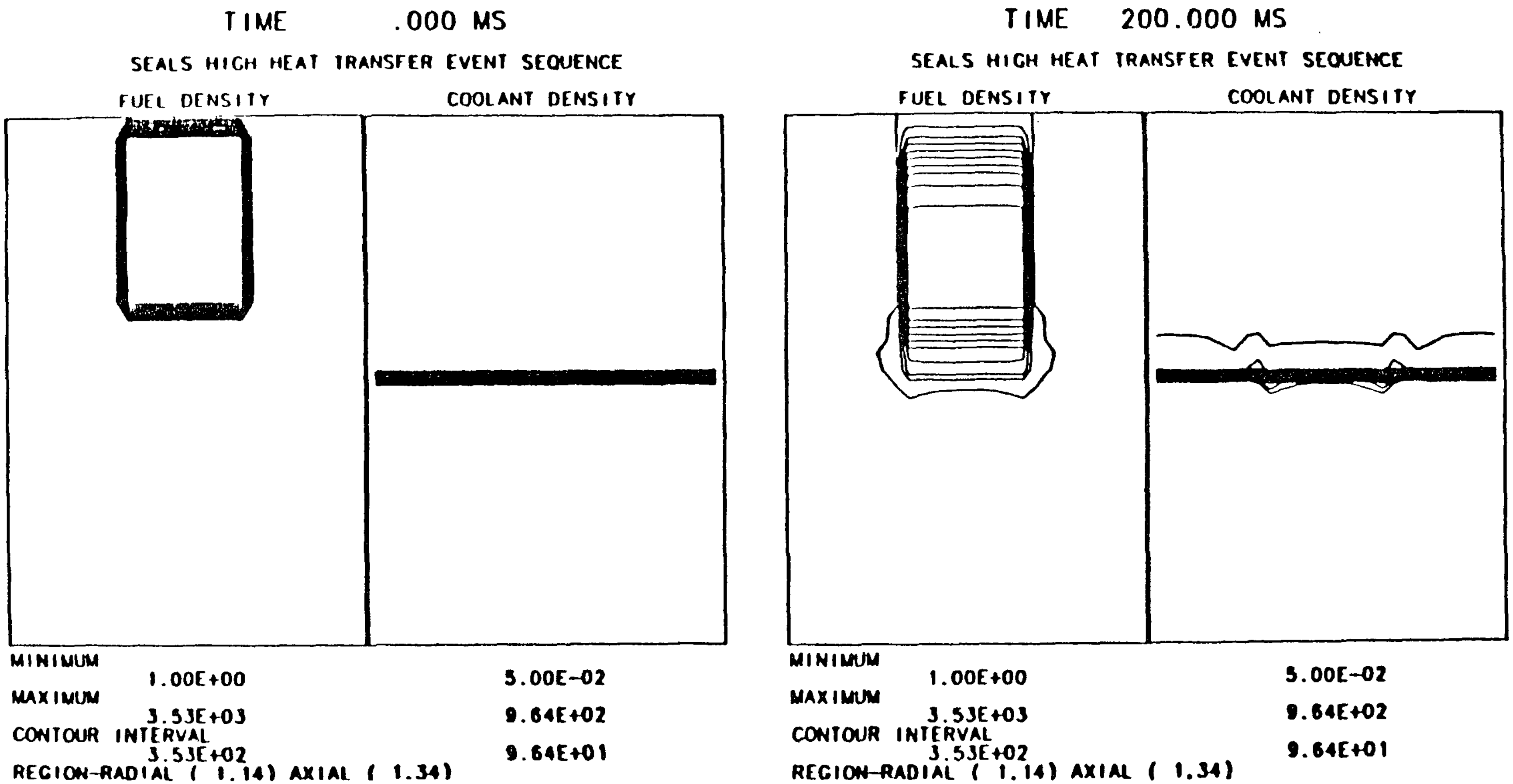


Fig. 18.

Developing density contours in SEALS test simulation, high heat transfer - 1.

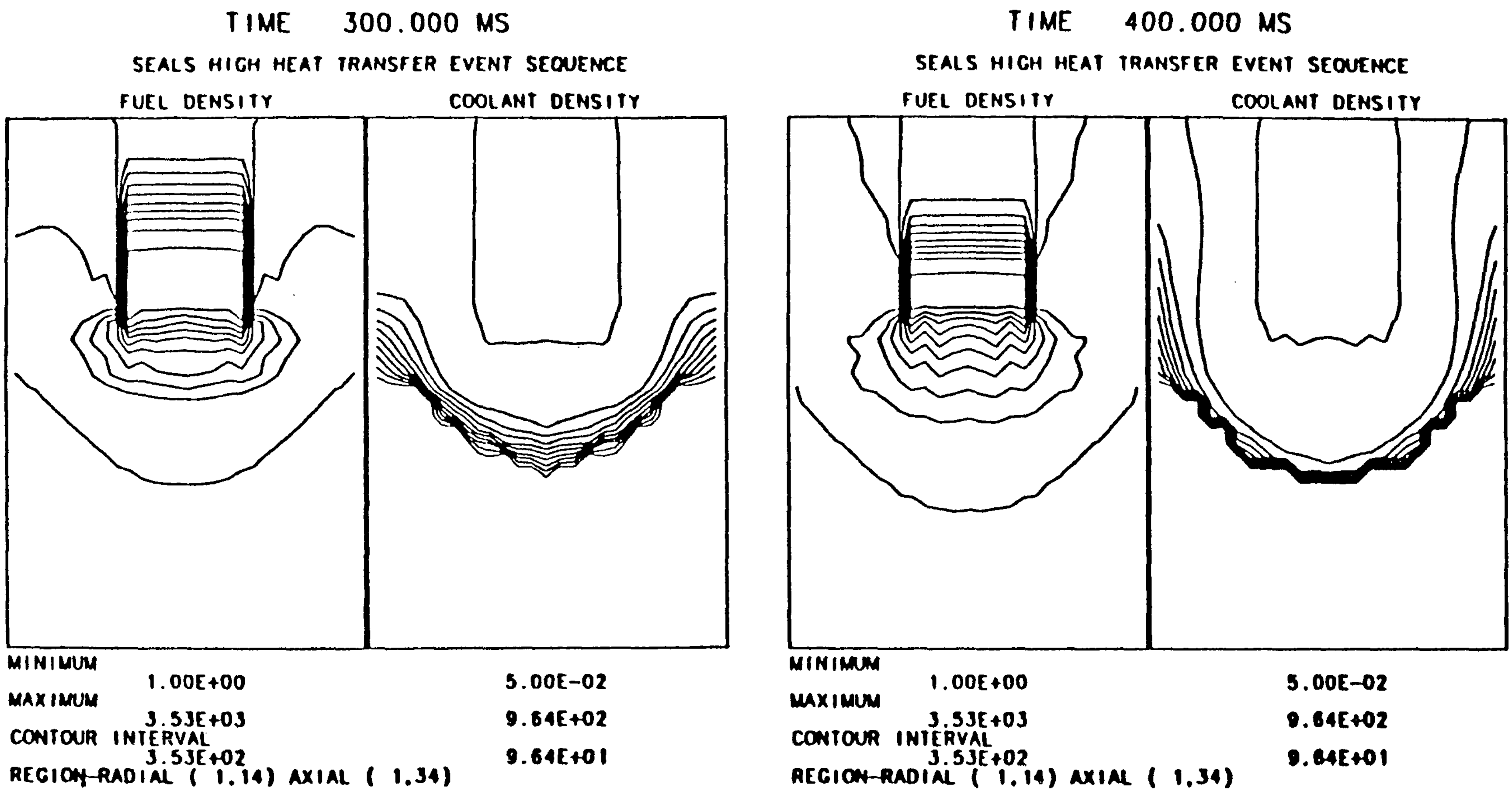
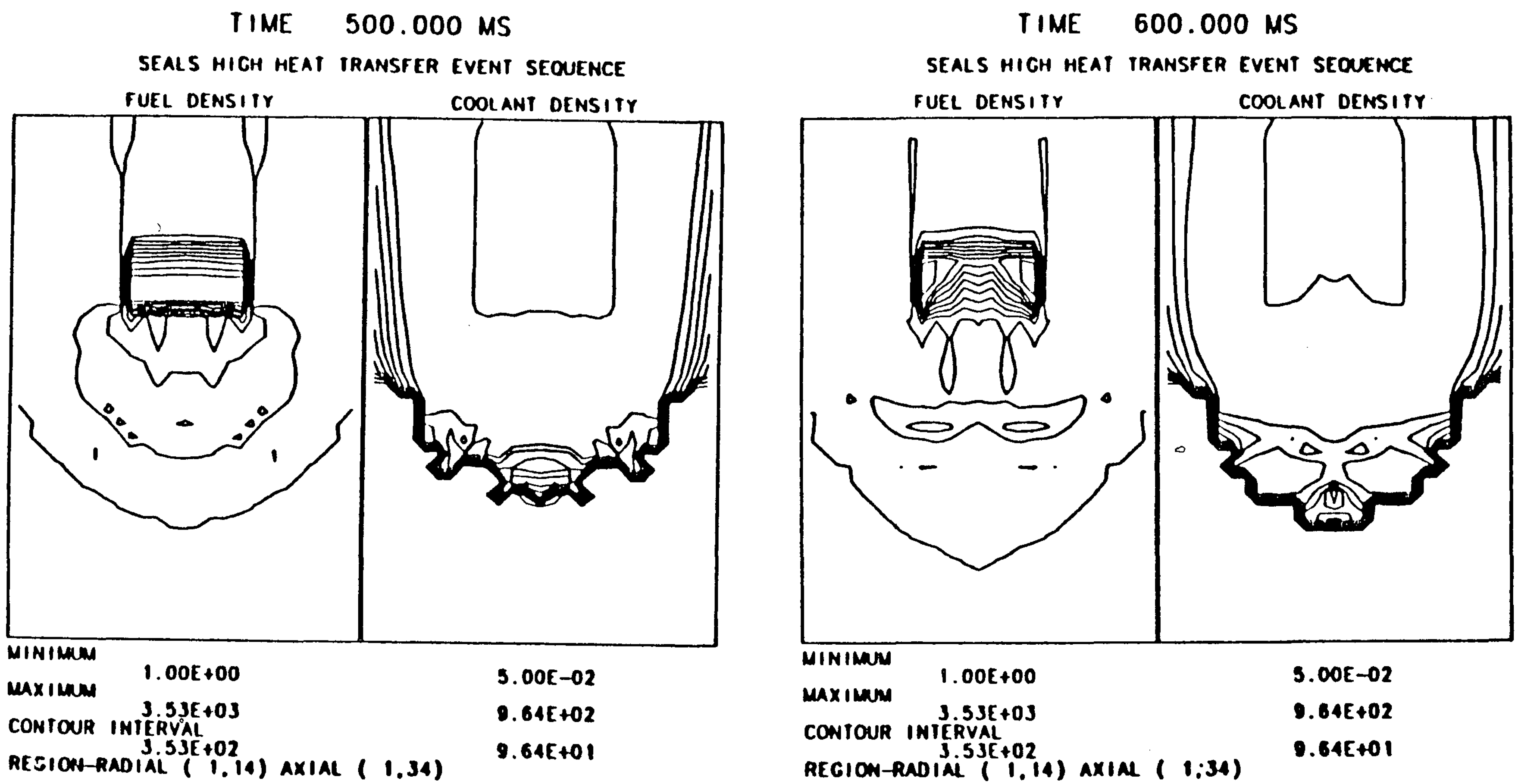


Fig. 19.

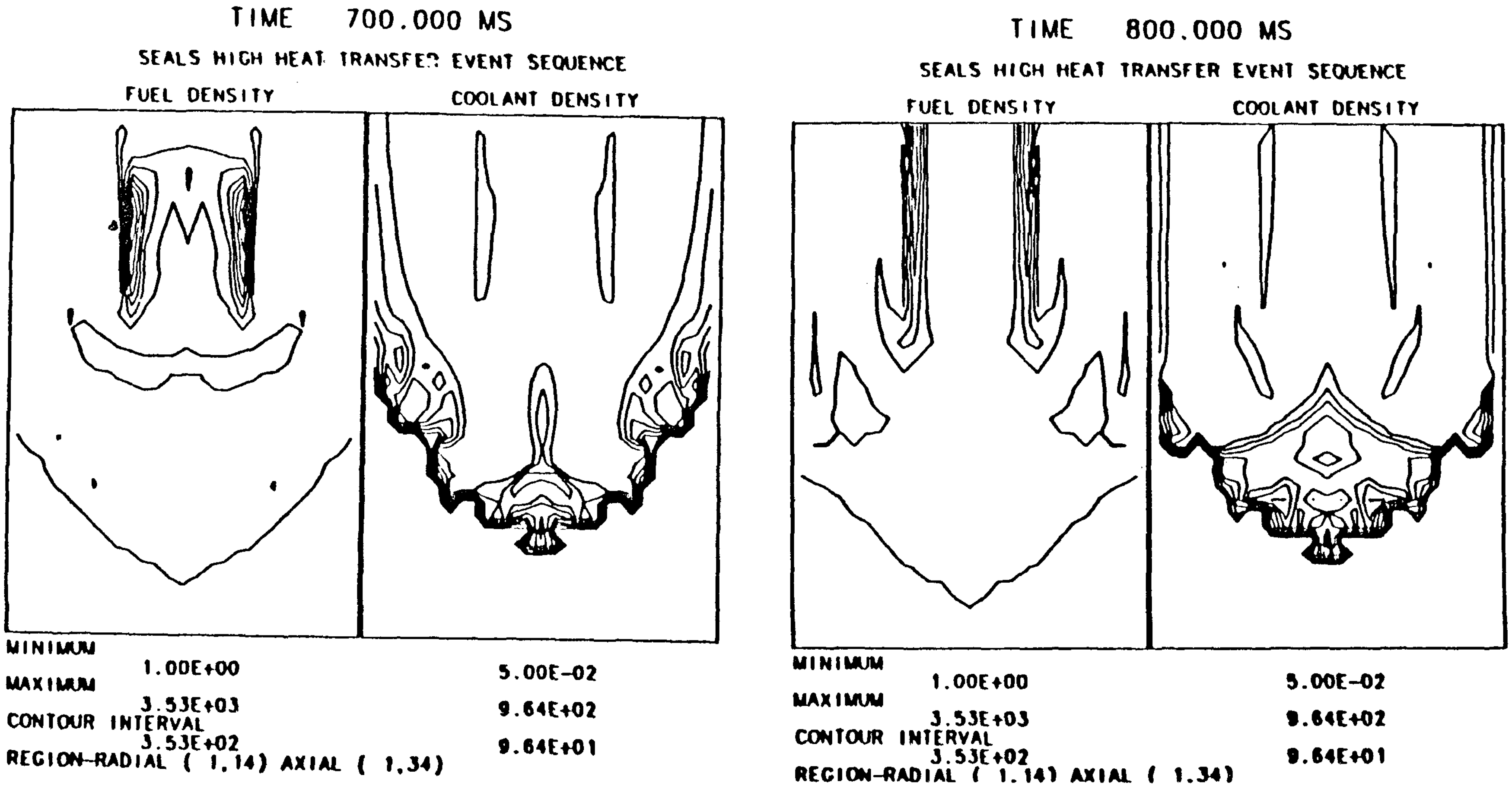
Developing density contours in SEALS test simulation, high heat transfer - 2.



C-2.87

Fig. 20.

Developing density contours in SEALS test simulation, high heat transfer - 3.



C-2.88

Fig. 21.

Developing density contours in SEALS test simulation, high heat transfer - 4.

of molten corium. However, the reduced tendency of an entering molten corium plume to initially disperse in the reactor case is modified by the structure in the lower plenum.

The configuration used for the "explosion" was that at 800 ms in the low steam production case. The thermite-water-steam mixture is beginning to contact the bottom of the container and water is closing over the top of the mixture. The SEALS CSQ problem geometry was selected for this case. This meant a rezoning of the SIMMER-II mesh with 5 added nodes to bring the total height to 3.5 m and an elimination of structure. The two cases run are an open case with constant pressure boundary conditions at the top and sides and a closed case with rigid wall boundary conditions. The unconstrained case resulted in a free expansion. Pressures at the center of the lower rigid bottom boundary are shown in Fig. 22. The corresponding plot for the constrained case is in Fig. 23. Besides the increase in peak pressure there is some increased area under the curve. This occurs even without SIMMER-II models for additional fragmentation and heat transfer in the constrained case. Also interesting is the liquid expansion in the closed case. Liquid volume fraction plots are shown in Figs. 24 and 25. An essentially single-phase liquid impact first occurs at the top along the sides of the container. Fluid then moves towards the center and a high two-phase pressure pulse develops in the center node. Figs. 26 and 27 show these pressure spikes. Material then collapses into the center of the cylinder. The force on the top of the problem is mainly the consequence of initial impact from water moving up the sides. This is shown in Fig. 28. The kinetic energy plot is given in Fig. 29. Appreciable axial pressure equilibration resulting from liquid-gas slip occurs before head impact is obtained. With a thermite available energy of 2.8 MJ/kg or a total of 5600 MJ, the maximum of 125 MJ of kinetic energy gives a conversion ratio of about 2.2%. Finally, the side pressure at the bottom of the cylinder is given in Fig. 30. These pressures are slightly reduced relative to the center explosion pressures. Reduction is typical in a SIMMER-II simulation of a steam-explosion experiment in cylindrical geometry.

An interesting point is that the explosion here involves a larger fraction of the melt than similar mixing and explosion assumptions gave for the reactor configuration. In the reactor case, the 131,760 kg of 3100 K corium gives a

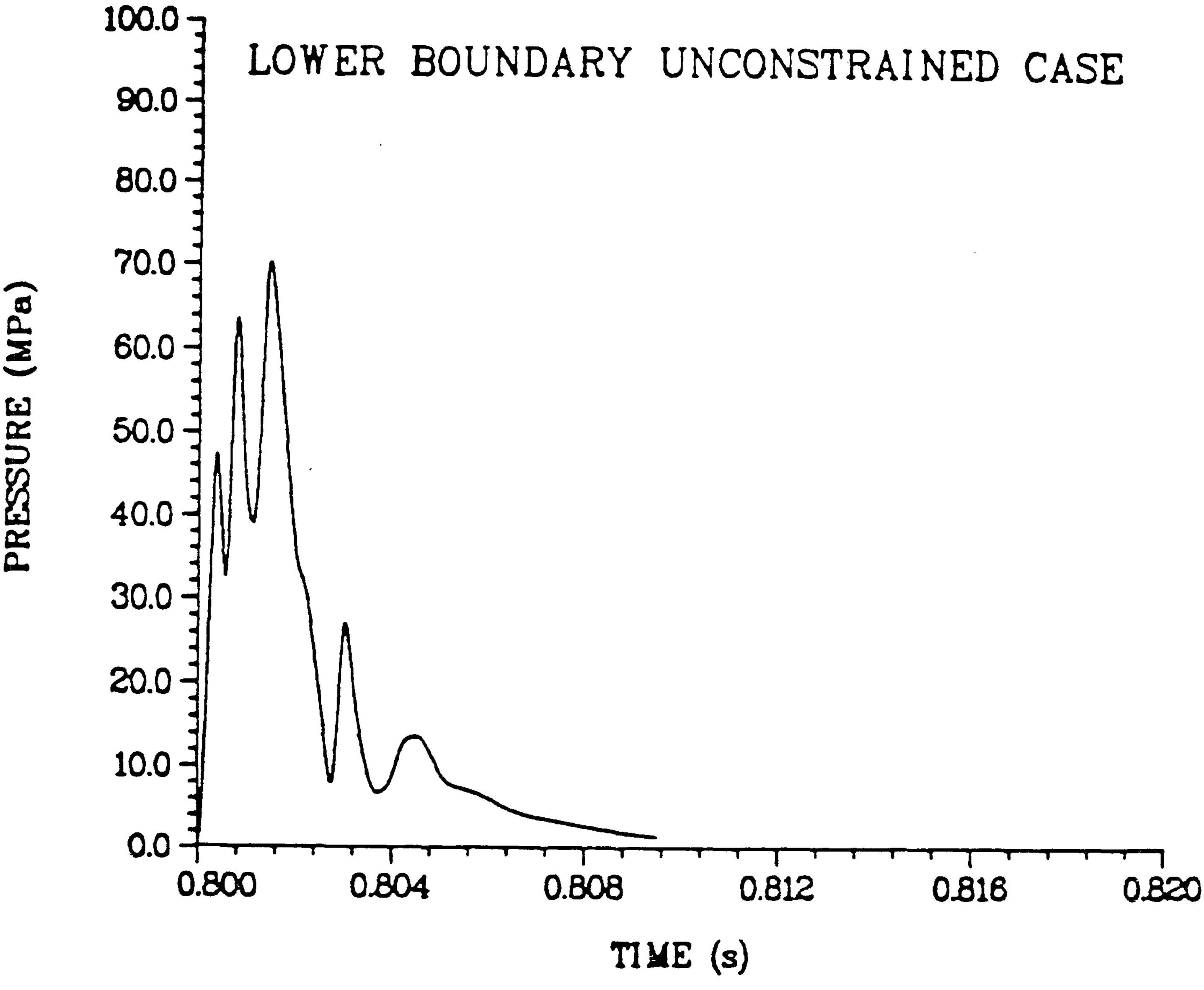


Fig. 22.
Center of bottom boundary, SIMMER-II explosion in unconstrained SEALS geometry.

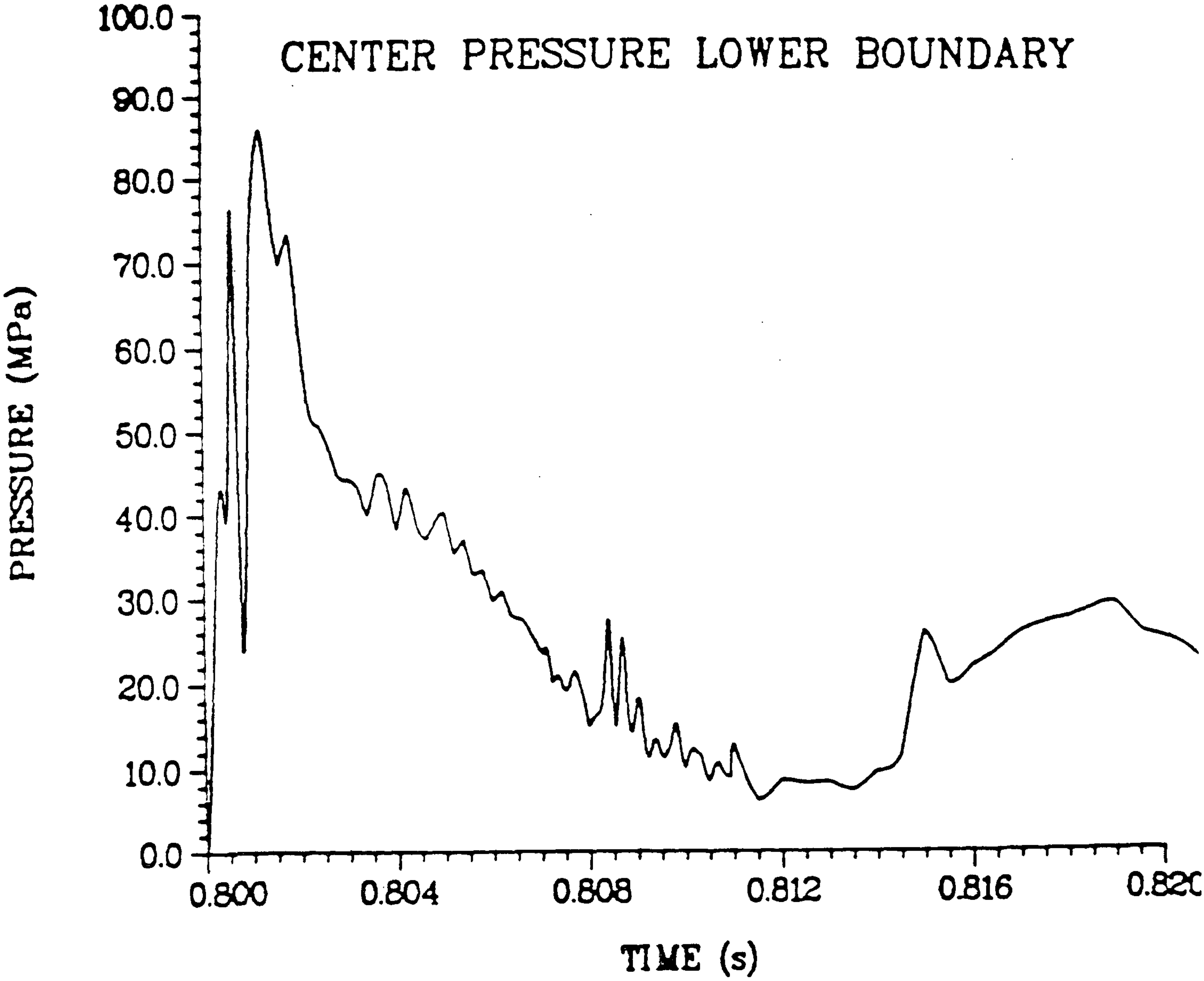
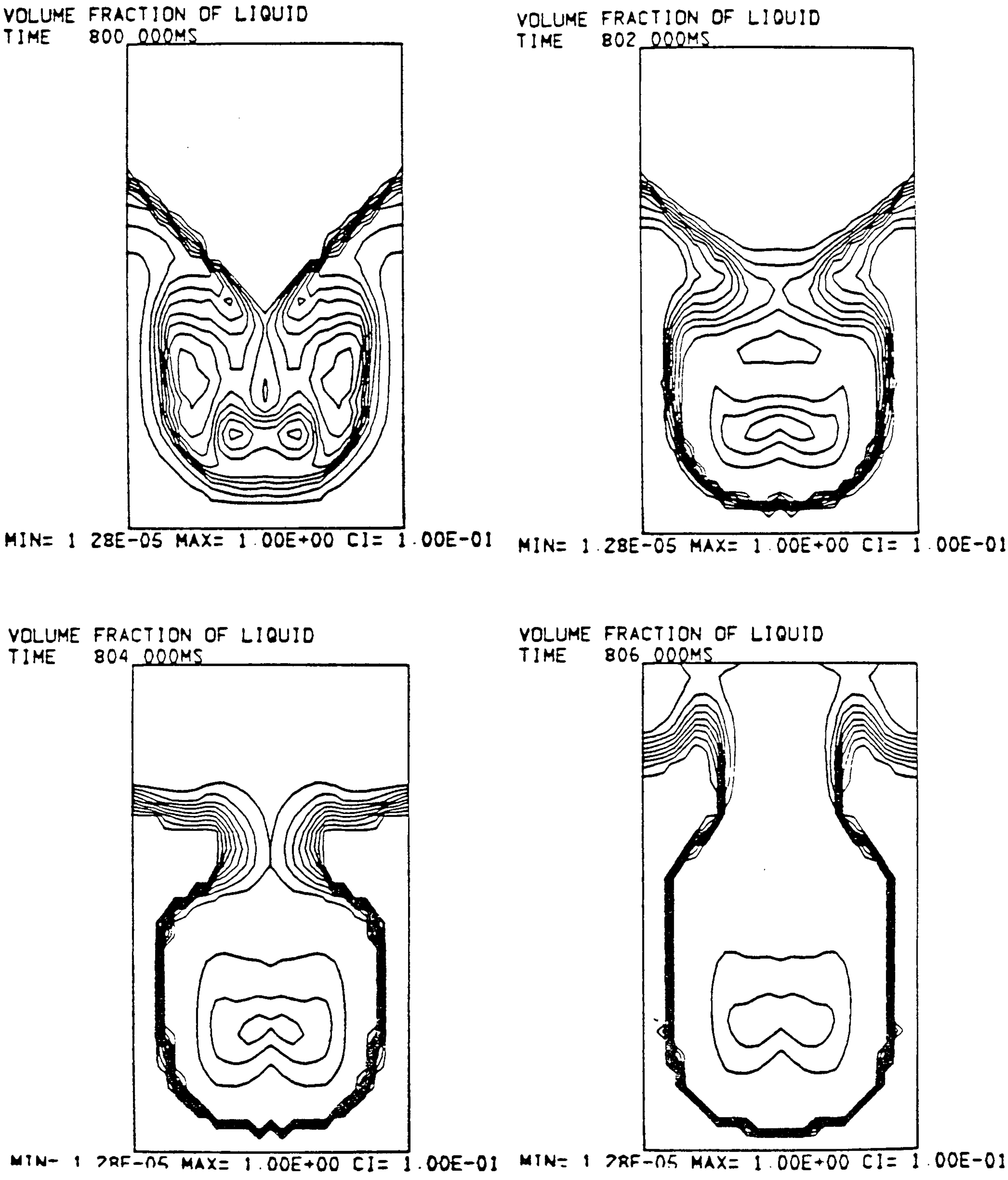
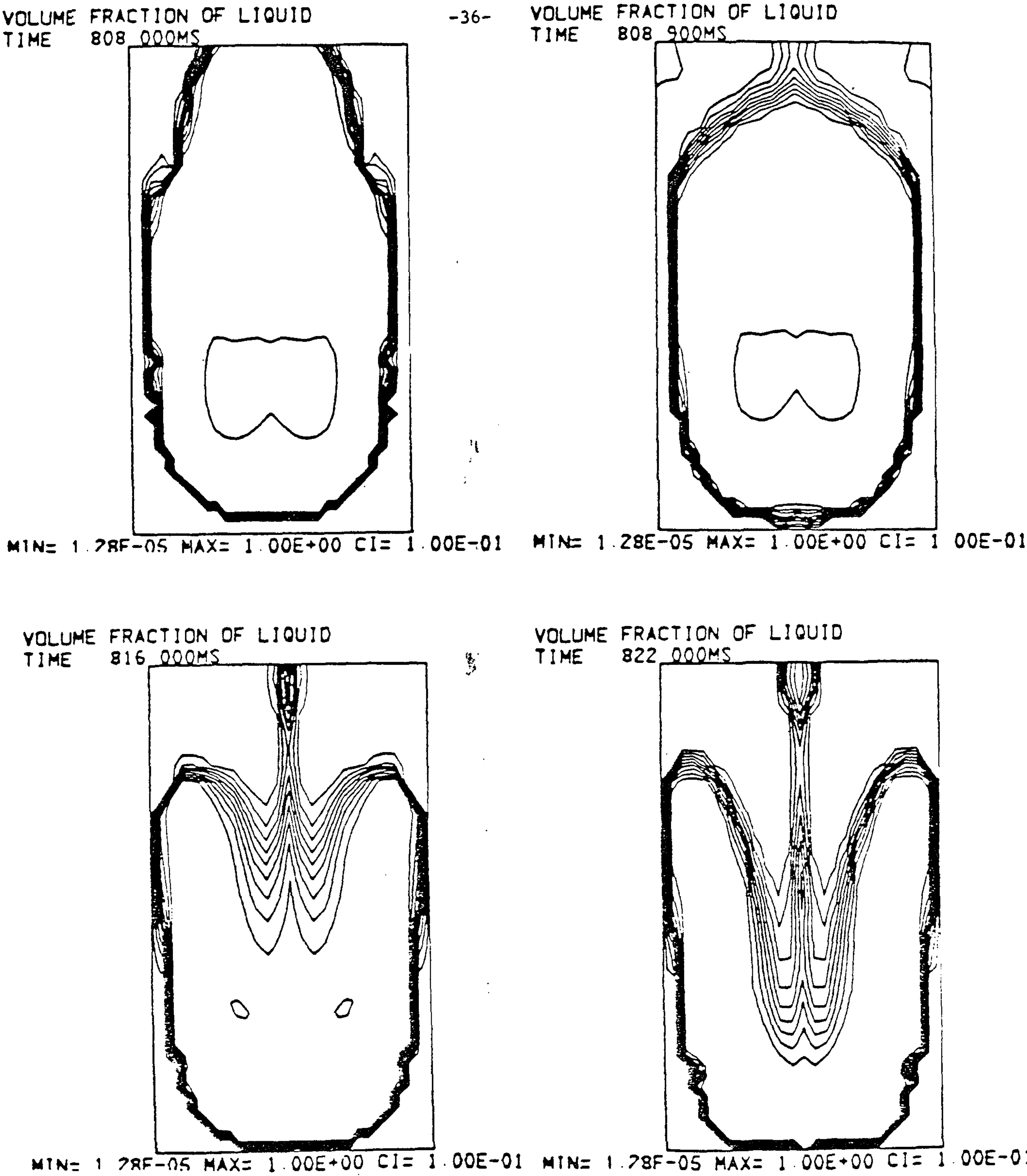


Fig. 23.
Center of bottom boundary, SIMMER-II explosion in SEALS CSQII geometry.



C-2.92

Fig. 24.
Liquid volume fraction plot giving the initial expansion in the SEALS explosion calculation.



C-2.93

Fig. 25.
Liquid volume fraction plots giving head impact and subsequent motion in the SEALS explosion calculation.

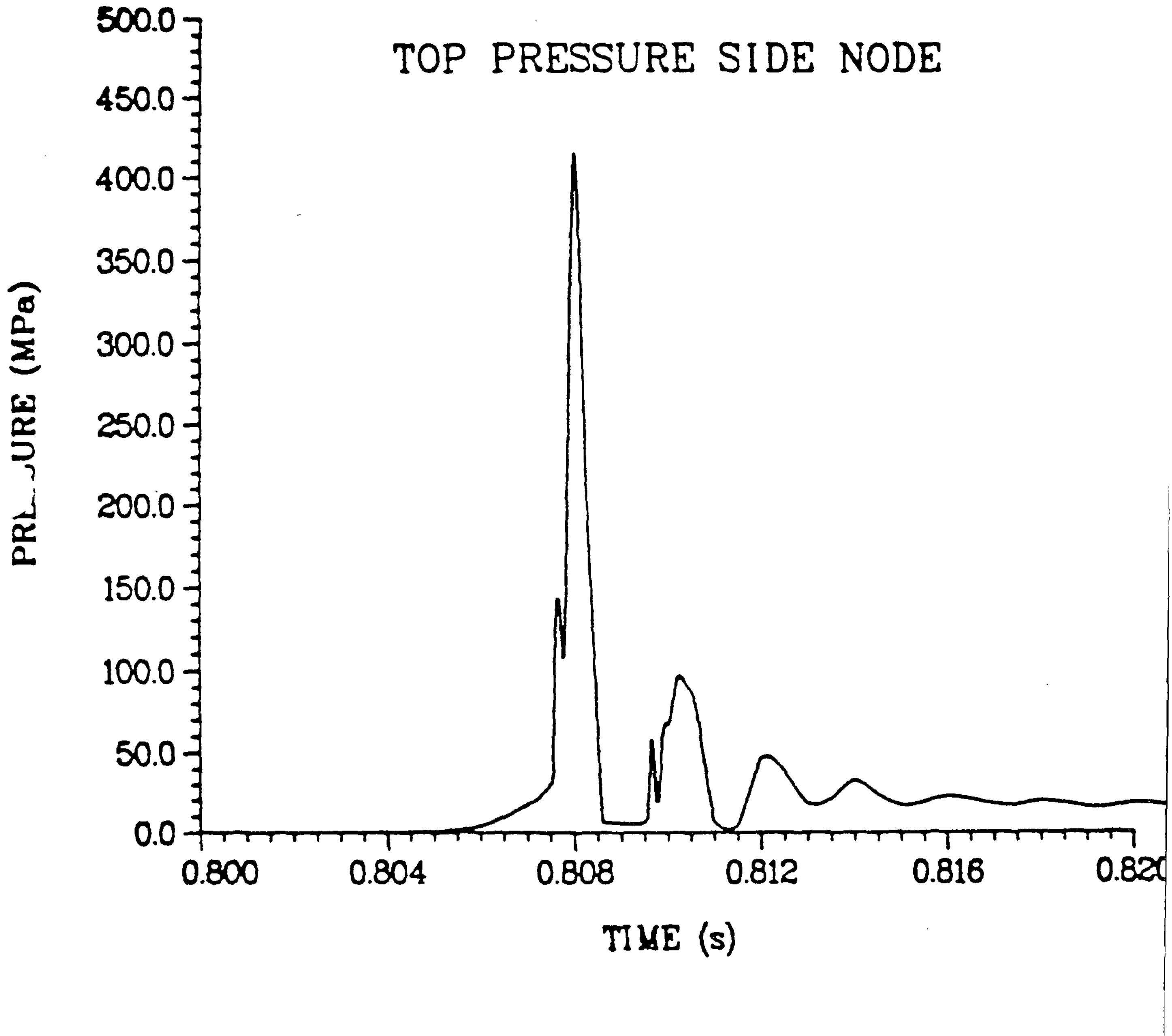


Fig. 26.
Pressure in the upper right corner of the cylinder, SEALS explosion calculation.

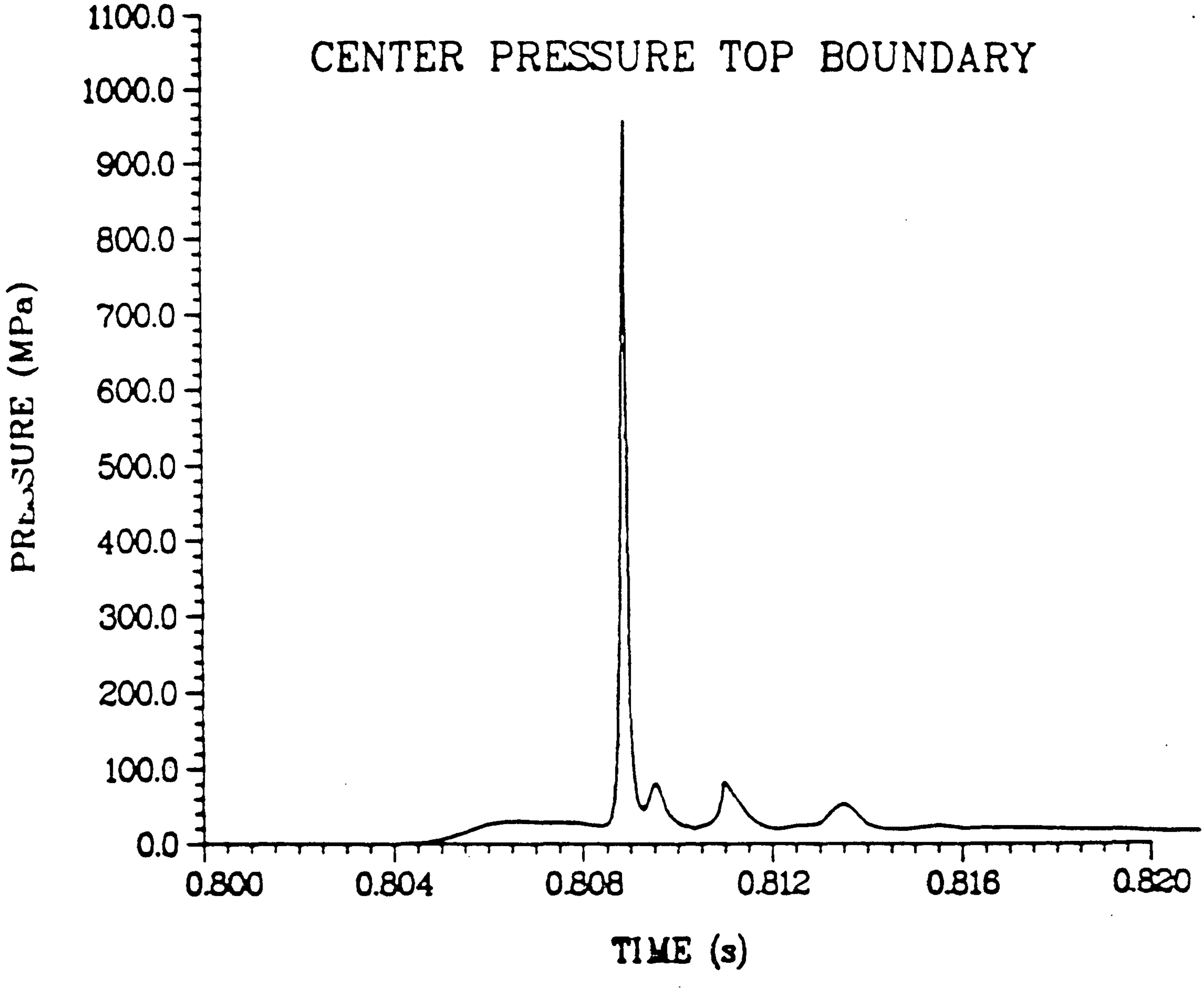


Fig. 27.
Pressure at the top along the centerline, SEALS explosion calculation.

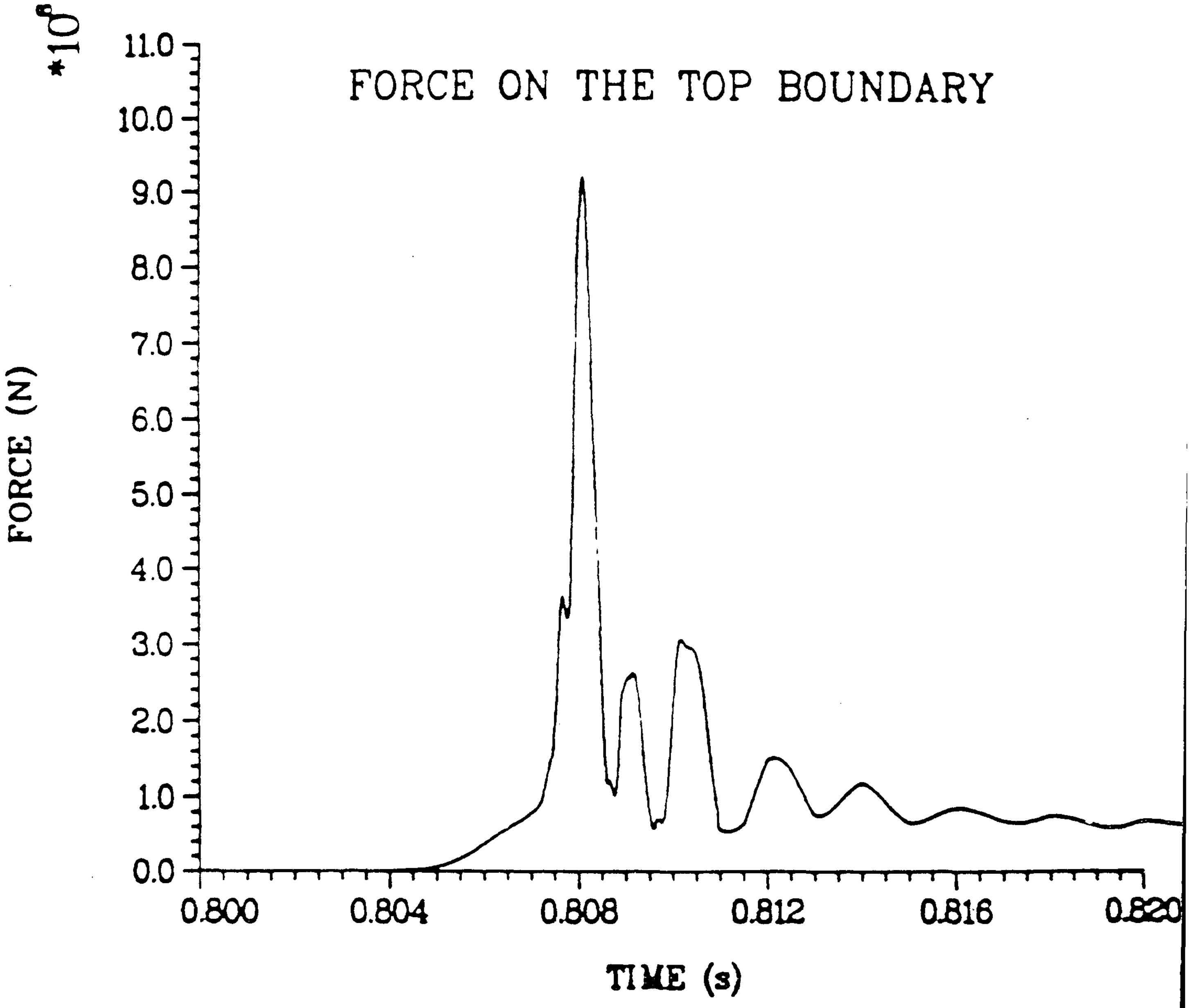


Fig. 28.
Force integrated over the top boundary, SEALS explosion calculation.

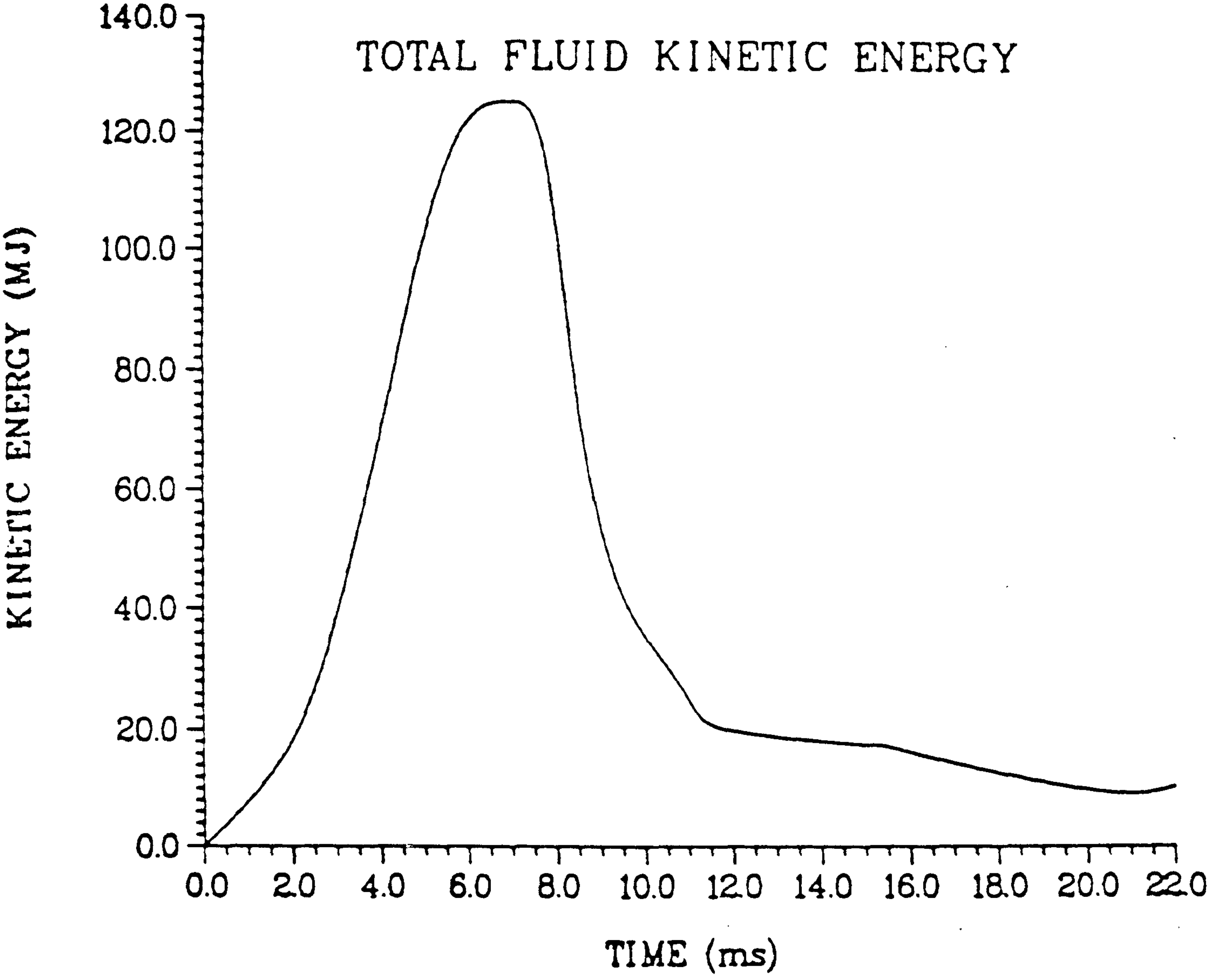


Fig. 29.
Fluid kinetic energy from the start of the explosion sequence, SEALS calculation.

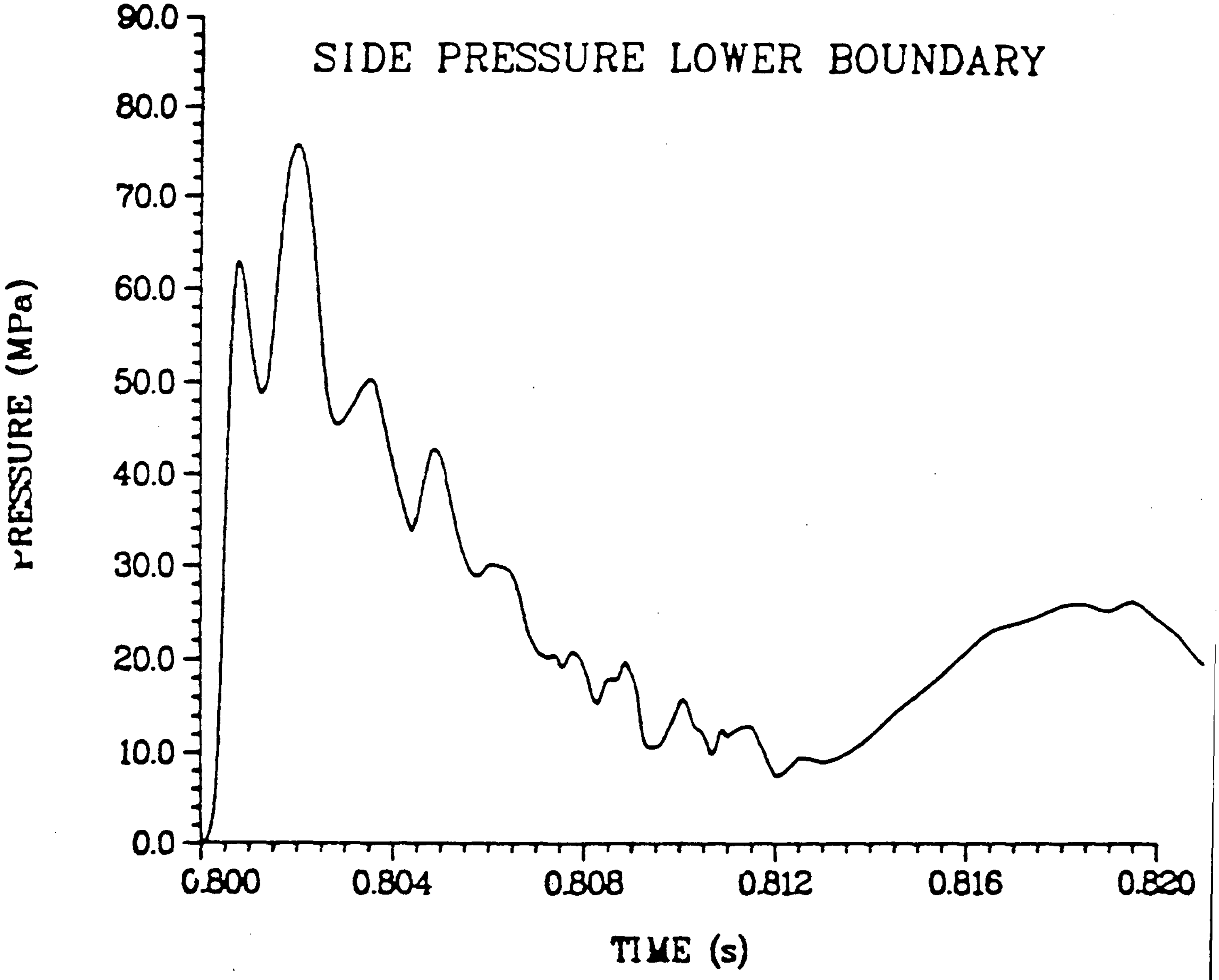


Fig. 30.
Side boundary pressure at the bottom of the cylinder, SEALS explosion calculation.

conversion ratio of 0.64% for a kinetic energy of 1460 MJ. If we examine just the corium that has flowed out of the corium pool, and into the lower plenum, we obtain Fig. 31. It shows that about 40,000 kg of corium are at axial elevations below the bottom of the corium pool at 0.7 s. This 40,000 kg gives a conversion ratio of 2.1%, which is similar to the SEALS simulation. The reactor case does have higher initial transient pressures, probably as a consequence of water entrapment during the premixing phase. The amount of corium actually interacting with liquid water seems to be much less than 40,000 kg. Development of an accepted method of characterizing the percentage of the core mixed in such a mechanistic calculation is desirable. We may be calculating a relatively efficient interaction involving a small amount of corium (compared to 132,000 kg) in a highly constrained configuration. Both cases do not suggest the water quenching effects that are present in the model when simulating currently available intermediate scale tests involving 10-20 kg of thermite.

In conclusion, consideration of large scale tests is desirable for helping substantiate arguments regarding large-scale mixing and arguments that would limit the magnitude of a steam explosion at large scale. Care must be taken that the experimental results are not misleading. Some recommendations to improve the usefulness of the results in this context are given in the next section.

5. Major Recommendations

(a) Analysis — Current one-dimensional quasi-steady-state models, PHOENICS calculations reported by Professor Bankoff, and SIMMER-II calculations suggest important non-linear effects resulting from scaling in considering steam explosions. In our opinion, if large-scale tests are to be performed, a best-estimate computational capability should be developed. This capability should at least have the potential for not giving misleading results in extrapolating from the present FITS tests, to the SEALS tests, and then to the reactor configuration. Results of the previous section cannot be considered a best estimate regarding SEALS behavior. The minimal computational requirements involved are at least two-dimensions, transient hydrodynamics, separate velocities for water and melt, a melt fragmentation capability that depends on the local phenomenology, a realistic equation of state, (probably)

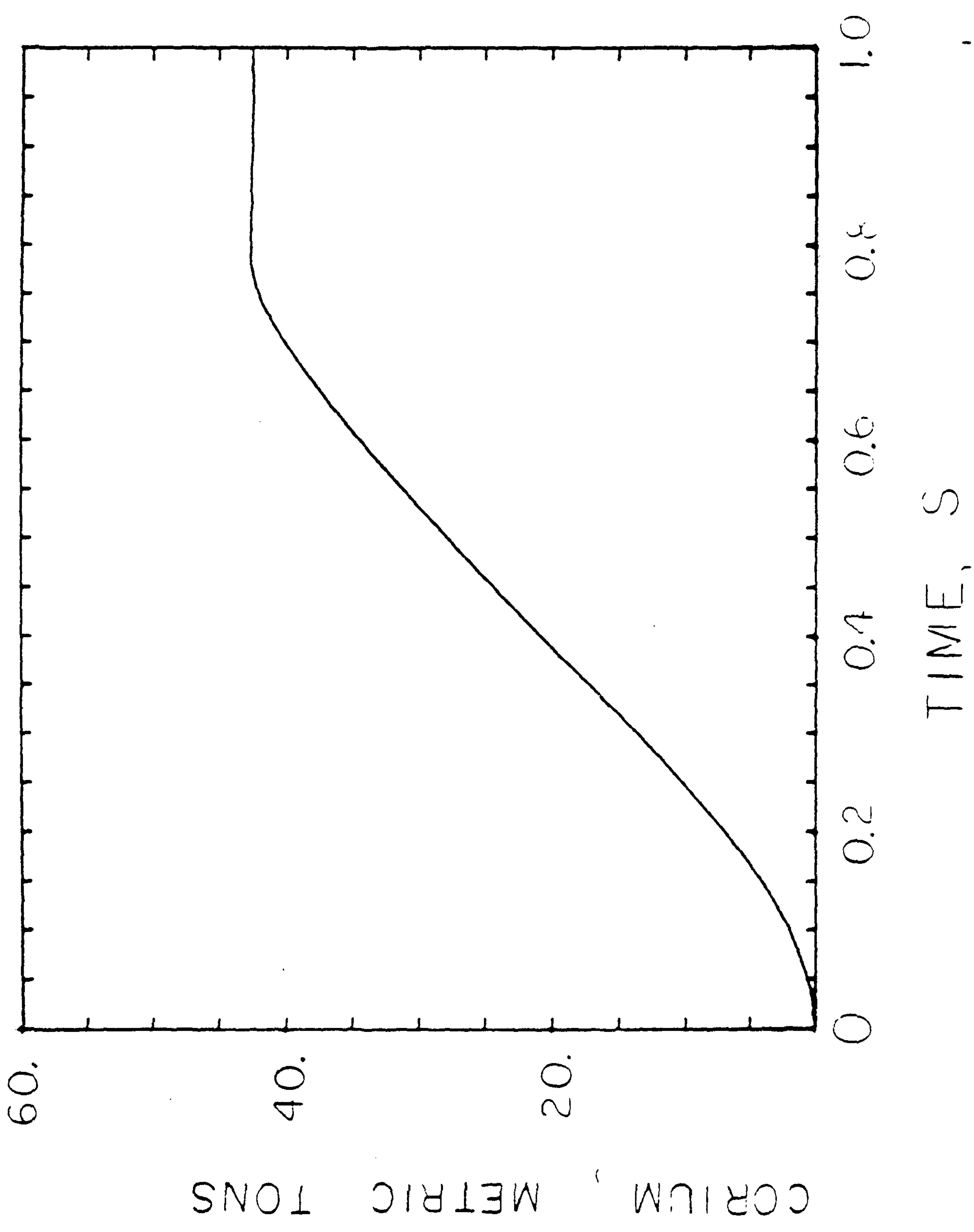


Fig. 31.

Corium below the bottom of the initial corium pool, SIMMER-II reactor coarse premixing problem.

C-2.100

non-equilibrium water vaporization, and (possibly) hydrogen generation to the extent that heat transfer is affected. Such a best-estimate capability was not shown in the SNL proposal, but both SNL[6] and LANL have code development and analysis activities that can contribute to the design and understanding of the experimental results. We recommend these programs be accelerated so that pretest SEALS analyses can be performed. A more narrow window of possible test outcomes than can be obtained by current SIMMER-II calculations is desirable if outside observers are to be convinced that these problems are resolved.

(b) Experimental — Previous SIMMER-II calculations on the Zion/Indian Point study[7] suggested that the inertial constraint posed by the fuel pool reduced sensitivity to heat transfer assumptions. Although some of the modeling exaggerated effects in that study, inertial constraint should be substantial until lower head failure in any reactor configuration where alpha-mode containment failure is of concern. Constraint effects do appear to be important in the reactor calculation presented in this report. Constraint has also been shown to be important experimentally in influencing the conversion ratio in SNL tests.

From this viewpoint, an important non-prototypicality with the current SEALS tests is the lack of an axial constraint. The SEALS results in Section 4 produced a water slug, not the acceleration of a corium pool. We propose the schematic shown in Fig. 32 as a configuration which would use sand as an axial constraint and could also lead to a better estimate of the conversion ratio. The amount of sand could be variable, although adoption of consistent scaling is probably best. Water and steam could escape up the sides and through vents during the coarse premixing phase of the test. If the presence of large-scale and both axial and radial constraint reduces random effects, confidence in our modeling capability would be improved.

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[6] M. F. Young, Personal Communication, Sandia National Laboratories, November 1984.

[7] M. G. Stevenson, "Report of the Zion/Indian Point Study, Volume II," Los Alamos National Laboratory report, NUREG/CR-1411, LA-8306-MS (1980).

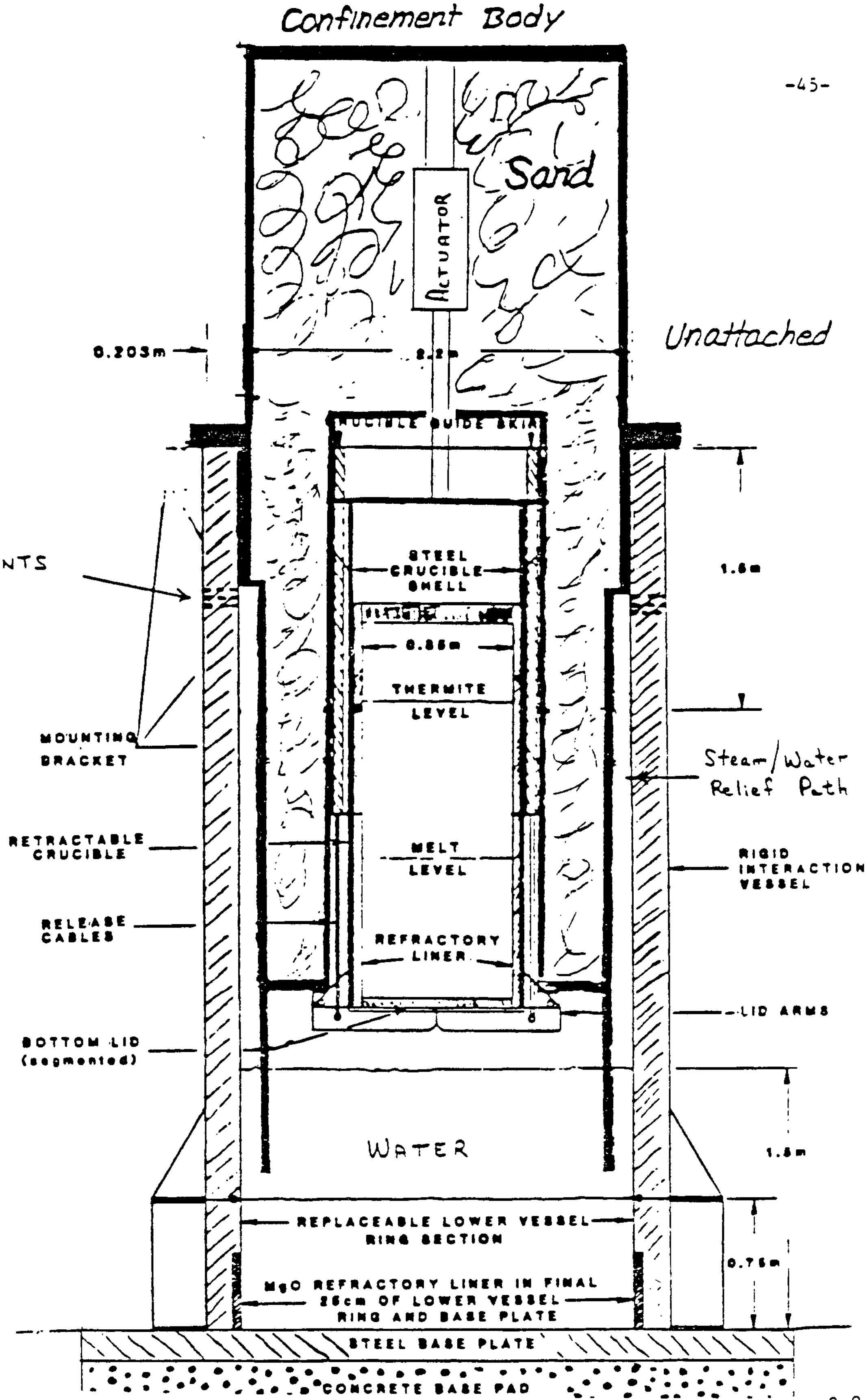

Fig. 32. Proposed schematic SEALS experimental configuration.

C-2.102

6. Other Comments and Recommendations

(a) The one-half linear scale idea is reasonable for setting sizes. However, besides the importance of inertia and other thermite properties potentially influencing the results, we must note that at large scale, mixing is expected primarily at the surface of pouring melt. The PWR equivalent pour mass might more reasonably scale as the surface area rather that the volume?

(b) The conditional nature of SEALS2 clearly depends on mixing limits appearing in SEALS1. The conditional nature of SEALS3 is not as clearly stated. Is SEALS3 basically motivated by a desire to measure the conversion ratio qualitatively expected in SEALS2?

(c) The CSQII calculations are interesting, and we believe reasonable given the extrapolation from the available FITS3B test for the initial conditions. The use of high density iron and the peak 17 MPa pressure achieved at the center of the chamber bottom (which is about the same as in the existing FITS tests) give support to the idea that volume fractions must be different in an assumed large "premixture" if true concerns are to exist on alpha-mode containment failure in a reactor environment.

(d) The most probable ambient pressure when water contacts corium in a reactor accident may be significantly above the Albuquerque ambient value of 0.083 MPa. Available data, FITSX experiments, and Winfrith experiments presumedly will allow pressure effects to be accounted for. The SHIPS tests are to address the issues of extremely high pressure. More information is required to assure that the issue will be resolved. Will an analytic program be added to obtain more confidence on the effects of higher ambient pressure? Will sufficient high quality data be obtained to permit model development to account for scaling effects and simulation of the reactor meltdown situation at elevated pressures?

(e) Water in the reactor case is almost certain to exist at saturated conditions. This may also be the worst case, if delayed triggering furnishes the true limits to mixing. If resources for large-scale experiments are scarce, we recommend priority be given to the use of saturated water.

(f) In the lucite geometry, photography will only give the surface picture. If the iron tank is not present, could gamma- or x- rays provide additional information on the interior of the premixed zone? Or do safety considerations dictate that the source must be placed too far away to be meaningful?

(g) The SEALS experiments are being justified on the basis of correlating the conversion ratio as a function of various parameters, particularly melt mass. For emphasis, assurance that the kinetic energy produced can be adequately quantified is essential.

To briefly recapitulate, in our opinion the proposed SNL experimental program covers more than simply alpha-mode failure issues and should be justified on that basis. Whether results can be expected to truly resolve NRC steam-explosion concerns is unclear. We expect difficulty in relating the proposed experiments to quantitative physics. Adding chaos and confusion from test-to-test scatter will not improve the current situation. However, properly conducted large-scale experiments could reduce uncertainties. We believe the Los Alamos program can be expected to provide some increased confidence on the potential damage from a steam explosion by an examination of post-explosion consequences. Initial explosive configurations must be assumed for these calculations. Because of non-linear scaling effects, large-scale tests could prove helpful in arguments regarding physically unreasonable initial configurations. If large-scale experiments are performed, we recommend an actual best-estimate computational capability be developed to maximize the information obtainable and avoid misinterpretation. Additional axial constraint would be desirable to increase prototypicality and possibly reduce random behavior. Also it is desirable to obtain unambiguous measurements of the kinetic energy developed. Our other minor suggestions are to increase the viability and usefulness of the results from the test program, if the decision is made to proceed. Whether any additional steam explosion tests actually will prove to be cost-effective cannot be rigorously justified at this time.

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STEAM EXPLOSION REVIEW GROUP

Q1. Probability of direct containment failure by Steam Explosion in a PWR

A recent report summarising my work on this topic has been circulated to the Group. This report, (AEEW-R1692)), contains a short review of the phenomena, and comes to the conclusion that 10^{-2} should be considered an upper limit on the range of reasonable estimates of this probability, consistent with current data. This conclusion should be seen in the context of the use of best estimate calculations in risk assessment. It does not imply that 10^{-2} has been established as an upper limit on this probability in the mathematical sense. Rather, it argues that there are a number of reasons for believing this probability is likely to be less than unity, which taken together, indicate that a best estimate value should not exceed 10^{-2}. For further detail refer to AEEW-R1692 "Probability of containment failure by steam explosions in a PWR".

Q2. Comments on NUREG/CR 3369

1. The Sandia study comprised a comprehensive analysis of the effect of uncertainty on key parameters on steam explosion yield, which was then related to vessel and containment damage. The method used has two important features:-

(a) a consistent set of physical models is developed to relate the various steam explosion phenomena to explosion yield and mechanical damage.

(b) a Monte Carlo technique is used to combine the effect of uncertainty on a number of parameters.

The report gives a full account of the modelling and method of analysis, and identifies the data used in the calculations. These features make it a particularly consistent and thorough study.

2. The aims of the study are stated to be to estimate the uncertainty on the conditional probability of containment failure by steam explosion, and to identify important contributions to this uncertainty. The study does not aim to identify a best estimate probability.

3. The approach adopted involves the use of five parameters, each of which is assigned a range on physical grounds. Within this range it is assumed that a flat probability distribution applies. However, it is the choice of limits to the values of the five parameters which controls the overall uncertainty. The parameters fraction of core molten, pour diameter and pour length taken together, allow the mass of core involved in a steam explosion to vary from 0 to 94 Te. The conversion ratio is varied from 0 to 5%. It follows that the explosion yield ranges from zero to 5.6 GJ; a value at which containment failure is highly probable. The calculations presented in the study confirm that the range of yields corresponds to regions of parameter space in which the probability varies over the whole range from 0 to 1. Given that the parameter ranges are intended to cover the full range of parameters possible in the light of current knowledge, I accept this conclusion. However, I would point out that it depends strongly on the assessment that limitations on the melt water mixing process cannot be considered sufficiently well-founded to justify a significantly lower limit on the mass of fuel involved.

4. The series of calculations using different parameter ranges has identified the sensitivity of the failure probability to different parameters. As expected, two of the key factors are identified to be conversion ratio, and pour diameter because of its strong effect on the mass of core involved.

5. The use of the Monte Carlo technique for combining the effects of uncertainty on different parameters is a powerful and effective technique.

However, the difficulty about its use in this case is that it becomes necessary to define a probability distribution for each parameter. Where a parameter is uncertain because it is derived from events which cannot be modelled in detail, such as conversion ratio, or pour diameter, it is particularly difficult to find a basis for such a probability distribution. The Sandia study makes the assumption that there is no physical knowledge available which is relevant to defining the shape of any of the distributions involved. Physical knowledge defines limiting values, eg pour diameter from 0 to 3.4 m, but this physical knowledge is considered not to be of a nature that allows any conclusions to be drawn as to the relative probabilities of different values, eg of pour diameter. In other words there is total ignorance of the probability distribution between the defined limits. As I understand the argument, the use of square distributions is then justified on the grounds that they are as good as any other, in the sense that the full uncertainty range would include the effect of all possible distributions, of which square distributions are a sub-set, which cannot result in an overestimate of uncertainty because it is a sub-set. Summarising, probability distributions are needed as input for Monte Carlo calculations, and in this study square distributions are adopted as representation of ignorance appropriate for an uncertainty study.

This study did not address the problem of defining a best estimate of the probability. In the absence of complete theoretical models of the important events, such a task inevitably involves judgement. Whilst this does result in subjective probability estimates, which are difficult to justify, it may substantially reduce the probability. I think there are two effects where this is the case. Firstly I think there are grounds for expecting the pour diameter more often to be in a low range than a high range. A non-uniform distribution peaked at low values would significantly reduce the proportion of cases in which containment failure is predicted. Secondly the argument that steam flow is likely to make mixing of large masses of fuel with water more difficult than for small masses, has been excluded on the grounds that no firm upper limit can be derived at present. My own opinion, as expressed in my own subjective estimate of the probability of containment failure, is that these effects are important, and that therefore there is a very significant difference between a best estimate calculation of the probability, and the upper limit of 1 identified in the Sandia Study.

7. The Sandia study has established a model which can be used to combine uncertainties on relevant parameters in a logical way. The application of this model has adopted a rigorous approach and excluded any allowance for partially understood phenomena. On this basis, the uncertainty range on the containment failure probability is from 0 to 1. However, it would be of value to investigate the extent to which the upper limit of 1 would be reduced, if alternatively a best-estimate calculation of the mass of fuel involved was made, allowing both for the improbability of large pour diameters and the effect of steam flow on mixing.

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2 November 1984

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16 November 1984

Dear Mr Allen

COMMENTS ON SANDIA STEAM EXPLOSION RESEARCH PROPOSAL

I have been most interested to read this proposal and would like to comment briefly on scope and objectives. Unfortunately time does not permit more detailed comments.

A programme of this magnitude can only be justified if there is an important uncertainty in the assessment of steam explosion effects on the public risk from severe accidents. The current lack of agreement between different estimates of alpha-mode failure probability is strongly indicative that this is indeed the case. In the UK the Atomic Energy Authority is pursuing a major programme of research into Fuel Coolant Interactions, because of concern about the role of such interactions in severe accidents in both PWR and Fast Reactors such as that proposed.

The origin of the current uncertainty is that detailed physical models of steam explosion phenomena have not been developed to the stage at which they can be used with confidence for quantitative prediction. In particular both the initial mixing phase, and the fine fragmentation/heat transfer mechanisms are not well-understood in detail. This makes the assessment of the importance of mass scale difficult and uncertain, since for the materials of interest, experimental data is only available from comparatively small systems. The Sandia proposal identifies this as the prime issue, and tackles it directly by proposing much larger scale experiments. I believe that such experiments offer the prospect of significant reduction in the uncertainty on mass scale effects, hence I strongly support the SEALS proposal, and agree that it should have the highest priority.

To emphasise the importance of establishing the effect of mass scale on mixing phenomena, I think experimental results have demonstrated that conversion efficiency can be of order 5% of thermal energy for small masses of fuel. I know of no reason why conversion efficiency should fall with increasing mass scale, given the same degree of pre-mixing before the explosion. Hence if it is to be shown that major damage is not to result, it will be essential to argue either that not all the core melt available can be involved in a single steam explosion, or that the degree of pre-mixing changes with scale in such a way as to significantly reduce conversion efficiency.

In either case, such arguments depend upon knowledge of the mixing which results from contact between large masses of core melt and water. The SEALS facility would provide experimental data for larger systems, which would be valuable both for validation of calculations and because it would significantly reduce the extrapolation presently involved in assessment of severe accidents involving meltdown of a large fraction of the core.

If real value is to be obtained from very large scale experiments such as SEALS it is important that a a carefully planned programme including a significant number of experiments is completed. One or two ad-hoc experiments at full-scale would be of relatively little value whatever the results. Hence it is important that the SEALS programme is to be supported by intermediate scale experiments, and by a substantial effort on theoretical modelling of the phenomena and the subsequent analysis of the experimental data.

A second important area of uncertainty is associated with the effect of system pressure on steam explosion phenomena. The rather sparse experimental data are restricted to a low range of pressure, but there are indications that it is more difficult to initiate an explosion at higher pressure, and that mixing phenomena are pressure dependent. Trigger phenomena could contribute to the improbability of a large scale steam explosion, if either triggering occurs very quickly (resulting in multiple explosions involving limited mass), or if triggering proves practically impossible above some threshold pressure. hence I support the SHIP proposal is likely to provide data relevant to the assessment of trigger phenomena at high pressure.

The initial mode of contact between core melt and water is expected to be some form of pouring, but as the accident proceeds, two other modes have to be considered. Firstly there is the possibility of multiple explosions leading to different, more energetic and less well-defined modes of contact, and secondly there is the formation of an underlying layer of melt in the lower head. Although data on these phenomena may arise from large scale tests in SEALS, it should be possible to make more specific investigations in existing smaller scale facilities such as FITS or FITS X, particularly into the behaviour of stratified systems. The FITS X programme would also provide valuable information on more detailed topics such as the influence of melt composition. One aspect of composition which has received little attention is the influence of a highly chemically active metallic phase, such as will occur if significant metallic zirconium is included.

To summarise, I strongly support the proposal for further experiments at Sandia, aimed at evaluating the importance of larger mass and higher pressure on steam explosion phenomena. These are also the objectives of the current UKAEA programme at Winfrith. The proposed Sandia and Winfrith programmes are complementary in the sense that the work at Winfrith is at intermediate scale, whereas the Sandia programme is centred on very large scale mixing experiments, with some single droplet investigations of pressure effects.

Yours sincerely

Tony Briggs

Tony Briggs

INTRODUCTION

In my view, until a greater understanding of the physical processes are achieved, the application of rigorous probabilistic analysis is not possible. This does not mean that one cannot follow the steps that one would imagine are necessary to achieve a damaging steam explosion and use engineering judgment to come to a conclusion. This approach is a combination of categories one and two as described by Berman [5]. This procedure has been used by Briggs [2] and by Squarer and Leverett [3]. Such a procedure clearly delineates the important steps along the way to a steam explosion. Although consensus may be difficult to achieve, the thinking process will be transparant to others to judge.

Experimental reproducibility of vapor explosions is not possible. The stochastic nature, in my view, stems from the melt conditions at entry into the coolant. Small perturbations in shape and motion result in differing growth due to Taylor Instabilities. The differing Taylor waves result in differing initial melt-coolant contact that in turn yield local shock waves of differing characteristics. To even expect reproducibility only demonstrates a certain naiveté. It would be impossible to reproduce the small perturbations that lead to an interaction.

After a number of years of research, I still find that all we can say are that various phenomena are "more" or "less" likely than they were thought to be before we started. A few impossible occurances such as explosions when the coolant is saturated and explosions of stratified melt-coolant without an artificial trigger have been found. We now know a "little more" than before but certainly not enough to be "sure." To go into detail about our lack of knowledge and why certain efforts have not yielded information claimed by the experimentors would serve no useful purpose here. It is not, however, impossible to arrive at a reasonable estimate of the chance a damaging steam explosion might occur. I will follow the approach taken by Briggs [2] and by Squarer adn Laverette [3] in doing so. (Ross question number 1) Others such as Bankoff [1] have taken such an approach but, in my view, only Briggs has given a clear explanation that is consistant with the state of understanding. This will be followed by a brief review of NUREG/CR-3369 (Ross question 2).

PROBABILITY OF CONTAINMENT FAILURE

Following the approach outlined in references [2] and [3], the possibility of containment damage by a steam explosion is assumed to involve "many stages whose individual probabilities can be separately assessed, and suitably combined." Whereas Briggs starts with a large molten pool, here the earlier stages will be considered. This changes some of the stages from those considered by Briggs but leaves the approach essentially the same. The stages of the steam explosion event are assumed to be

(i.) The core melt process leading to a large pool of hot (well above the melting point) molten corium.

(ii.) The large pool of molten corium penetrates the fuel bundle nozzles, the lower core-plate and the core support ring as a large coherent mass.

(iii.) The large coherent mass flows rapidly into the lower plenum.

(iv.) The lower plenum ocntains a large enough mass of subcooled water.

(v.) The large mass of corium mixes with the water without premature triggering.

(vi.) A triggering event produces a shock wave that sets off the thermal explosion.

(vii.) A slug of water with sufficiently low viod fraction is located above the region where the thermal explosion takes place.

(viii.) Sufficient acceleration of the slug takes place to impact the upper head with enough momentum to launch it through the containment dome.

Following Briggs or Squarer and Leverett, arguments will be given for the probability of each stage separately even though there are interdependencies. An incredible event will be given a probability of 0.1 and a sure event a probability of 1.0 This will bias the results on the conservative side of this reviewer's judgment.

(i) Core Melt Process. The overheated core will begin to melt in the high power zone and eventually melting will occur. As the molten pool grows it will interact convectively with the crusts that form its boundaries. The crusts in turn interact with the cooler regions of the core by radiative and convective heat transfer. The convection within the pool could very likely be dominated by the melting process that allows it to grow. The material being melted only needs to have a density that is 10% greater than that of the molten pool to completely dominate the convective process. This will lead to very good thermal coupling of the pool to its boundaries and temperatures very close to the melting temperature of the materials being penetrated. One must next address the importance of the molten pool temperature.

Experimental studies of iron-oxide, by Sandia (NUREG/CR-2481, pp 64-67), very close to its freezing point were carried out with interesting results. If the trigger is large enough, an interaction takes place. One must, however, look closely at such a statement before drawing any conclusions. The interaction had to be triggered. The trigger pressure pulse was 1.24 MPa and the resulting interaction maximum pressure pulse was 1.4 MPa. It would be difficult to sort out how much of the 1.4 MPa was a result of the initial pulse and how much was thermal. In any case, the interaction did not appear to contribute significantly to the initial pulse. One would have to carefully analyze the pressure-time traces to draw strong conclusions.

The roughly linear relationship between peak pressure and melt temperature suggests that the explosion potential is related to thermal energy above the melting point. The experiments of Wright using molten aluminum were noted by Berman to follow a similar behavior. If this is the case the thermal hydraulics of the core melt process must take on more importance. Berman argues that the 1.4 MPa is an indication of explosivity even at the melting point. This reviewer would argue that needing a trigger of 1.24 MPa magnitude favors the earlier argument.

There are other qualitative evidence that molten materials near their melting point do not result in steam explosions. Volcanic flows underwater are thought to be at temperatures very near their melting point. There are very seldom any explosions and those that occur do not seem to propagate. The high temperatures (white hot) result in high radiative heat transfer and the low thermal diffusivity yields rapid crusting that protects the melt from intimate contact with the coolant.

It is my opinion that the heat transfer to the boundaries of the pool will be high and the temperature will be near the pool freezing point. It is possible that it will be not unlike a slag. This leads me to conclude that the probability that the pool will be hot enough to result in a significant interaction is $P_i = 0.2$.

(ii) Molten Pool Penetration of Lower Vessel Region. The molten pool must melt its way through a large amount of steel before it can interact with the water in the lower plenum. The first steel encountered is the fuel bundle bottom nozzles. The bottom nozzles mate with holes in the lower core plate. The lower core plate is permeated with small holes that look on the order of an inch or two in diameter. Next the diffuser plate is encountered. It has fewer but larger holes. Finally the material will encounter the core support forging which is a massive piece of steel. The volume below the core support forging contains a forest of instrument guide tubes and the secondary support assembly.

It would be miraculous if all the steel described above could be penetrated in a coherent way. Additionally, the large amounts of relatively cool steel and high heat transfer coefficient (my guess) will result in further justification for the assumption that the molten corium will be near the solidus temperature of steel. The question here however is what the probability of a coherent massive penetration should be. It is my opinion that it will be relatively low. In the spirit of 0.1 being incredible, the probability for a large coherent penetration is taken to be $P_{ii} = 0.2$

(iii) Rapid Entry of the Molten Pool Into the Water. According to Briggs [2] , at least 6 Te must enter the pool in a time on the order of 1 second. Bankoff [1] believes the time must be less than 10^{-4} seconds. Briggs further estimates that a flow rate of 6 Te/s would require an area of 0.1 M^2. Considering the tortuous path the material is following, the chances of achieving such a flow rate are small. If one uses the time given by Bankoff, the chance of getting it into the water quick enough are incredibly small. The probability of large scale rapid entry into the water is taken to be $P_{iii} = 0.1$.

(iv) Availability of Enough Water. The hot molten pool will most likely enter the pool slowly. If a high rate of entry is to occur it will be after some materials have already entered the pool. With the route for steam blocked from above it can only leave the lower plenum via the doncomers and outer periphery of the core where it may be undamaged. This last path, of course, brings up the question of radial progression. It is my view that by the time the molten pool begins to penetrate into the lower core plate it will have blocked off most of the core cross-sectional area. In any event the result will be a strong possibility that the water will be saturated and a good fraction of it held up in the downcomers when the molten pool arrives.

Arguments have been given that radiative heat transfer will be so high that the water will be heated to saturation and significant amounts vaporized. Such arguments do not hold. The low thermal diffusivity of molten corium will result in crusting which decreases the heat transfer to moderate valves. Further heating from above by any mechanism is not very effective.

The arguments against the availability of water are more speculative than most. Here the probability is taken to be $P_{iv} = 0.75$.

(v) Mixing of Molten Corium with Water. The arguments given by Briggs [2] are very convincing. Here he separates high pressure sequences from low pressure sequences. Then he argues that spontaneous triggering at low pressure will result in blowing part of the molten pool out of the way before it can enter the water. The high pressure sequences will yield circumstances where triggering is more difficult and as a result a better chance for more molten material to enter the water before it is triggered.

It is my view that objects like the instrument tubes will help the triggering process and result in less time for mixing. The resulting probabilities would therefore be lower. Lack of sound arguments, however, lead me to use the values suggested by Briggs,

Low Pressure $P_v = 0.3$

High Pressure $P_v = 0.7$

(vi) Triggering of the Event. At low pressure available evidence indicates that triggering an event is highly probable if not sure. Therefore the low pressure probability is chosen to be low pressure $P_{vi} = 1$.

As pressure is increased, triggering becomes monotically more difficult. It cannot, however, be precluded and each new experimental study brings out some forgotten factor. Until a large data base for true corium exists, the probability is taken to be artificially large, high pressure $P_{vi} = 0.75$.

(vii) Water Slug Above the Interaction Zone. The existence of a slug of water above the explosion that can be accelerated in a coherent way is difficult for me to conceive. Simple 3 - 50 g experiments at UCLA demonstrated that Taylor instabilities break the slug into drops very quickly. Even if a slug exists to be accelerated, it will most likely not reach the top as a slug. The GE suppression pool model for water impact might be a helpful tool to assess this phenomenon.

Typical experiments in the laboratory allow the melt to fall from above into the coolant without interference. Triggering typically takes place with the melt somewhat submerged. This places it, at least in part, beneath water and there is a slug to be accelerated. RPV lower plenum hardware will change this and the melt will have a difficult time entering the coolant without early rapid steam generation. The existence of a slug of water above the interaction zone that is not highly voided is thought to be improbable.

Even though I think my arguments are sound, I have a vague recollection that a spherically expanding bubble does not result in Taylor instabilities. For this reason, the probability for a good size slug to exist above the interaction zone is taken to be $P_{vii} = 0.6$.

(viii) Containment Failure. Briggs [2] notes that Gittus concluded that at least 3 GJ would be needed to fail the containment. For a 3 GJ steam explosion the 6 Te of molten corium would have to interact with extremely high and quite improbable efficiency. The probability of 3 GJ resulting from a 6 Te high efficiency ($\eta \sim 30\%$) interaction or a 50 Te 4% interaction is taken to be $P_{viii} = 0.1$

Summary of Probabilities. Calling the estimates made in the above arguments probabilities is an insult to those who deal in statistics. Rather, engineering judgment (mine) has been invoked to choose a set of numbers that will give the fraction of core melts that might result in a serious steam explosion. The resulting "chance" of a serious steam explosion is

Low pressure $P = 5 \times 10^{-5}$

High pressure $P = 10^{-4}$

The two sets of numbers are close enough to one another that I would use 10^{-4} as the conditional probability of containment failure by steam explosions. The two sets of numbers probably should differ more appreciably but lack of physical understanding and conservatism in the estimates of their parts make further differentiation impossible. I have not distinguished between PWRs and BWRs because I believe the differences are swamped by the ignorance factor. On a purely intuitive basis I would argue that the probability of a containment rupturing steam explosion in a BWR is a decade lower than in a PWR.

REVIEW OF NUREG/CR - 3369

An Uncertainty Study of PWR Steam Explosions

Use of Monte Carlo methods to address physical phenomena without meaningful distributions will not yield meaningful results. This is particularly true when the various stages along the way to a steam explosion are assumed to have their own probability distributions. They are not independent. When one goes through the same process using an engineering judgment type approach such as was done by Briggs [2] or by Squarer and Leverett [3], one integrates the interdependencies even though values of the probabilities for each stage are given. The Monte Carlo approach yields final results that may include many physically impossible or inconsistent combinations. Such an approach would have to have a large number of conditional probabilities built into it before it could equal the engineering judgment approach. By this, I don't mean that the Monte Carlo approach shouldn't be pursued. Rather, today it just isn't being done in a believable way.

The introduction to the report contains comments about how improvements to a similar approach were made. The claim is made that a more realistic limit is placed on the energy dissipation with the reactor upper internals (the new limit is lower). This is done without addressing the all-important issue of initial pool characteristics (amount and temperature) or slug acceleration physics. Playing games with assumed initial distributions such as amounts of melt participating and explosion efficiency yield nothing but unnecessary controversy. A lot could have been done that would have been more productive.

In the following paragraphs I will comment on the modeling done that was the basis for the Monte Carlo analysis. I will only comment on those where I think meaningful analysis could be accomplished. The conclusion I reached after carefully reading the report was that I gained nothing. There are too many loose ends for the results to be considered meaningful. The numbers in parenthesis refer to section numbers in the report.

Fraction of Core Molten (3.2). It seems to me that before one tries to argue for some fraction of the core being molten one ought to do some calculations. If one postulates that the molten pool is surrounded by a crust then calculational tools are available to calculate heat transfer to its boundaries. To just speculate, then give it meaning is not science; it is science fiction.

Pour Diameter (3.3). It would have been helpful again to have had some calculations done. One can assume almost anything. The large number of support columns will act as fins. One wonders what the heat transfer rate will be and how much the downward penetration of the melt will be retarded. These are simple calculations but can give a great deal of insight into the process under study.

Radial migration of the melt front until it penetrates the core barrel is something that is amenable to calculation. Mayinger's early studies on molten pool behavior in the lower plenum certainly demonstrate this. Uniform probability distributions of pour diameter is again a copout. Some simple analysis certainly would have allowed some reduction in the range. ANL has a code that, although primitive, could have been used for sensitivity studies. Within reasonable limits, the problem is only two dimensional (r, Z) and codes certainly exist for this. One could, for example, use a modified version of SOLA 2D.

<u>Pour Length (3.4)</u>. With all the steel beneath the core, I would argue that the smaller pour length would be more probable. Further if the water is saturated, boiling and steaming will be vigorous. At atmospheric pressure the water in the lower plenum will be saturated. For the high pressure sequences it "may" be subcooled. Most likely it will be saturated as well. Although I agree with the reports observation about explosion potential, even though saturated, there are other considerations. For example, the increased vapor production will change the entry pattern of the pour. The Henry-Fauske arguments use thermal radiation to achieve high steaming rates. This is incorrect because of the low thermal diffusivity of the melt. I am convinced that the water being saturated will change the course of the process dramatically. I don't know what it will be. Again, scoping calculations would be invaluable. One could then eliminate impossible or unlikely scenarios. Without having done this, I again find arguments for a uniform probability distribution unsatisfactory.

<u>Mixing Limitations (3.5)</u>. I agree with the arguments against previous analysis that have lead to limited mixing. A great deal more understanding of the mixing process is needed before one can limit the mixing process. Here, I think we must accept that what gets in could mix. Of course, all the support columns and instrument tubes may play a role in reductin or limiting the mixing -- but we just can speculate.

As mentioned in my own analysis, mixing is probably due to local Taylor waves coming into intimate contact with the coolant. Prediction of their amplitude depends on what the initial disturbances are. Even in the simplest laboratory experiment (with the scale equal to a Taylor wavelength) this is essentially impossible.

<u>Conversion Ratio (3.7).</u> Arguments about heating gases (steam) and their contribution are incomplete. If I am accelerating a slug of fluid as a result of the thermal explosion, I would think that the primary contribution would be the initial steam production. Calculations could have been carried out to establish what effect the partitioning of energy between thermal energy of the gases and initial propulsion of the slug would have on the final outcome.

<u>Heat Content of Fuel (3.8)</u>. Here again analysis would be helpful. Certain physical arguments are given earlier that lead to molten pool configurations that are calculable. One should do such calculations so that a consistent (conditional probability) set of distributions will result. Without such calculations one really must broaden the temperature range used to include the boiling point of the corium. Without calculations, I cannot exclude it. This would considerably increase the heat content. I don't believe this limit and only mention it to bring out the need for analysis.

<u>Fraction of Remaining Melt Above Explosion (3.9)</u>.

<u>Fraction of Water Above the Explosion (3.10)</u>. No mention of the downcomers--water could easily be blown into them and reduce the explosion effectiveness. Further, interactions below the massive core support forging would cause the explosion to become quite incoherent in its ability to accelerate a slug of water above. There would be choking in many of the restricted passages as well. This, I think could have a dramatic effect and is calculable.

<u>Slug Composition (3.12)</u>. Acceleration of a layer or slug of water from below by a gas has been studied by many. Although not fully understood, it is not as uncertain as stated. GE faced the same kind of questions in their suppressoin pool work. GE actually has a model for the breakthrough process. The accelerating gas can acuse Taylor instabilities as has been shown by myself

at UCLA and many others. Somehow this thermal-hydraulic information must play a role in our deliberations. Here, the higher the acceleration, the more rapidly the slug disintegrates.

Energy Dissipation by Vessel Internals (3.13). When one considers vessel internals there are factors other than just dissipation. First, one must know where the explosion is taking place then one must look to see whether the expansion process can occur unimpeded. I have trouble imagining the kind of piston process described. For example, if the explosion is below the core support plate, all the little holes will become choked. Not only will they be choked, but the flow through them will be like overexpanded jets which is a rather inefficient process with many shocks occuring in the plume. The jets will blow the slug apart as well as significantly reducing the propulsion capability of the thermal explosion.

Slug Impact Model (3.14). Arguments given about slug impact pressure being puc are incomplete. One only achieves puc when the slug surface and the impacted surface are exactly parallel and even then only for a time on the order of a millisecond. The domed head of the vessel along with all the internal hardware eliminate this from ocnsideration. Again this is an area well studied by both NRC and GE.

The force on the upper support plate will be the core area less the flow area times the pressure for a brief instant. Drag resulting from flow through the orifices should be calculated.

Summarizing Remarks. If this were a paper I received for review, I would reject it. It is too filled with supposition and lacks physically based analysis. In many areas, of course, we can't do the analysis, but in others we can. Without more depth, I can only conclude that the study was a computer exercise and has little bearing on nuclear reactor safety. The structure created for such analysis, however, is a good one. It seems to me that one now has to improve it by introductin analysis based conditional probabilities and replacing certain probability distributions with analysis.

Ivan Catton

SCHOOL OF ENGINEERING AND APPLIED SCIENCE
LOS ANGELES, CALIFORNIA

DATE: 16 November 1984

TO: Alan Wong

FROM: Ivan Catton

RE: SANDIA Plan for Resolution of the Steam Explosion Issues

CC TO: Alan Cartis

Unfortunately I don't have a clear statement of the NRC research plan to resolve steam explosion issues. I was not at the 4-5 June meeting in New Orleans where Marshall Berman made his case for their recommended plan. I will try, however, to comment on the plan as stated in the package from the meeting.

The SANDIA recommended plan is the following:

1. Assess current state of knowledge for all issues;
2. Based on this knowledge, determine most important experimental and modeling uncertainties.
3. Plan and execute an "efficient" test matrix.
4. Incorporate tests results into new models and codes.
5. Assess uncertainties in model predictions. If "sufficiently" low, terminate further experimental and analytical FCI research. Move to risk assessment phase.
6. If uncertainties are still too high after steps 3,4,and 5, expand test matrix and repeat the process.

One cannot fault such a plan. If executed it could eventually lead to some sort of reasonable conclusion. It is so vague that detailed comment is impossible. I will, however, comment on copies of various aspects of the above general approach. It should be noted that, in my view, no clear statement of the state-of-the-art (knowledge?) exists.

To highlight a few holes in our understanding, consider the following observations:

1. Molten lava flowing under the sea seldom results in massive steam explosions.
2. The thermal explosivity of molten NaCl is very surprising.
3. A saturated pebble bed at the bottom of the water pool seems to inhibit thermal explosions.
4. The thermal explosion poteneial seems to decrease linearly to zero as the melt temperature approaches its freezing point.
5. A very viscous material seems to have much less potential for thermal explosions.

With a bit more time, I could probably list another dozen or so such observations. In the face of the requirement -- resolve the issue -- questions such as the above need to be addressed. Frankly, I don't see where in the program that this is being done.

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To be more specific, out of the above observations I conclude that surface radiative heat transfer and crusting are very important. The molten material diffusivity is an important contributer to the crusting behavior. This means that without some understanding of how the prototypic material bahaves one will never reduce the uncertainties. Similarly, molten pool viscosity (a strong function of the mix of materials and temperature) must be known. All experiments should report both the molten pool viscosity and thermal diffusivity, melting temperature and crusting characteristics. Our state of knowledge (at least mine) is so primitive that I am not sure what crusting characteristics are. I can think of some examples but without some effort would probably miss the key ones. In many respects, study of steam explosions is not unlike study of turbulence. The basic phenomena is very complex and it may be centuries before enough computer horsepower and physical understanding exist for we can make meaningful first principle predictions. This does not mean we don't continue to try. Rather, for engineering purposes we use empirical relations that have stood the test of time. Here, I think we must do the same.

The proposed SANDIA research plan is for the most part experimental. It consists of five parts of which only one is analysis or modeling. The first part (SEALS) involves construction of a facility capable of handling 2000 kg experiments. In my view, just bigger is not reason enough. Before I could agree to such a test series, I would need to know more about "the question" to be answered and why they felt that such a test series would answer it. For example, have we fully resolved the question of what corium should be used and why? If we have, I have not seen it. Further, there is the mode of molten material entry (sporadic, continuous, etc.) that may be as important (maybe more) as the type of corium. A number of experimental variables are listed. It is the fixed parameters (Item III on the view graph) that concern me.

Element two of the series addresses steam and hydrogen generation and debris bed characteristics. These are important aspects that need to be calculable. Unfortunately the major drawbacks of the facility lead me to conclude that its usefulness is marginal. Without a great deal of convincing, I would argue that it should not be funded.

The third element of the SNL proposed research program is to investigate thermal interactions at high pressure. Small scale experiments will be conducted. Here no indication of how small was given. If the scale is too small (less than a couple of Taylor wavelengths) the results will be only of academic interest. Further, the melt composition needs to be carefully considered. If the scale is large enough and some thought is given to melt composition, mode of entry and a

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carefully calibrated trigger is used, I would recommend that these experiments be pursued. Here I must admit my enthusiasm is colored by an interest in the phenomena and as a result my positive view should be weighted less than unity.

The fourth element (ELVIS, a 400 kg enclosed vessel) appears to be similar to the effort of the British but with much larger mass. Here I think a carefully thought out experimental plan might be fruitfull. I don't know how they arrived at the experimental plan described. Someone must spend the time to address seriously the scaling issue. For example, why can't I put a mock-up of the lower vessel core support plate, flow baffle plate and forging above the water (downcomers around the periphery)? If I could, then meaningful answers might result (particularly if Briggs (GB) did the same at his smaller scale). Although on the surface this experiment looks productive, I find the preliminaries weak or non-existant (at least to me).

Coolant interaction model development plans are the fifth element of the five-step series. I find model development at this stage a waste of time. The fundamental physical processes that determine the observed phenomena have not been addressed in suffieicnt detail to allow meaningful modeling to be accomplished. This may be an overstatement and some of us may be smart enough -- but I doubt it. We have seen model development outpace understanding in many model development efforts where we had much more data and there are still problems. The SIMMER code is the most obvious example. Certain aspects of TRAC adn RELAPS are others. Without understanding of many of the phenomena I have hinted at in the above paragraphs, the modeling will result in codes with limited usefulness. They will only have acceptable uncertainty for circumstances they were tuned for. In essence, they become expensive, somewhat useless correlations. To clarify my point, let's look at item two -- triggering and film collapse. Film collapse can be the result of a simple heat balance; less heat supplied than taken away. Even this simple process requires that one know the effective thermal diffusivity of both the melt and the coolant. The effective thermal diffusivity will depend on the previous history of both. If we go beyond the simple case we must consider Taylor instabilities resulting from a three fluid configuration: melt, vapor, coolant. This problem has not been solved except for very small melt drops where it can be treated as a sphere. Further, one must include <u>all</u> initial motions as they will all lead to Taylor waves that grow.

In summary, I would like to paraphrase Bill Kerr's comments at the UCLA PAHR meeting. You have to start with a question, then lay out a program to answer it. The question is certainly asked but it is not complete. We need to focus more clearly on how well we need to answer it then decide how this will be accomplished. It is not clear that this has been done in a thorough enough manner, which leads me to a final comment.

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The NRC research program in this area suffers from the same malady as it does in many others. The program is weak in its support of fundamental research that will lead to sufficient understanding to make certain judgments about safety. In my view, since reasonable judgments (my own) lead to low probability of a serious impact on risk, the pace of the research need not be hectic. I do believe, however, that a sensible plan should be developed and pursued. Its basis should be that outlined by Mayinger at SANDIA several years ago. One should first ask questions about the State-of-knowledge. These lead to relatively simple experiments. In this case they may be neither simple nor quick but they should be carried out. Then one attempts to model the process and finally designs an experiment to test the model. If it does not work out, mothball the experiment and go back to basics. The only other meaningful approach is to accept the reasoned judgment of those you perceive to be experts and quit.

Responses to Requests 1 and 2 on page 3 of the August 24 Ross letter

(D. H. Cho, Argonne National Laboratory)

- Response to Request 1

I am reluctant to give a probability number since I do not know how to derive one in a satisfactory manner. However, I do feel that the WASH-1400 estimates (10^{-2} for PWRs) are very conservative. If it is assumed that the explosion energy required to fail the containment is in excess of 2 GJ, the containment integrity will not be threatened unless a single, coherent steam explosion involving many tens of tons of molten core occurs. If such a steam explosion were to occur, many tens of tons of fuel melt would have to mix en masse with the water in the lower plenum of the reactor vessel. I think this is an extremely unlikely event. First, the core melting is a gradual process and the movement of melt would be incoherent. The melt would be expected to fall into the water in small amounts over long periods of time. Second, even if tens of tons of melt were to fall into the water over a very short period of time, say, one second, it seems unlikely that a single coherent explosion involving the entire melt would occur in the lower plenum. It is possible that multiple explosions will occur, consuming most of the melt, but the fuel mass involved in any one single explosion would be a small fraction of the total mass. These comments are generic and would be independent of particular reactor systems or accident situations.

- Response to Request 2

It appears that the results presented in the Sandia study are direct consequences of the assumed flat distributions. If one combines the assumed distributions for the various parameters (pour length and diameter, conversion ratio, etc.), one finds that the explosion energy ranges from zero to 0.14 GJ for the low thirds of the uncertainty ranges, from 0.14 to 2.22 GJ for the middle thirds, and from 2.22 to 5.64 GJ for the high thirds. The boundary between the middle and high thirds is 2.22 GJ, which happens to be about the same as the threshold explosion energy required to fail the vessel top head. Thus, the results do not seem surprising. One might say that the conclusions given in the report simply reflect the authors' personal belief that the overall uncertainty is very, very large, essentially ranging from zero to one.

The question then is, how reasonable are the assumed distributions? The authors seem to assume that the low, middle and high thirds of the uncertainty ranges are equally probable. I think this assumption is questionable. If the low thirds had been assumed to be more probable than the high thirds, different conclusions would have been obtained. The authors claim that this report offers several improvements over a previous analysis by Swenson and Corradini. (Monte Carlo Analysis of LWR Steam Explosions, NUREG/CR-2307, SAND 81-1092.) I do not quite understand this claim.

Comments on SNL's Proposed Research Program

(D. H. Cho, Argonne National Laboratory)

(1) My opinion regarding the proposed experiments is as follows.

- SEALS: This large-scale experiment is important in assessing the scale effect and should have the highest priority. I generally agree with the proposed three-phase approach, although I have some reservations about Phase 3 (see comments below).

- FITSX: I am not sure what more will be learned from continuing experiments in the FITS facility. It appears that contributions from FITSX would be minimal as far as assessing the alpha mode of containment failure is concerned.

- SHIP: This experiment will be useful in investigating the stability of water vapor film at very high pressures. Also, it may produce valuable information on rapid cooling and solidification of fuel melt during film boiling at high pressures. The experiment should be pursued provided experiments involving high pressures of the order of 100 atm can be carried out at modest cost, as suggested by Sandia personnel.

- ELVIS: It appears that this experiment is mainly concerned with ex-vessel phenomena. Assuming that the SEALS experiment will proceed as planned, I do not think ELVIS will add much to resolution of issues relating to the alpha mode of containment failure. Further, ELVIS is likely to involve very substantial developmental work and the cost might become prohibitive. In my opinion, ELVIS need not be considered at this time.

(2) Comments on SEALS

- The proposed experiment matrix calls for a fixed pour diameter of 0.85 m. I would like to see some tests using smaller diameters than this while keeping the melt mass constant (perhaps by a factor of five?). I feel that the dropping mode of melt is important and that a simple way of varying the dropping mode is to vary the pour diameter.

- In SEALS 3 it is proposed to use a hemispherical upper and lower head. I am not convinced that the use of a hemispherical shape is necessary. It might be more convenient to use a movable circular cylinder (piston) for the upper head in conjunction with an energy absorber such as crushable honeycomb. The piston could travel some distance prior to impact on the energy absorber. Perhaps this could be done as a simple extension of SEALS 2.

- It is important to define clearly the measurements of the explosion energetics or the conversion ratio. The measurement methods should be described in detail for each test. It might be useful to have a group of experts conduct a review of the measurement methods and post test analyses as well as their implications for the alpha-mode failure issue.

To: Steam Explosion Committee Members

From: Michael Corradini
Nuclear Engineering Department
University of Wisconsin

COMMENTS ON PROBABILITY OF DIRECT CONTAINMENT FAILURE DUE TO STEAM EXPLOSIONS

Enclosed you will find my comments on the first two requests made by Dr. D. Ross of NRC research. I have attempted to be as quantitative as I possibly can for this complicated issue. My past work in this specific subject area has been issued as past Sandia reports (SAND79-2002, Feb. 1980; SAND80-2132, June 1981; SAND81-1092, Oct. 1981). Since that time my overall conclusions have not changed significantly; i.e. the probability of direct containment failure due to a steam explosion ('alpha-mode' failure) is quite small and even conservative upper bound estimates on this probability is still low, both being one to two orders of magnitude lower than what was estimated in WASH-1400. The quantitative numbers assigned to these qualitative statements were in the range of 0.0001 to 0.01 (point estimate to conservative bound). However, since that time the fundamental reasons for these conclusions have been reviewed to gain further insight into the phenomena and aid in understanding for various geometries and conditions (the original conclusions were based on the Zion PWR design). Based on my work and the original work of others like Fauske and Theofanous the reasons in support of these conclusions have come into sharper focus. It is those issues that I emphasize in these comments. I assume the other committee members are intimately familiar with the past analyses and results.

Question 1) What, in your judgement, is the best estimate value for the probability of containment failure arising from a steam explosion, given a core melt, that we should be using in the severe accident program. As part of your response please provide the bases (experimental and/or analytical) and the approach used in reaching your conclusions.

The estimation of a probability for this very rare event is not a straightforward engineering or reliability calculation. It must involve the use of 'engineering judgement' as well as theoretical analyses and experimental data. Because it involves a judgement based on past experience, simple analysis, and intuition coupled with experimental analyses and data interpretation one must recognize that the probability is simply a quantitative way of giving an qualitative judgement. For example if the probability I suggest is <<1 then qualitatively I am saying that the probability is 0 but my uncertainty does not allow me to unequivocally say it is 0. Therefore, the probability is a way quantifying the upper bound uncertainty. I am quite aware that this interpretation of the probability is different than what PRA analysts use, however, I feel it is quite valid for rare events where physical processes are not completely understood; if the processes were completely understood I would think that the probability would be a 'delta function'; i.e. a probability very near zero or one.

The information needed to evaluate this possible process could be divided into three areas: 1) initial conditions for the severe accident such as the amount of the core which is molten, the accident sequence that caused this current event, pressures and temperature etc; 2) boundary conditions that describe the physical geometry of the system such as the lower plenum structure, the free volume of the lower plenum and downcomer; 3) models and analyses that have been used in understanding experimental data and then used in predicting what may occur at full scale. The major difficulty in attack this question is that the available information is incomplete, and this requires one to use some engineering judgement to approximate initial conditions or physical phenomena with simple models (e.g. fuel-coolant mixing) for a variety of possible conditions.

The method to be used will be based on the assumption that if a steam explosion is to directly fail the containment a sequence of events must occur. This approach is similar to what is used in the WASH-1400 study and what I had used in SAND80-2132. The likelihood of each individual process in the sequence will be considered separately and subjectively assigned a probability to quantify its relative likelihood. To estimate this probability one must focus on a specific reactor geometry and set of sequences. For the sake of this discussion I consider the Zion PWR plant and a low pressure severe accident sequence (AB) and a high pressure sequence (TMLB) . Other PWRs and BWRs have different geometries and the conclusions may differ. However, for in-vessel steam explosions I think this discussion is generally valid for most PWRs and is probably conservative for BWRs because of the additional substantial structure in the lower plenum of the BWR. The major mechanism that is considered which might cause a direct failure of the containment is the generation of a large mass missile (composed of some part of the reactor head) due the impact of a liquid slug at or near the reactor head stud bolts caused by a large steam explosion in the lower plenum of the vessel. Based on our work in SAND80-2132 I consider that the threshold kinetic energy of a liquid slug necessary to cause a large mass missile and direct containment failure to be in the range of 1-2 GJ. This value is somewhat dependent on the type of the slug and the vessel design, and the range in this value indicates this variability. I do not consider other mechanisms which have been postulated to directly fail the containment from an in-vessel explosion because they have been analyzed and deemed incredible (see SAND80-2132).

The sequence of events necessary to cause this direct containment failure are:

1) A large molten 'corium' pool is formed (mass greater than 10% of core);

2) The molten corium pours into the lower plenum when water is present;

3) A trigger is available to initiate the explosion;

4) A steam explosion occurs with sufficient fuel-coolant mixing and conversion ratio to produce an energy of 1-2 GJ;

5) A liquid slug (corium and/or water) is formed which delivers the energy to the reactor vessel head during impact.

Let us consider each event separately and assess the relative likelihood.

In the event of a prolonged loss of coolant the decay heat generated by fission product decay will eventually cause the fuel rods to degrade and melt. The likelihood of a large molten corium pool forming in the core region is not precisely known. As the fuel-clad system melt and oxidizes current analyses envision that the molten corium will flow to colder regions of the core and form blockages. These blockages will allow the corium melt to pool up above these regions and collect as the decay heat continues to heat up the surrounding fuel rods. Experiments conducted at KfK using electrically heated rods indicate this behavior qualitatively and lend some credence to this physical picture. Recently analyses by B.R. Segal of EPRI and T.G. Theofanous of Purdue seem to indicate that if the degraded core accident progresses to this stage a major fraction of the core will experience this overheating and melting phenomenon due to natural convection currents that distribute the decay heat and the exothermic oxidation energy throughout the core region (as an aside their analyses also suggests that the time to core melt situation could be substantially delayed due to these same natural convection currents which would tend to spread out the energy deposition more evenly throughout the core region). Given these analyses and scoping experimental results one concludes that if the accident progresses to core melt then the melting phenomenon will spread and involve a large fraction of the core as a pool of molten corium is formed. Whether the molten corium pool formed is a substantial fraction of the core mass (>10%) is not well known because one does not know how large a pool could be formed before its very weight would cause some structural collapse. Given current analyses and small scale real material data I would estimate he likelihood for t i.e. ahis event, given a core melt has begun , to be quite high; probability near one.

The likelihood of water being present in the lower plenum of the reactor vessel while this core melting process is underway has been investigated by a number of researchers (e.g. one can read the Sandia Report by L.D. Buxton entitled Molten Core-Coolant Contact Analysis). In all analyses I have seen the calculations have indicated that the lower plenum can remain essentially full of water as the core meltdown proceeds if one assumes that blockages form and a coherent pool of corium forms. The reason for such a result is that the decay heat generated and the exothermic oxidation energy produced goes into heating surrounding fuel rods and upper structure while the solid fuel-clad blockages effectively thermally insulate the lower plenum region of the vessel. This result is somewhat dependent on the upper and sideward temperature boundary conditions, but the prime reason for the result is the concept of corium pool being formed. Given past analyses and the current physical picture I would think that the likelihood of a substantial mass of water being present (20-30 mtons of water) is quite high; probability near one.

The availability of a trigger to initiate an explosion may be one of the truly stochastic events for this physical process. The Sandia steam explosion experiments show that the explosion is triggered in a variety of circumstances. Spontaneously triggered steam explosions have been repeatably observed for oxide melts, metallic melts, iron-alumina melts, corium melts, in cold water ($T_{sat}-T_{sub}$ = 70-80K), in saturated water, for small masses of fuel (1-5kg) and coolant (20-40kg) and for large masses of fuel (20-25kg) and coolant (100-225kg). The explosions have been triggered at the surface of the water pool, within the bulk of the pool, and when the melt has reached the bottom of the chamber. In early experiments by L.D. Buxton and W.B. Benedick (Sandia Report on Efficiency Scaling Studies) in an open geometry chamber, some crude structure was put in the water chamber to see if the triggering behavior was altered; it did not appear to be. When the ambient pressure is increased it has been noted that the explosion first becomes easier to trigger (1-5 bar) and then much more difficult to trigger. In small scale experiments by Nelson an artificial trigger is used to initiate the explosion for timing purposes; these experiments reproducibly indicate that small scale interactions require a trigger of three times higher pressure than the ambient pressure. In the large scale FITS experiments the same behavior has been observed for spontaneous explosions. At 1 bar pressure spontaneous explosions always occur while at pressures of 5-10 bars no spontaneous explosions were observed. However, when an external trigger was applied the explosion is easily induced. This behavior and subsequent analysis indicates that the triggering of the explosion becomes harder as the ambient pressure increases. Therefore for the core melt accident where different accident sequences occur at different system pressures the effect of pressure may be important. The likelihood of the trigger given the existence of molten corium and water in the lower plenum is judged to be high at low pressures (1 bar) and lower at high pressures (100-200 bars); probability of one to one-tenth. It should be emphasized that the lower probability is a educated guess as to the upper bound value.

I will skip the topic of mixing and conversion ratio for the moment because it is the most important process and the least understood.

If an explosion does occur damage will result only if the energy transferred from the corium to the coolant can be transferred to the structure by the impact of a slug of material (fuel and/or coolant). In past analyses a number of simplified expansions were considered where the coolant or the fuel was considered as the slug (see detailed analysis in SAND80-2132). Either of these materials could become the slug depending on the contact mode and the time of triggering. For example if the explosion occurs during the pouring of the corium into the lower plenum then two types of slugs would be formed; a water slug up the downcomer and a corium fuel slug up through the core region. Both would impact the upper head assembly and stress the stud bolts. If the explosion occurs after the pouring of the corium is completed then the slug would be composed of primarily the coolant and the fuel fragments. In all cases the slug would begin to breakup due to Taylor instabilities. This breakup would cause the loading of the upper head to become more spread out in time but would not change the total impulse delivered. Therefore, I would estimate the likelihood of a slug impacting the upper reactor head to be quite high; probability near one.

The amount of fuel-coolant mixing and the subsequent explosion conversion ratio are the most important aspects of this process. There has been a large amount of discussion about these topics, and it may be useful to briefly discuss these concepts in general. To do this let me suggest that one consider two example cases for illustrative purposes:

CASE 1 : A steam explosion with no 'fuel-coolant mixing'
CASE 2 : A steam explosion with 'fuel-coolant mixing'

In Case 1 the fuel pours into the coolant pool and does not distort or break apart into discrete masses of any sort but simply pours into the pool as a coherent stream and collects on the lower plenum as a separate stratified pool. An important question to ask is could a steam explosion occur? The answer is yes, and situations of a pool of fuel surmounted by a water layer have been observed to explode in the Sandia tests. The key point to make is that such a situation would probably yield an explosion of lower conversion ratio (defined as the mechanical energy output divided by the available fuel thermal energy input). Why? The answer lies in the fact that the interfacial surface area between the molten fuel and the coolant liquid is minimized when there is no 'mixing' of the fuel and coolant. This surface area is the location where the vapor film collapses and the event is triggered. It is also the location where the fuel rapidly fragments and causes the vapor production to become explosive. As the explosion proceeds the fuel at this interfacial surface is fragmented and quenched transforming its thermal energy into mechanical energy of the system. The depth of fuel which rapidly fragments and quenches at this interface is not known although a number or investigators (i.e. myself, Theofanous and Fauske) have guessed that it may lie in the range of 1-10 cm. Therefore, in the complete absence of 'mixing' the volume of fuel which can participate in the explosion is given by the interfacial pool surface area between the fuel and coolant liquid times the depth of fuel which can be rapidly fragmented before the system expands and halts further rapid fuel fragmentation and quenching. As a point of reference if one were to take the cross-sectional area of the lower plenum of the Zion vessel this corresponds to an area of $15m^2$, and using a depth of 1cm corresponds to a mass of fuel participating of 1100 kg.

Now in Case 2 'mixing' simply causes the interfacial area between the fuel and coolant liquids to increase above the previous case of an undistorted or non- fragmented interface. Now the depth of fuel which rapidly fragments and quenches when this event is triggered may be altered because the geometry has changed but this is probably a second order effect in comparison to the increase in the available fuel surface area for fragmentation and quenching. Therefore, in the presence of fuel 'mixing' with the coolant the volume of fuel which can participate in the explosion is given by the interfacial pool surface area between the fuel and coolant liquid times the depth of fuel which can be rapidly fragmented before the system expands and halts further rapid fuel fragmentation and quenching. Theofanous considered this mixing phenomenon to be dominated by hydrodynamic instabilities which caused the fuel pour stream to break apart into individual discrete masses as it fell through the water pool. For small fuel pour streams the interfacial area was caused by this phenomenon, while for large pour streams he considered the water pool depth to be too shallow to allow for substantial mixing and increase in the available surface area above what would be caused by the original fuel pour stream and the stratified fuel pool. He concluded that the maximum fuel mass that could participate in an explosion would be 2-3% of the total core mass for the Zion PWR geometry (i.e. 2500 - 4000 kg). In his analysis he considered the depth of fuel that could be fragmented and quenched in the explosion (so-called <u>mixing length scale</u>) to be 10cm. Now this type of hydrodynamic mixing of fuel in a coolant could not go on indefinitely. Fauske pointed out that there was a limit to this type of mixing; i.e. if the interfacial surface area of the fuel and the coolant liquid became too large then the rate of heat transfer and steam production during the mixing would cause the coolant liquid within the pool to become fluidized and be carried out of the mixture. This concept is very important because it gives one an upper bound on the amount of surface area and mixing one could have. Fauske went further and using steady-state analyses and the concept of the critical heat flux estimated the maximum fuel mass to be 100 kg if the <u>mixing length scale</u> is assumed to be 1cm. In our previous work where we tried to analyze the FITS experiments we tried to estimate the amount of mixing in a full scale condition. We developed a simple model for transient breakup of the fuel pour stream as it fell through a water pool and looked for the limit of mixing based either on hydrodynamic or boiling limitations; the results for the Zion PWR geometry indicated that a maximum mass of 2900 - 5000 kg of fuel could participate in an explosion if the <u>mixing length scale</u> were in the range of 5-10cm. Recently, we have compared these results and tried to get a general upper bound for the fuel mass participation in a steam explosion based on the concept of coolant fluidization (see Figure 1). The results based on steady-state analyses suggest the range of fuel masses lie between 1000 and 10000 kg for <u>mixing length scales</u> of 1-10cm. I should point out that the actual depth of fuel fragmentation and quenching at the interface is still under investigation and I would consider these assumed values as upper bounds at this time.

The one point that has not been addressed for the actual reactor vessel is the effect of lower plenum structure. There is a lower core grid plate and baffle plate as well as a massive lower support forging. Structure can have two effects; first, the fuel coming in contact with structure may trigger a steam explosion, and second, the structure may aid in breaking up the fuel pour stream and mixing it with the coolant pool. The first effect would tend to reduce the amount of fuel mass that can participate in an explosion because the lower plenum volume is divided into three sub-volumes because of these structures (there are also incore instrumentation tubes which pass through all three volumes; see Figure 2). If explosion triggering occurs when the fuel contacts a solid surface one would expect multiple explosions within each sub-volume in the lower plenum. This behavior seems reasonable based on the past results of the FITS experiments, however, there have been no specific tests which demonstrate this behavior this type of structure. Past tests by Buxton and Benedick included some structure in the water chamber but there no apparent changes in integral behavior. The second effect of the structure would be to help breakup the fuel pour stream. This breakup would enhance the hydrodynamic effects in the absence of structure. However, even with this effect the upper bound amount of fuel which could mix is still limited by the effect of coolant fluidization in the lower plenum volume. This amount of fuel would be further reduced by triggering at structure surfaces. Therefore, the net effect of structure is probably to reduce the amount of fuel that can mix before the explosion is triggered.

The final issue we must consider is what is the explosion conversion ratio given a specified mass of fuel which may participate in the explosion. The conversion ratio is a function of a few more variables than just the fuel mass. Based on the analysis of the FITS experiments, the other important variables are the coolant mass (liquid and vapor) in the mixing zone, the final fuel fragmentation diameter, and the time for fuel fragmentation. The mass of coolant involved in the explosion is the next biggest determinant in the conversion ratio. If one considers the maximum amount of work that can be done by a steam explosion based on thermodynamics (i.e. an isentropic expansion after a constant volume thermal equilibration of the fuel and coolant masses, see Figure 3) one notices that the conversion ratio varies from a few percent to 30% depending on the coolant mass (liquid and vapor). The second point to note is that in the reactor vessel the expansion volume is a constant therefore the conversion ratio is reduced even further (almost a factor of two at high conversion ratios when the volumes of fuel and coolant are equal) because the pressure of the explosion would not decrease to ambient conditions at the time of slug impact . Therefore, without even considering the kinetics of the interaction the conversion ratio of the explosion is highly variable and below what some people quote as the thermodynamic maximum, 30%.

The amount of coolant involved in the explosion is also probably dependent on the mixing that occurs as the fuel pours into the pool. Based on analysis of the FITS tests and on simple analysis I think one could identify two bounds to the amount of coolant involved in the explosion.

First, if the mixing and interfacial surface area between the fuel and the coolant is small (i.e. given a fuel pour stream and pool with little distortion and breakup) then the only coolant that becomes involved is at this interface and it seems resonable that a near equal volume of liquid coolant would participate in the explosion. The rationale for this estimate is that the fuel and coolant would be essentially in a stratified separated state before the explosion and the coolant involved would be induced by the explosion, similar to the concept of a mixing length scale for the coolant. This estimate would imply that for small amounts of mixing (1000kg of fuel) the conversion ratio could be large 10-15%, and <u>the explosion energy delivered to the head would be less than 0.25 GJ which would pose no threat to the containment (in this calcualtion we have used a depth of penetration due to the steam explosion of 1 cm based on our theoretical calculations for vapor film collapse and jet penetraion)</u>.

Second, if the mixing and interfacial surface area between the fuel and the coolant is large then the coolant liquid and vapor would be intermixed with the fuel and would be the continuous phase. If the explosion is triggered in this geometry then more liquid coolant could become involved in the explosion as the coolant vapor expands. This would be especially true when the volume fraction of the liquid and vapor coolant is large compared to that of the fuel. This situation is the case in the lower plenum of the reactor where for fuel masses of 1000 - 10000 kg the volume fraction of fuel within the plenum is only 3-5%. An increase in the fuel volume fraction for these large masses of fuel is precluded due to the effect of coolant fluidization. The resulting conversion ratio for these situations is then much than 30% because the ratio of coolant to fuel mass participating in the explosion is on the order of 1-10 rather than 0.1; the conversion ratio would be 25-5%. <u>If one considers a fuel mass similar to what is calculated for the Zion lower plenum, neglecting the effect of internal structures, then a mass of 3000-5000 kg is predicted. The maximum explosion energy delivered to the head would be less than 0.5 GJ which again would pose no threat to the containment</u>.

Finally if one considers the effects of the kinetics on the explosion, the conversion ratio could be reduced even further because it takes a finite time to fragment the fuel and a finite time to transfer its thermal energy to the coolant. In the analysis of the FITS experiments we have noted that the fuel fragment size, post-explosion mass average, increases as the overall coolant to fuel mass ratio decreases. This increase in the fuel fragment size directly affects the conversion ratio decreasing it markedly, and is an indication of how the fuel-coolant mixing is retarded. Also as the amount of mixing is decreased the probable fragmentation time increases further reducing the conversion ratio. The difficulty with kinetics, i.e. the final fragmentation diameter and its characteristic time, is that it is very dependent on scale so that all one can do is qualitatively estimate the effect of scale based on FITS results. For the explosion it appears that mixing is the key variable which affects the fragmentation diameter and the time; as the mixing increases the fragmentation diameter and time decreases increasing the explosion conversion ratio toward the thermodynamic maximum. In our analysis of the FITS we have tried to scale the experimental results and our analysis indicates that if one successfully matches FITS explosion data and then keeps the dimensionless variables constant for the full scale behavior, the results indicate a conversion ratio a factor of two to four times smaller than at small scale conditions (see Figures 4-7). Therefore, qualitatively one would expect the conversion ratio to decrease below thermodynamic values by a factor of two to four due to the effects of kinetics. This would probably reduce the optimum explosion conversion ratio to a value of 7-8%.

Based on all the above discussion I would estimate that the likelihood of a steam explosion with fuel-coolant mixing and a conversion ratio that would produce an energy exceeding 1-2 GJ to be very small. I would subjectively assign a probability for this event of 0.01 to 0.0001. I actually do not think that the steam explosion would be large enough to cause direct containment failure, but I cannot prove it conclusively, therefore I conservatively assign a non-zero small number. This probability represents the total probability of 'alpha-mode' failure and indicates that the effect of fuel-coolant mixing and the subsequent explosion conversion dominates the results.

The only topic not specifically covered in this discussion is the difference between a PWR and a BWR in regard to steam explosions. The most pronounced difference is the presence of much more structure in the lower plenum. In fact the control rod guide tubes and structural support for the group of four fuel assemblies completely compartmentalize the lower plenum and seems to restrict almost all fuel-coolant mixing for 50-60% of the lower plenum volume. I would then estimate the likelihood for mixing with a mass sufficient for a large and prompt explosion to directly fail containment to be at least an order of magnitude less likely if not more.

Question 2) Provide written comments on the Sandia study described in NUREG/CR-3369 and any additional comments you feel are relevant to this study.

The Sandia report had two purposes, first to perform an uncertainty study to determine the range of probabilities for direct containment failure from steam explosions, and second to determine the most important parameters in the steam explosion process. In regard to the first purpose the study actually performed a sensitivity analysis. The initial conditions needed for the deterministic analysis were chosen based on a set of uniform frequency distributions which were sampled using the Monte Carlo sampling technique. These distributions were purposely picked not to be 'biased' by engineering judgement and reflected the widest possible range of values without violating obvious physical limits. These frequency distributions were then sampled in three distinct regions to determine the important input variables. Based on these calculations it was found that the frequency of direct containment failure ranged from 0 to 1. Also it was found that the amount of fuel mixed with the coolant and the conversion ratio were the two most important variables in determining the frequency of direct containment failure. The result of these calculations prompted the authors to report that the probability of direct containment failure due to steam explosions was 0 to 1 (with a 100% confidence level as I interpreted it) and this was the true measure of the uncertainty.

My comments about this report can be summarized in a few brief opinions:

1) First, I cannot accept this study as an uncertainty study, it is primarily a sensitivity study whose results are somewhat obvious. Because of this I do not think that the report necessarily adds very much to what was previously done using this methodology.

2) The report does correct some modelling deficiencies of the past report but these changes do not change the main points of what was observed in the first study. This methodology is primarily a simple deterministic calculation done a number of times with different initial conditions chosen from a range of initial conditions sampled by Monte Carlo techniques. The input frequency distribution functions were not improved in this study but made more general and less physical <u>purposely</u>. Therefore, I am not all surprised by what was found to be important and the wide variability of the results. Careful engineering judgement would have produced the same conclusions by experimental observations and simple calculations .

3) My biggest criticism is the interpretation of the sensitivity results that the range of uncertainty for 'alpha-mode failure is truly between 0 and 1. In a statistical sense this is a precise interpretation of the calculations, however in an engineering sense this is a somewhat useless conclusion. It

fails to focus on the important variables which the study specifically identified and come to some estimates on what are reasonable values for fuel and coolant mixing and the subsequent explosion conversion ratio. It would have been much more useful if the authors would have taken the results of their sensitivity analysis and then incorporated the mixing models of Fauske or Theofanous or others into the calculation to see how the results would differ from the wide variability that was built into the overall study. In its original inception I suggested that our transient mixing models of the FITS experiments be incorporated into the analysis in place of some of the ad-hoc assumptions used. This was not done, but I feel something more should have been done to lend some engineering balance to the work. In its present state the conclusion on the uncertainty range can only be considered as a opinion supported by parametric models which I feel does not validate the result. In fact I think this conclusion detracts from the credible job done on the sensitivity study.

To: Steam Explosion Committee Members

From: Michael Corradini
Nuclear Engineering Department
University of Wisconsin

COMMENTS ON PROPOSED LARGE SCALE STEAM EXPLOSION EXPERIMENTS

The enclosed letter is a response to the third question posed by Dr. D. Ross. I would recommend that some sort of large scale steam explosion experiments be conducted. This recommendation is based on the premise that the current FITS experimental apparatus is first used to delineate what are the proper large scale tests to perform that may prove the mixing and conversion ratio limits theoretically explained in my past letter. The intent of this recommendation is to regard the larger scale experiments as selected data points to demonstrate the effect of scale and not as a complete test series; the FITS experiments should provide the complete experimental data for purposes beyond alpha-mode failure. I also would strongly suggest that all interested members of the present committee be invited to do pre-calculations on these experiments to help focus on the important effects of scale.

Question 3: What are your comments on the Sandia proposed experimental program in particular:

a) Provide your opinion as to whether these experiments or other experiments and/or analyses will contribute to resolution of residual issues and to a further reduction in uncertainties;

b) Provide your recommendations on pre-experiment analyses or experimental procedures which will maximize the usefulness and effectiveness of the tests.

To determine if these proposed experiments (i.e. FITSX, SEALS and ELVIS experiments to address alpha-mode failure) would be useful one has to consider them in light of responses to the first question raised by Dr. Ross. My response to the first question was that based on fuel-coolant mixing theoretical arguments coupled with estimates of the explosion conversion ratio I would expect an in-vessel explosion to produce an energy of less than 0.5 GJ which is substantially below the threshold value for direct and simultaneous vessel and containment failure - 2GJ. Since these estimates were based on models and simple analysis to bound the behavior I arbitrarily assigned a non-zero probability for the point and bounding estimate, 0.0001 and 0.01 respectively. The only way one can consider larger scale steam explosion experiments justified is if (1) the upper bound probability values given are still so large that they cause the steam explosion alpha-mode failure probability to be a dominant contributor to the risk of severe accidents, or (2) one requires experimental verification at a scale larger than the current or planned FITS tests for the models and simple analyses that have been used to demonstrate the lack of a steam explosion threat. I do not know if the first requirement can be answered at this time because of the uncertainty of many other parts of severe accident phenomena. Therefore, if one assumes the answer to the first requirement is no then one must focus on the second requirement. Is it prudent to base a conclusion on the likelihood of alpha-mode failure on plausibility arguments using simple analyses with judiciously selected larger scale experiments used as points of verification? I think the answer to this question is yes, if certain requirements are met that help focus the experiments and the associated analysis, and therefore minimize their cost. The requirements could be summarized as:

a) Clearly identify the goal of these tests;

b) Consider the appropriate scaling for the test;

c) Perform tests at the FITS scale to get qualitative trends;

d) Verify that instrumentation can measure data within the desired accuracy;

e) Perform pre-test and post-test analyses to aid understanding.

If these requirements are met then the experiments would provide useful data in regard to steam explosion behavior for alpha-mode failure and would reduce the uncertainty substantially. Let me consider the Sandia proposed tests in light of these requirements and make specific suggestions for improvement during the discussion.

The Sandia proposal (memo dated 10-29-84) advances the goal of the SEALS tests to be the measurement of the explosion conversion ratio as a function of the fuel mass under scaled conditions (water depth, temperature and geometry with internal structures). The conversion ratio is defined as the ratio of the measured kinetic energy of the fuel and coolant masses to the initial thermal energy of the fuel delivered into the water. The Sandia memo argues that at large scale direct measurement of fuel-coolant mixing is impractical if not impossible and therefore one can indirectly determine its effect on the explosion by measurement of the conversion ratio. I agree with this approach because even in the FITS experiments that we have been trying to analyze one can only determine integral mixing behavior, and I cannot see how this would improve as the scale increases. The Sandia proposal implies that if the amount of fuel-coolant mixing is limited in these large scale tests then the conversion ratio will dramatically decrease and this would be a clear indication of a limit to mixing in the given scaled geometry. I expect this to occur based on some of the FITSB test data that we have looked at. I will address this in detail later. One final point is that I think one should try to measure other important quantities in these tests (e.g. mixture volume, displace water volume) but realize that visual measurements of mixing would only be qualitative and integral at best.

Let me cover the next two requirements together; i.e. scaling of the tests and performance of tests at FITS scales. The Sandia memo seems to put the SEALS experiments at a higher priority than experiments in FITS, designated FITSX. I would say that both experiments are of equal priority and one would want to first do scaled experiments in the FITSX scale so that qualitative if not quantitative results would be obtained as the melt delivery system is being developed for the SEALS experiments. In this way one could identify the important experiment or group of experiments to do at larger scale and reduce the need for unnecessary large scale experimentation.

Let us now consider the scaling of the SEALS experiments and the FITSX experiments assuming similiar tests would also be done at FITS scale first. The Sandia memo proposes to use the volume of the fuel and its energy as the fundamental scaling parameter with the fuel temperature and the ambient pressure held constant as a function of scale, using Iron-Alumina melt as a simulant to the Corium melt. The characteristic geometry of the in-vessel situation is given in Figure 1 with the properties of Corium and Iron-Alumina given in Table 1 - 2. If one compares the volume of the fuel (Corium or Iron-Alumina) or its energy, one finds that the decrease in density of Iron-Alumina is exactly balanced by the increase in its average specific heat (including the heat of fusion) causing a volume/energy scaling to be a good way to characterize the melt. Taking this result along with the rest of the assumptions of the Sandia memo one can construct a list of the initial conditions at full scale and at the SEALS and FITSX scales, Table 3. The only initial conditions which are not straight forward are the expansion volume and the internal structures. In the experiments one wants to be able to unambiguously measure the velocity of the expanding products after the explosion and to do this it appears the Sandia design has distorted the downcomer region to cause preferred expansion into this region. If this is done one should preserve the expansion volume to impact, i.e. the volume up to the nozzles and the stud bolts in the full scale system (57 m^3). This would reduce the required length of travel in the experiments. The inclusion of internal structures has never been done before in the FITS tests. I think structures should be included in the FITS and SEALS tests but cannot be scaled directly by the linear scale because that would cause too much prefragmentation of the melt compared to the full scale system. I would recommend that FITS tests first be done with the internal structures present at different sizes to assess its qualitative effect before anything elaborate is used in SEALS.

Now if one takes the resultant scaling from the Sandia proposal, and based on the previous analyses of Theofanous and Fauske and others one would expect the important variables to compare at these three scales to be:

1) The time available for fuel breakup as it falls through the water pool due to hydrodynamic instabilities, T^+, and the associated Weber number, We (see Table 4 and nomenclature);

2) The characteristic fluidization velocity for coolant intermixed in the fuel coolant mixture divided by the vapor velocity generated by the fuel-coolant heat transfer, v^*.

The first two dimensionless groups were identified by Theofanous and myself as being important in the breakup of the leading edge of the fuel pour stream as it enters the water. Theofanous also pointed out that for large pour streams (>10 cm) the remainder of the stream cannot break apart because the Rayliegh breakup time, t_b, is too long compared to the fall time through the lower plenum; in Table 4 the dimensionless ratio of the fall time to this time t_b is also included, designated T_b. The second dimensionless group is based on the concept first advanced by Fauske which compares the velocity needed for coolant fluidization to the superficial steam vapor velocity generated by the fuel-coolant mixture. Although there is some disagreement about the absolute number for this quantity I think there is agreement about the importance of this concept. This dimensionless group can then be used to compare the different scales of the experiments on a relative basis to the full scale situation.

Let us first consider the effect of hydrodynamic instabilities. Our theoretical work on droplet breakup indicates that the proper dimensionless group for modelling the breakup of droplets into smaller sizes is $T^{+} * We^{.25}$. This result is in agreement with the past liquid-liquid data of Theofanous and seems to agree with a wide range of other experimental data for gas-liquid systems. If one uses this dimensionless combination one finds that the scaled experiments are similar to the full scale situation; i.e. the dimensionless variables have similar quanitative values at different scales. Therefore the scaling of the experiment is reasonable for these hydrodynamic effects, assuming one is concerned with leading edge effects, and neglecting the effect of internal structures.

Next consider the effect of the mixing limit due to coolant fluidization. Using the dimensionless group described one finds that as the scale decreases the dimensionless velocity, v^{*}, increases linearly with the scale. This implies that as the scale decreases a larger fraction of the fuel can be mixed to a given <u>mixing length scale</u>; in this comparison we have used a length scale of 0.01m. The implication is that this upper bound mixing limit will become less restrictive at smaller scales and more fuel-coolant mixing would be allowed. Therefore the FITSX tests may result in more fuel fragmentation and larger conversion ratios than in SEALS or in the full scale. If this occurs then one would want to repeat a certain number of the experiments at larger scale to demonstrate that the FITSX results are 'conservative' in that they would allow more fuel-coolant mixing and produce larger explosion conversion ratios.

Let us consider the fourth requirement that the experimental instrumentation can measure the data to the required accuracy. What is the required accuracy for the measurement of the conversion ratio? Given the current method of measurement the uncertainty on the measured conversion ratio is $\pm$ 50-100%. Now if one is looking for an order of magnitude change in the explosion conversion ratio as a function of fuel mass, geometry and scale then this level of accuracy is sufficient. This level of accuracy is attained in the weak wall geometry of the current FITS geometry. I think that the conversion ratio measurement would become more accurate for rigid wall confinement; approximately 20-25% of the uncertainty in the current geometry is attributable to the uncertainty in the partition of mass ejected axially and radially. In a rigid wall vessel this would be eliminated completely, leaving the uncertainty in mass or velocity as the key variable. I would only recommend a limited number of future tests in the FITSX geometry using weak wall confinement. For the

SEALS geometry I would expect weak wall confinement is only warranted during the melt delivery development tests (SEALS I). For the rigid wall tests I think it is necessary to only measure the explosion conversion ratio to within $\pm$ 25-50%. The reason for this is that the stochastic nature of the triggering phenomena causes the explosion conversion ratio to vary at least this amount. In the design of the rigid wall SEALS II test chamber there does not appear to be a direct measurement of the upward kinetic energy after the explosion products leave the top of the chamber. One way to measure this energy directly would be to replicate what was used the open geometry tests by Buxton and Benedick; i.e. a cover over the top of the chamber. With a massive cover atop the SEALS II chamber one would purposely generate a large mass missile and this would give an unambiguous measurement of the upward kinetic energy and the explosion conversion ratio, see Figure 2.

The result of current FITS experiments seems to suggest that if the proper mass ratio of fuel to coolant is used the conversion ratio decreases dramatically. Consider the data from the FITSB experiments (see Table 5 and 6 and Figures 3 and 4). In FITS experiments 6B and 7B the initial coolant to fuel mass ratio was 1.5 - 3.0, in subccooled and saturated water; conditions very similar to what may occur in-vessel. The resultant explosions were weak with estimated conversion ratios in the range of 0.1 to 0.3%. These two experiments are the only FITS tests which explicitly address the proper geometry and scaling. I would expect the rigid wall experiments could produce more efficient explosions; however if the scaling analysis discussed earlier is valid one would also expect less fuel-coolant mixing as the scale increases and this would cause a decrease in the conversion ratio. Only experiments as a function of scale in a rigid wall chamber will settle this uncertainty.

I am not certain of the need of the SEALS III apparatus and the ELVIS apparatus. I assume SEALS III is to be used only if the conversion ratio does not decrease markedly as the scale increases; i.e. the conversion ratio needs to be measured more exactly $\pm$ 10%. The ELVIS tests do not seem useful because they do not seem to measure any variables different than SEALS III, and the mass is limited to only 400 kg, which seems to be too small in relation to the SEALS tests. I would not recommend the ELVIS tests unless they are more well defined.

Finally, I would suggest that the committee members and other interested parties participate in pre-test and post-test calculations of the upcoming FITSX and SEALS experiments. These analyses would help in the experiment design and in the subsequent interpretation of the results. Perhaps the organization of this committee could be used to facilitate this activity.

FIGURE 1
SCALED STEAM EXPLOSION
GEOMETRY

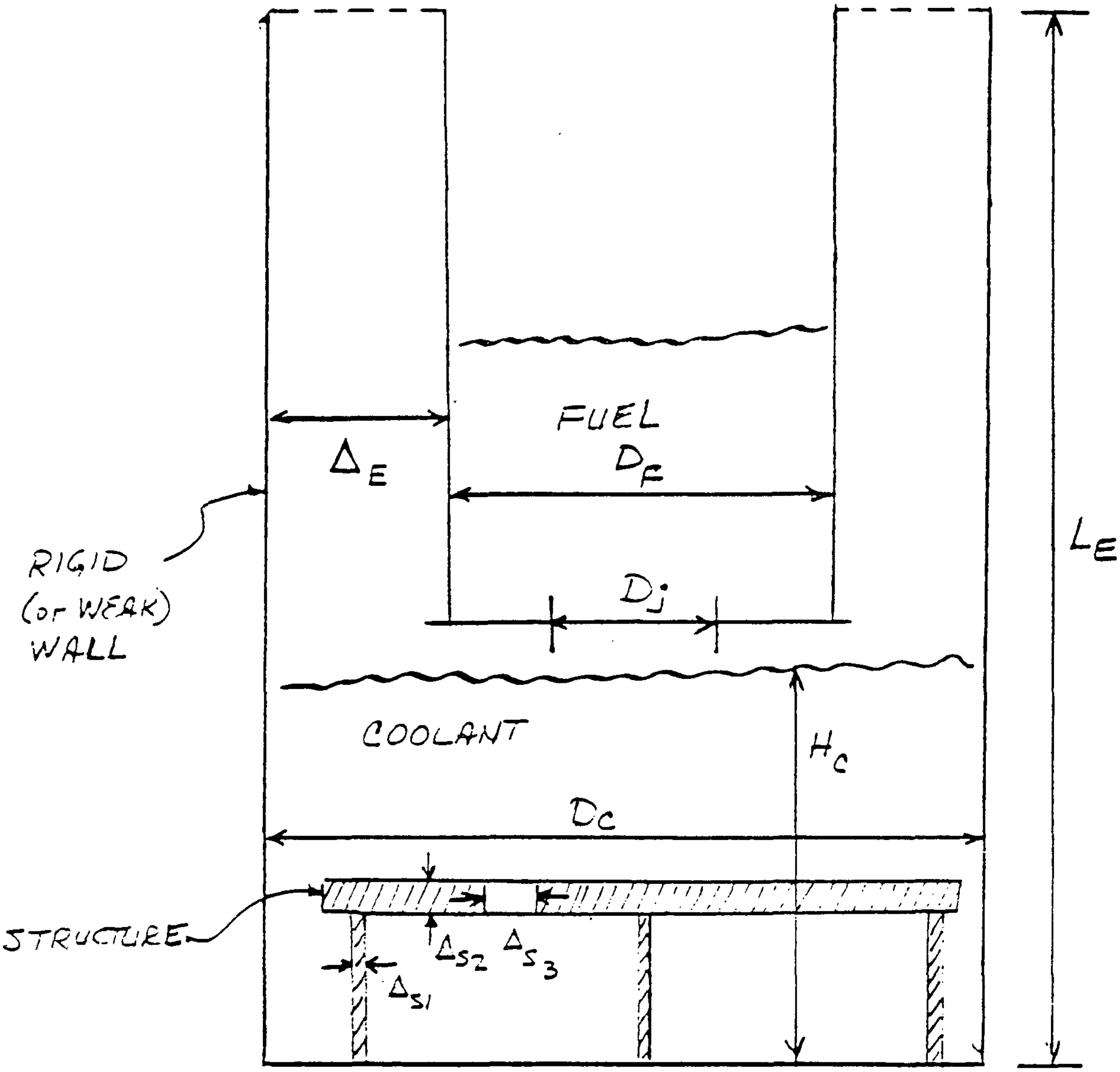


FIGURE 2

MODIFIED SEALS II GEOMETRY

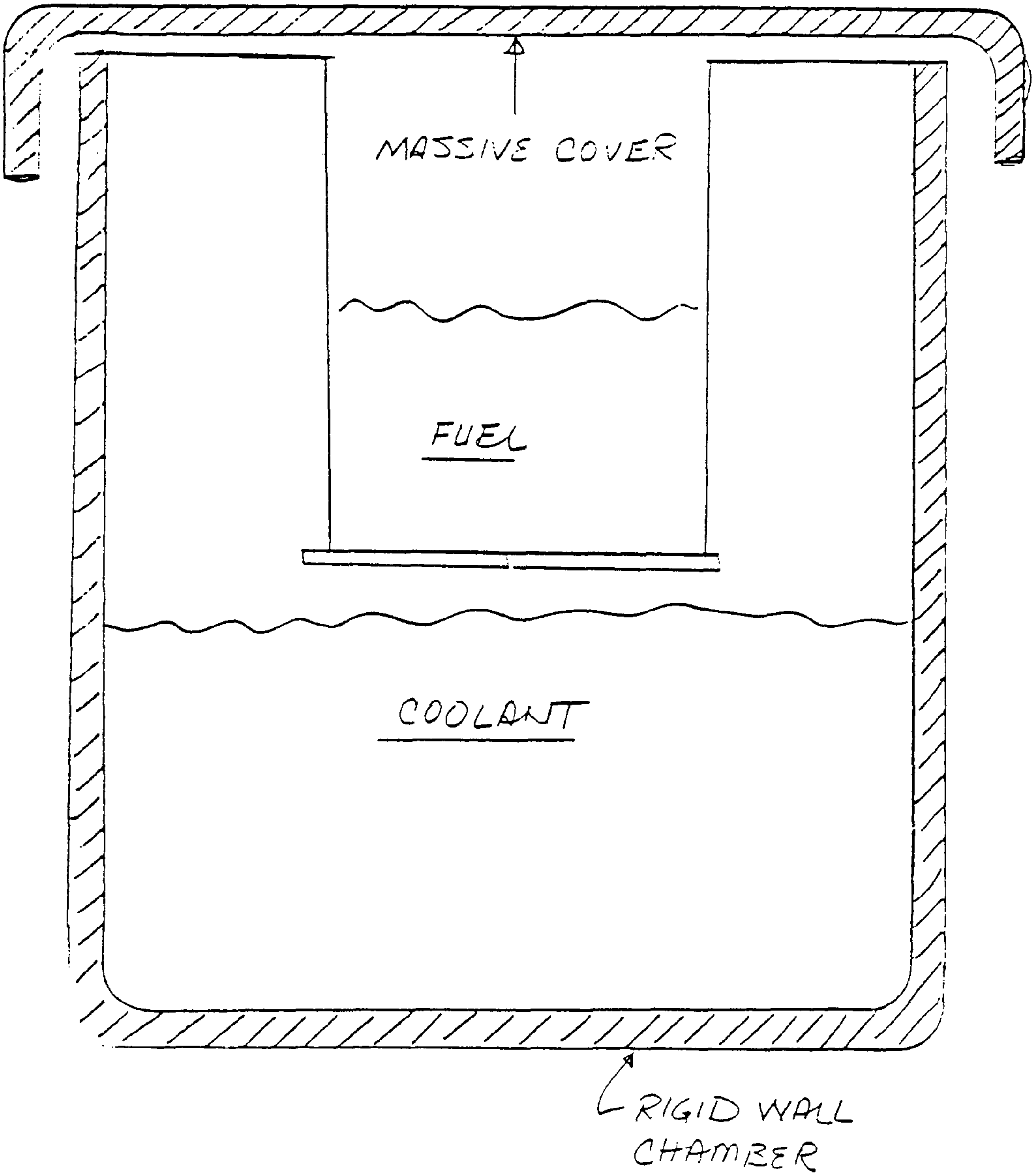


FIGURE 3

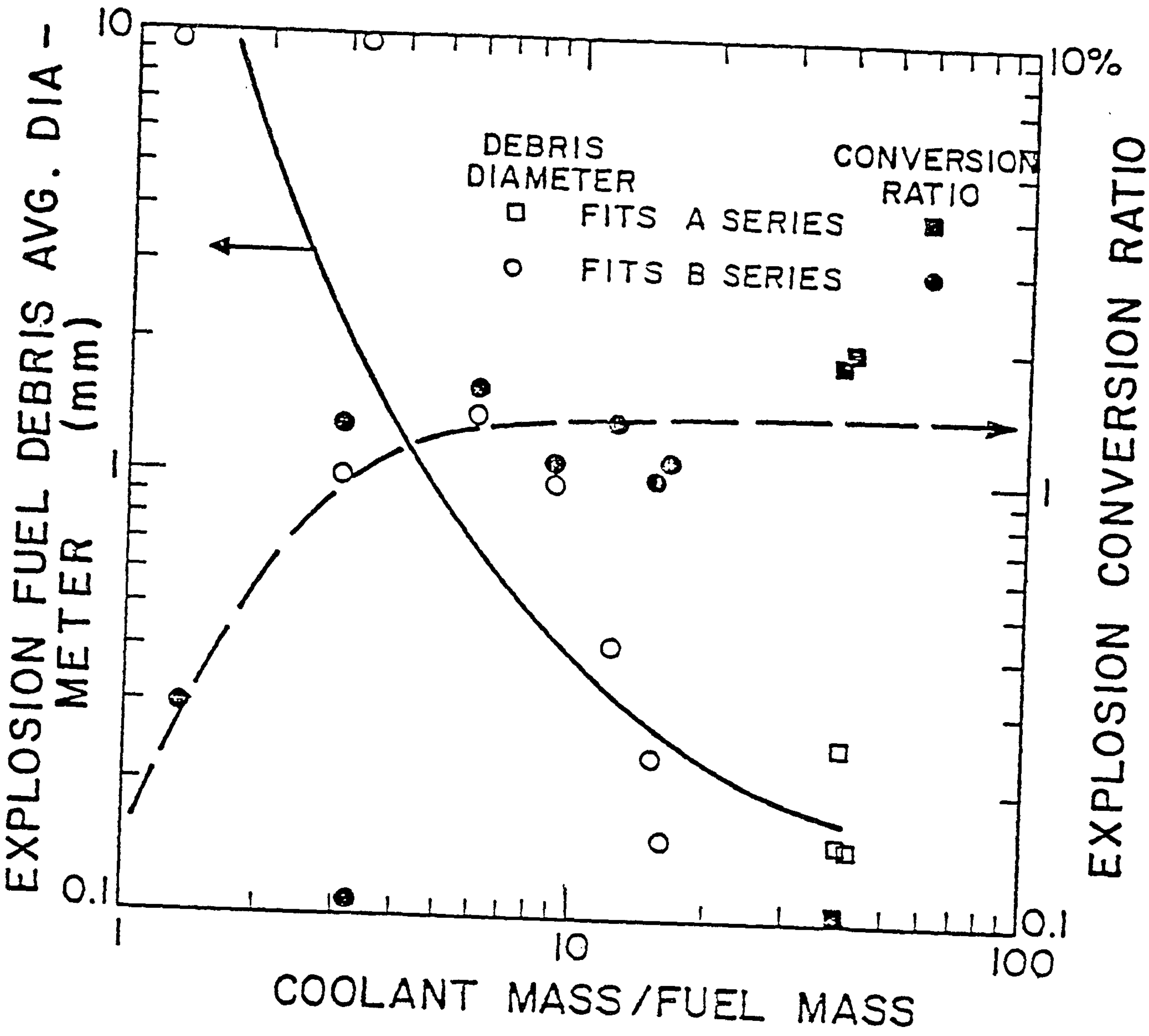
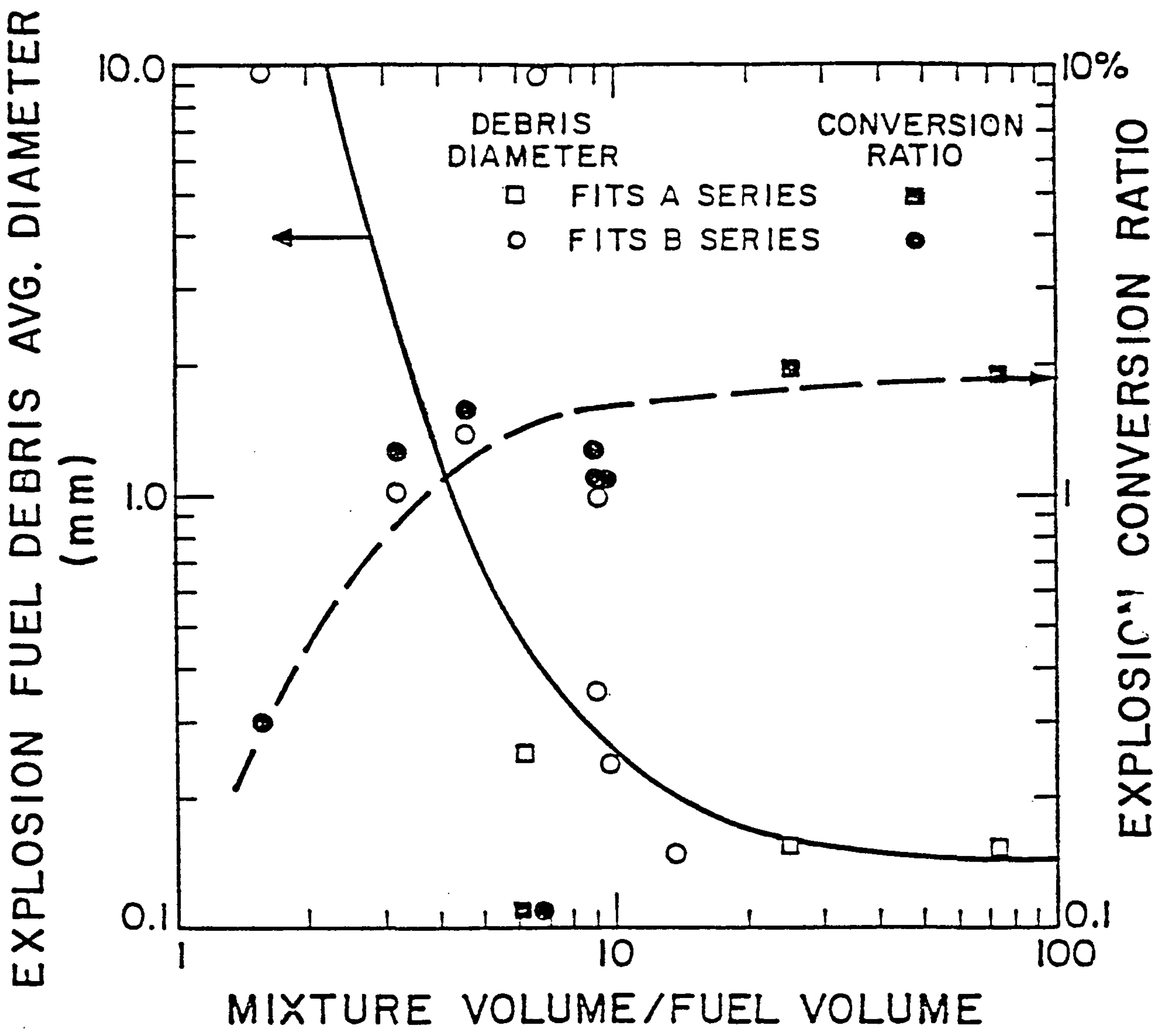

EXPLOSION FUEL DEBRIS AVG. DIA - METER (mm)
EXPLOSION CONVERSION RATIO
DEBRIS DIAMETER
CONVERSION RATIO
FITS A SERIES
FITS B SERIES
10
1
0.1
10%
1
0.1
1
10
100
COOLANT MASS/FUEL MASS

FIGURE 4


EXPLOSION FUEL DEBRIS AVG. DIAMETER (mm)
EXPLOSION CONVERSION RATIO
DEBRIS DIAMETER
CONVERSION RATIO
FITS A SERIES
FITS B SERIES
10.0
1.0
0.1
10%
1
0.1
1
10
100
MIXTURE VOLUME/FUEL VOLUME

NOMENCLATURE

A_{CHAM} CHAMBER CROSS-SEC. AREA

C_D - DRAG COEFFICIENT

c_f - FUEL SPECIFIC HEAT

D_j - FUEL POUR DIAMETER

g - GRAVITATIONAL ACCEL.

H_c - WATER DEPTH

i_{fg} - ENTHALPY OF VAPOR.

k_f - FUEL THERMAL COND.

m_f - FUEL MASS

P_∞ - AMBIENT PRESSURE

q''_{fDROP} - FUEL DROP HEAT FLUX $\left[\sigma_r(T_f^4 - T_{sat}^4) + h_{fb}(T_f - T_{sat})\right]$

t - TIME

τ - FALL TIME IN COOLANT

T^+ - DIMENSIONLESS TIME

T_b - τ / t_b WHERE t_b IS THE RAYLIEGH BREAKUP CHARACTERISTIC TIME

V_f - FUEL VELOCITY

V_{FI} - FLUIDIZATION VELOCITY

We_{crit} - CRITICAL WEBER NO. (12)

α_v - STEAM VOL. FRAC.

α_f - FUEL VOL. FRAC.

ρ_v - STEAM DENSITY

ρ_f - FUEL DENSITY

ρ_c - COOLANT DENSITY

$\Delta\rho$ - $\rho_c - \rho_v$

μ_f - FUEL VISCOSITY

σ_c - COOLANT SURF. TENSION

σ_f - FUEL SURF. TENSION

Dmix - MIXING LENGTH SCALE (assumed ~0.01 m)

TABLE I

CORIUM PROPERTIES

CONSTITUENTS	UO_2	ZrO_2	Zr	S.S.+	'FUEL'**
MASS* (m ton)	98	14.8*	11	6.4	~130
MELT. TEMP. (K)	3138	2963	2141	1700	1700-2700
WEIGHT FRAC. (w/o)	75.2	11.4	8.5	4.9	100
MOLAR FRAC. (a/o)	50.7	16.6	16.6	16.1	100
VOLUME FRAC. (v/o)	74.2	18.65	12.15	7.0	100
DENSITY (kg/m^3)	8700	~5250	~6000	6000	7660
SPECIFIC HEAT (J/kg K)	500	710	364	777	526
THER. COND. (W/mK)	3.66	2.3	31.5	22	8.5++
VISCOSITY (kg/ms)	0.0043	~0.004	~0.005	0.007	—
SURFACE TENS. (N/m)	0.5	0.5	~1.5	1.5	—
HEAT of FUSION (KJ/kg)	278	700	251	261	323

* ASSUMES 50% OXIDATION of ZIRCONIUM AS USED IN CLWG SP-1 (ZION)

\+ AUSTENITIC S.S. — Fe-85 w/o, Ni-5 w/o, Cr-10 w/o;

++ VOLUME AVERAGED

** ASSUMES A FUEL TEMPERATURE OF 2700 K

TABLE 2

IRON-ALUMINA PROPERTIES

CONSTITUENTS	IRON	ALUMINA	'FUEL' **
MASS * (m_f - kg)	1100	900	2000
MELT TEMP (°K)	1700	2300	1700 – 2300
WEIGHT FRAC. (w/o)	55	45	100
MOLAR FRAC. (a/o)	69	31	100
VOLUME FRAC. (v/o)	31	69	100
DENSITY (ρ_f - kg/m³)	6770	2500	3830
SPECIFIC HEAT (c_f - J/kgK)	800	1400	1060
THER. COND. (k_f - W/mK)	35	8	16.4
VISCOSITY (μ_f - kg/ms)	0.007	0.004	—
SURFACE TENS. (σ_f - nt/m)	1.8	0.5	—
HEAT of FUSION (KJ/kg)	270	1000	600

* BASED ON MASS PROPOSED FOR SEALS TESTS

** ASSUMES A FUEL TEMPERATURE OF 2700K

TABLE 3

SCALING of INITIAL CONDITIONS

VARIABLE	FULL SCALE	1/2 SCALE	1/8 SCALE
FUEL MASS (m_f - kg)	32000*	2000	32
FUEL TYPE	CORIUM	IRON-ALUMINA	IRON-ALUMINA
FUEL VOLUME ($\bar{V}$ - m^3)	4.2	0.52	0.0082
FUEL ENERGY ($\bar{E}_f$ - MJ)	51200**	6400**	102.4**
FUEL DIAMETER (D_j - m)	1.7	0.85	0.21
COOLANT DEPTH (H_c)	3.0	1.5	0.37
COOLANT DIAM. (D_c - m)	4.4	2.2	0.55
EXPANSION VOL. (m^3)	57+	7.2	0.26
$[\pi D_F \Delta_E L_E - V_C]$		DISTORTED	DISTORT.
Δ_E (m)	0.5	~0.5	~0.125
D_F (m)	3.4	~0.85	~0.3
V_C (m^3)	30	5.7	0.088
L_E (m)	8.5	~4.0	~1.0
INTERNAL STRUC.		DISTORTED	DISTORTED
RODS (ΔS_1 - m)	0.05		
P. THICK. (ΔS_2 - m)	0.21		
HOLES (ΔS_3 - m)	0.25		

* THIS CORRESPONDS TO 25% OF CORE MASS

** FUEL ENERGY CORRESPONDS TO A CORIUM HEAT CONTENT OF 1.6 MJ/kg and IRON-ALUM. of 3.2 MJ/kg

\+ CONSIDERS EXPANSION TO NOZZLES MINUS WATER VOLUME

TABLE 4

SCALING OF IMPORTANT PARAMETERS

VARIABLE	FULL SCALE	1/2 SCALE	1/8 SCALE
VELOCITY (V_f - m/s)	6	~6	~6
FALL TIME (τ - H_c/V_f s)	0.5	0.25	0.0625
FUEL DIAM.** (D_j-m)	0.1 – 1.7	0.05 – 0.85	0.0125 – 0.2
DIM. TIME (T^+)	11 – 0.64	15.5 – 0.9	15.5 – 0.9
DIM. BR. TIME (T_b)	1.25 – 0.018	0.88 – 0.013	0.44 – 0.006
WEBER NO. (WE)	3600 – 61200	1800 – 30600	450 – 7650
REYNOLDS NO. (RE)	10^5 – $2(10^6)$	$5(10^4)$ – 10^6	12500 – $2.5(10^5)$
FROUDE NO. (FR)	0.02 – 0.46	0.01 – 0.23	0.002 – 0.06
FLUID. VEL. (V_{Fl} - m/s)	4.0	4.0	4.0
DIM. VEL. (V^*)	0.01	0.02	0.08

** UPPER BOUND IS 1/2 CORE DIAM; LOWER BOUND IS 1/2 FUEL ASS. DIAM

$$T^+ \equiv \frac{V_f \tau}{D_j}\left(\frac{\rho_c}{\rho_f}\right)^{\frac{1}{2}} \qquad WE \equiv \frac{\rho_c V_f^2 D_j}{\sigma_f} \qquad V_{Fl} = \alpha_v \left[\frac{4}{3}\,\frac{We_{cr}}{C_D}\,\frac{g\sigma\Delta\rho}{\rho_v^2}\right]^{\frac{1}{4}}$$

$$T_b \equiv \left(\frac{H_c}{V_f}\right)\frac{8}{\pi}\left(\frac{\sigma_f}{\rho_c D_j^3}\right)^{\frac{1}{2}} \qquad RE \equiv \frac{\rho_c V_f D_j}{\mu_f}$$

$$FR \equiv \frac{g D_j}{V_f^2} \qquad V^* = \frac{\rho_v i_{fg} A_{CHAM} V_{Fl}}{\frac{6 m_f}{\rho_f D_{mix}}\, q''_{f\,DROP}}$$

TABLE 5

FITS INITIAL CONDITIONS

TEST NUMBER	1A	2A	3A	4A	5A	1B	2B	3B	4B	6B	7BR	8B	9B	1C	2C	3C	5C	1G	2G
FUEL*	$FeAl_2O_3$	$FeAl_2O_3$	$FeAl_2O_3$	$FeAl_2O_3$	$FeAl_2O_3$	$FeAl_2O_3$	$FeAl_2O_3$	$FeAl_2O_3$	$FeAl_2O_3$	$FeAl_2O_3$	$FeAl_2O_3$	$FeAl_2O_3$	$FeAl_2O_3$	$FeAl_2O_3$	Corium	Corium	$FeAl_2O_3$	$FeAl_2O_3$	$FeAl_2O_3$
Mass Inj (kg)	1.94	2.87	5.28	4.3	5.38	18.7	18.6	18.6	18.7	18.7	18.7	18.7	18.7	17	17.4	11.5	19.5	20.4	13.6
Mass Rec (kg)														15.9	15.68				
Fuel Temp (k)	~2700	2700	2700	2700	2700	2700	2700	2700	2700	2700	2700	2700	2700	2700	2700	2700	2700	2700	2700
OXIDATION																			
H_2 Mass (gm)	38.1	56.4	103.7	84.5	106	367	367	367	367	367	367	367	367	334	125/39	825/255	383	400	267
Energy (MJ)	0.61	0.9	1.66	1.35	1.7	5.88	5.88	5.88	5.88	5.88	5.88	5.88	5.88	5.35	4.36	2.85	6.13	6.41	4.27
Oxid Fuel Mass	2.40	3.55	6.53	5.32	6.6	23	23	23	23	23	23	23	23	21.03	19.4	12.8	24.12	25.2	16.8
COOLANT	H_2O	H_2O	H_2O	H_2O	H_2O	H_2O	H_2O	H_2O	H_2O	H_2O	H_2O	H_2O	H_2O	H_2O	H_2O	H_2O	H_2O	H_2O	H_2O
Mass (kg)	90	150	226	226	226	226	113	57	226	64	28	283	170	112	226	113	113	44.4	110
Temp (k)	283	286	297	298	297	298	298	295	299	367	290	288	289	299	296	300	428	268.5	367
Depth (m)	0.43	0.53	0.62	0.61	0.61	0.61	0.3	0.3	0.61	0.3	0.15	0.76	0.46	0.305	0.61	0.38	0.305	0.216	0.546
Width (m)	0.457sq	0.53sq	0.61sq	0.61sq	0.61sq	0.61sq	0.61sq	0.43sq	0.61sq	0.46sq	0.43sq	0.61sq	0.61sq	0.61sq	0.61sq	0.53sq	0.61sq	0.453sq	0.455sq
COVER GAS	Air	Air	Air	Air	Air	Air	Air	Air	Air	Air/St	Air	Air	Air	N_2	N_2	N_2	N_2	Air/St	Air/St
Temp (k) ♣	283	286	297	298	297	298	298	295	299	367	290	288	289	299	296	300	428	310.5	313
Press (MPa)	0.083	0.083	0.083	1.02	1.09	0.083	0.083	0.083	0.083	0.083	0.083	0.083	0.083	0.083	0.083	0.083	0.52	0.084	0.083
Mass (kg)	6.6	6.53	6.3	77	77	6.3	6.3	6.3	6.3	5.1/3.25	6.3	6.3	6.3	6.08	6.14	6.05	25.7	5.93	5.75
Humidity	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	75%	96%
GEOMETRY	Steel	Steel	Steel	Steel	Steel	Steel	Steel	Steel	Steel	Steel	Steel	Steel	Steel	Steel	Steel	Steel	Steel	Steel	Steel
Temp (k)	283	286	297	298	297	298	298	295	299	n.o.	290	288	289	299	296	300	n.o.	295	298
Volume (m^3)	6.5	6.5	6.5	6.5	6.5	6.5	6.5	6.5	6.5	6.5	6.5	6.5	6.5	6.5	6.5	6.5	6.5	6.5	6.5
Surface Area (m^2)	20	20	20	20	20	20	20	20	20	20	20	20	20	20	20	20	20	20	20

*Fuel Compos: Fe/Al_2O_3 55/45 w/o; $\rho = 3830\ kg/m^3$; $Cp = 1060\ J/kgK$; $k = 16\ w/mk$; $l_{fs} - 0.6\ MJ/kg$; $\sigma \sim 5\ mt/m$; $T_{ref} - 273\ k$

$UO_2/ZrO_2/SS$ 53/17/30; $Fe/Cr/Ni$ 67/21/12 w/o; $\rho - 7000\ kg/m^3$; $C_p - 590\ J/kgK$; $k - 10.4\ w/mk$; $l_{fs} - 274\ kJ/ks$; $\sigma \sim 0.5\ mt/m$; $T_{ref} \sim 273°k$

TIK X5

FIT: .TS

TEST NUMBER	1A	2A	3A	4A	5A	1B	2B	3B	4B	6B	7BR	8B	9B	1C	2C	3C	5C	1G	2G
FUEL	$FeAl_2O_3$	$FeAl_2O_3$	$FeAl_2O_3$	$FeAl_2O_3$	$FeAl_2O_3$	$FeAl_2O_3$	$FeAl_2O_3$	$FeAl_2O_3$	$FeAl_2O_3$	$FeAl_2O_3$	$FeAl_2O_3$	$FeAl_2O_3$	$FeAl_2O_3$	$FeAl_2O_3$	Corlum	Corlum	$FeAl_2O_3$	$FeAl_2O_3$	$FeAl_2O_3$
Ent Vel (m/s)	6.2	4.6	5.0	7.0	5.3	5.4	6.0	6.0	6.8	7.2	7.4	6.5	7.0	~5	~5	~5	~5	8.0	8.0
Ent Diam (m)	0.035	0.036	0.033	0.035	0.032	0.05	0.2	0.20	0.18	n.o.+	0.2	0.18	0.2	n.o.	n.o.	n.o.	n.o.	disp	disp
Spher Diam (m)	0.1	0.11	0.14	0.13	0.14	0.21	0.21	0.21	0.21	0.21	0.21	0.21	0.21	0.2	0.17	0.15	0.21	0.21	0.19
Dist Test Diam																			
Mass Avg (mm)	2	0.26	0.15	3.8	0.15	0.2	1.4	1.1	0.25	10.	7.	0.145	0.9	1.02	1.57	>10*c	>10c	>10c	~10c
Sauter (mm)	2.4	0.17	0.09	4.7	0.09	0.19	0.56	0.33	0.21	2.1	1.2	0.1	0.33	0.23	0.77	Lump	Lump	Lump	Lump
FCI EVENT	FCI	1 Expl	1 Expl	FCI	Trigg/Expl	2 Expl	1 Expl	1 Expl	2 Expl	FCI	1 Expl	2 Expl	1 Expl	1 Expl	2 Expl	FCI	FCI	FCI	FCI
Time of Event (s)	0.115	0.075	0.15	0.25	0.45	0.142	0.084	0.077	0.016	0.4to	0.08	0.027	0.099	0.113	0.03	0.12	0.1	0.1	0.1
or Base Contact						0.275			0.134	0.15		0.146			0.15				
Location	None	Surf	Below	None	Base	Surf	Surf	Base	Surf	None	unk	Surf	Base	Surf	Surf	None	None	None	None
			Surf			Disp			Base			Base			Base				
Hydrogen																			
Gener (gm)	n.o.	n.o.	n.o.	n.o.	n.o.	n.o.	n.o.	n.o.	n.o.	n.o.	n.o.	n.o.	n.o.	110	48	7.8	49	~15	~10
Ins Pk Wall T (K)	n.o.	n.o.	n.o.	n.o.	n.o.	n.o.	n.o.	n.o.	n.o.	n.o.	n.o.	n.o.	n.o.	n.o.				296.5**	301**
Pk Gas T (k)	n.o.	n.o.	n.o.	n.o.	n.o.	373	n.o.	398	378	393	393	373	378	n.o.				433-473	<523
Gas Phase						(1s)		(1s)	(1-2s)	(1s)	(1s)	(1s)	(0.6)						
Explsion																			
Pk Press (Mpa)	-	n.o.	0.37	-	1.17	0.2	0.32	0.54	0.12	-	0.11	0.11	0.31	0.52				0.35	0.33
						0.3			0.6	-		0.473							
Time to Pk (s)	-	n.o.	0.002	-	2	0.144	0.087	0.08	0.03	-	0.05	0.017	0.1	0.003				12	>3
						0.282			0.146	-		0.144							
Gas Phase																			
Steam Sake										**	**	**	**						
Pk Press (MPa)	-	n.o.	0.2	-	1.17	0.26	0.33	0.6	0.24	0.525	0.525	0.226	0.295	0.43				0.35	0.33
Time to Pk(s)	-	n.o.	0.015	-	2	3.0	0.9	1.0	4	1	1.2	3.95	0.6	0.03				12	>3
Water Phase						/		/											
Pk Base Press (MPa)	-	n.o.	7.5	-	12.5	3.3/28	6.7	3.4/136	14/6	1.8	1.2	10	4.2/6	12.0				-	-
Rise Time (ms)	-	n.o.	0.2	-	0.2	0.1	1.0	2/0.3	0.4/0.2	2	5	0.1	1.0	0.25				-	-
Pk Wall Press (MPa)	-	n.o.	4.3	-	2.5	2.6	n.o.	n.o.	n.o.	0.6	0.6	9.6	3.7	6.0				-	-
Rise Time (ms)	-	n.o.	0.2	-	0.2	0.1	n.o.	n.o.	n.o.	20	40	0.8	0.5	0.25				-	-

* Melt Agglomerated @ base and froze with some corase fragments

/ Two values indicate different pressures either in time or location

\+ not observed or measured in test

** hydrogen burn data is not included

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January 18, 1984

Mr. Cardis Allen
U.S. Nuclear Regulatory Commission
Mail Stop 5003
Washington, D.C. 20555

Dear Cardis:

Enclosed is my response to the questions raised in Dr. Ross' letter of August 24, 1984, regarding steam explosions and their implications to reactor risk. The enclosed is a revision of and supercedes the material provided to you on November 12, 1984.

My estimate of the probability of direct containment failure due to in-vessel steam explosions in large dry containment PWR's is 1E-4, with a range of 1E-6 to 1E-2. The corresponding probability for Mark I BWR containments is 1E-3, with a range of 1E-4 to 1E-2. The rationale behind the development of these probabilities is given in the enclosure.

While containment failure due to steam explosions does not appear to be a dominant contributor to reactor accident risk, this containment failure mode may be representative of the upper limit to reactor accident consequences. Further, even in the absence of direct containment failure, [due to steam explosions] there are numerous indirect effects of steam explosions that can significantly affect the course and consequences of postulated core meltdown accidents. These indirect effects may be more important than the alpha containment failure mode; thus, further understanding of steam explosion phenomenology and its role in severe accident progression is still required.

My review of NUREG/CR-3369, "An Uncertainty Study of PWR Steam Explosions," led me to conclude that the unbounded range of uncertainty for containment failure due to steam explosions inferred from this study is directly attributable to the use of rectangular probability distributions and is not particularly definitive for this reason. The methodology utilized is potentially quite powerful and deserves further application. Specific comments and recommendations are given in the enclosure.

With regard to the steam explosion experimental program, while there appears to be a definite need for larger scale experiments, the program proposed by Sandia appears to be premature. A more focused program based largely on the use of existing facilities is recommended. Continued development and application of models for describing steam explosion phenomenology are recommended. The availability of at least partially validated models should be a prerequisite to the conduct of large scale experiments

I trust that the enclosed will be responsive to your needs. If there are any questions or if I can be of further assistance, please feel free to contact me.

Sincerely,

Peter Cybulskis

PC:dem

Enclosure

cc: D.F. Ross, NRC
R. Meyer, NRC
Steam Explosion Review Group

ON THE POSSIBILITY OF DIRECT CONTAINMNENT FAILURE DUE TO IN-VESSEL STEAM EXPLOSIONS

Historical Perspective

The Reactor Safety Study (WASH-1400) represented the first attempt at quantifying the likelihood of containment failure due to steam explosions. The derivation of these probabilities was highly judgemental, though it was supported by considerable analyses conducted to provide insight on the phenomena that may govern the occurrence of such failures. It was fairly obvious that simple containment pressurization by the steam and hydrogen generated from the interaction of the core debris with water was insufficient to threaten the PWR containment. Further, the large dry PWR containment boundary is rather far removed and well shielded from the reactor vessel; thus it was not clear if even violent events in the latter could affect the integrity of the former. The generation of a large missile, namely the top head of the reactor vessel, which impacted on the containment boundary was postulated as the essential link between the events in the reactor vessel and the containment. The analyses that were conducted focused on the likelihood of generating such a missile and the further likelihood that this missile could impair containment integrity. For the large dry PWR containment design it was difficult to see how anything that happened in the reactor cavity could affect the containment boundary, so we essentially dismissed the possibility of containment failure due to steam explosions in the reactor cavity. Most of the quantitative analyses in support of the WASH-1400 steam explosion evaluation were based on the PWR design. When we considered the BWR, we did recognize that the larger amount of in-vessel structures in the BWR would provide greater potential for steam explosion energy dissipation; on the other hand, the closer proximity of the containment shell to the reactor vessel in the Mark I BWR design with less intervening structure led us to conclude that less energetic missiles could potentially threaten this containmnent than was the case for the large dry PWR design. Based on a number of considerations, both qualitative and quantitative, we came up with a probability of 0.01, given a core melt, with an uncertainty of a factor of ten in either direction, of an in-vessel steam explosion leading to direct containment failure for both of the two designs considered. There were some differences related to the likelihood of having subcooled water in certain of the BWR sequences (at that time steam explosions in saturated water were believed to be less likely). Also, the probability of failure due to explosions in the reactor cavity was considered for the BWR design.

The probability of direct containment failure due to an in-vessel steam explosion, the alpha failure mode as defined in WASH-1400, was the product of three factors. The first was the probability that a significant fraction of the molten core came coherently into contact with water; this was assessed as being essentially unity. The second factor was the probability of fragmentation into small particles as the molten core material came into contact with water; this factor was assessed to be 0.1. The third factor was the fraction of interactions that would be expected (sufficiently energetic) to result in containment failure; this fraction was estimated to be 0.1. A major reason for the relatively efficient coupling between the explosion and the kinetic energy of the upper head missile was the confinement provided by the reactor vessel, as inferred from the one-dimensional analyses conducted for WASH-1400.

An important point that may not be generally appreciated is that aven at a probability of 0.01, given a core melt, containment failure due to in-vessel steam explosions did not appreciably contribute to reactor accident risk as assessed in WASH-1400. In the WASH-1400 analyses, sequences involving steam explosion containmnet failure in the absence of containment sprays were assigned to PWR Release Category 1, and steam explosion containment failures with the sorays operating were assigned to Release Category 3. In adding the probabilities of the individual sequences in each release category to arrive at the total probability for each category, WASH-1400 used a smoothing algorithm in an attempt to account for the uncertainties in release category assignments. This smoothing algorith effectively spilled over some of the total probability from each release category to adjoining categories. Examination of the makeup of the total probability of PWR Release Category 1 reveals that it is dominated by the spillover from release category 2, rather than by the sum of the sequences actually calculated to belong to this category, i. e., those involving containment failure due to in-vessel steam explosions.

Figure 1 presents the WASH-1400 median results, including the effect of the smoothing algorithm, in terms of probability per reactor year versus PWR Release Category. It will be recalled that the consequences in terms of fission product releases to the environment decrease with increasing categories; thus Release Category 1 represents the steam explosion containment failures, Categories 2 and 3 represent primarily containment overpressure failures, Categories 4 and 5 are comprised of containment isolation failures, and Categories 6 and 7 represent the containment melthrough cases. Translating these results into measures of risk as determined in WASH-1400 reveals that of the total risk of early fatalities, 35 percent stems from Release Category 1, 41 percent comes from Release Category 2, and 24 percent is due to Release Category 3. Measured in terms of latent fatalities, about 9 percent of the total is due to Category 1, 46 percent due to Category 2, 39 percent from Category 3, and lesser contributions from the other Release Categories. It should be noted that the foregoing results include the results of the smoothing algorithm.

Figure 2 illustrates the WASH-1400 results in terms of probability per reactor year versus PWR Release Category, with and without the smoothing algorithm. Without smoothing the total probability of Release Category 1 is about two orders of magnitude lower than Release Categpry 2. Figure 3 presents the results, with and without the smoothing algorithm, in terms of the contribution to the risk of early fatalities for the several release categories. Figure 4 presents the corresponding results for latent fatalities. It is clear that the smoothing algorithm tended to overemphasize the significandce of Release Category 1. A similar situation is also found for PWR release Categories 4 and 5, where the overall probability is dominated by the spillover from other categories rather than by the sum of the sequences actually assigned to them; the latter categories, however, were not risk significant, thus the impact is minimal in any case.

The above discussion illustrates that the smoothing algorithm used in WASH-1400 gave a distorted picture of the risk and of the risk significance of the steam explosion containment failure mode. If the smoothing is removed from the WASH-1400 results, then the overall risk is overwhelmingly dominated by PWR Release Categories 2 and 3, with little impact from Release Category 1. Release Categories 2 and 3 in WASH-1400 were dominated by the containment bypass sequence and containment overpressure failures.

As part of the Reactor Safety Study Methodology Applications Program (RSSMAP), we performed a series of calculations rebaselining the WASH-1400 results using the analytical tools and insights available at that time. In this rebaselining all sequences involving containment failure due to in-vessel steam explosions were assigned to PWR Release Category 1, the probability of containment failure due to steam explosions in low pressure core melt sequences was taken to be the same as in WASH-1400, or 0.01, but for high pressure core melt sequences this probability was reduced to 0.0001. These changes did not alter the perspective of the relative unimportance to reactor risk of the alpha mode of containment failure.

Thus if the WASH-1400 perspectives were believed to be consistent with current knowledge, it could be concluded that unless the probability of containment failure due to steam explosions were substantially higher than 0.01 per core melt, this failure mode was unimportant and could be dismissed from further consideration. I would speculate that most knowledgeable analysts would agree that the probability of direct containment failure due to in-vessel steam explosions given a core melt is less than on chance in a hundred. As is discussed below, the WASH-1400 perspectives on the risk of reactor accidents have changed substantially and are continuing to change; thus the question of the possible significance of steam explosions must be viewed in a different light. Such a view is given below.

Current Perception

In what follows I have attempted to assess the possible significance of the steam explosion containment failure mode in light of today's perspectives on accident fission product source terms and containment failure mode probabilities. These questions will, of course, be addressed in considerably more detail by the Accident Source Term Program Office in NUREG-0956, but pending the availability of that document I felt that even a limited perspective could be useful. My principal sources of information were the ASEP and BMI-2104 results, as well as the work of the Containment Loads and Containment Performance Working Groups (CLWG and CPWG); these were combined with a lot of judgement and speculation on my part.

The ASEP results for Surry, the design considered in WASH-1400, show a significant reduction in the likelihoods of the V and S2C sequences, two of the dominant accident sequences as assessed in WASH-1400. Changes in the probabilities of other important sequences as determined by ASEP, however, lead to a somewhat greater (about a factor of two) total core melt probability than in WASH-1400.

The BMI-2104 results for Surry indicate generally lower fission product releases to the environment, in some cases substantially lower, than WASH-1400. BMI-2104 is not a comprehensive reassessment of risk for the plant designs being considered, focusing instead on the examination of selected sequences and containment failure modes for a variety of designs. For example, the BMI-2104 analyses do not address the likelihood or the consequences of steam explosion events. Whether the BMI-2104 results should be taken at face value or not is not clear for a variety of reasons. The environmental source terms in BMI-2104 are, in a number of cases, based on substantial retention of volatile fission products in the reactor primary system; the ultimate fate of these fission products is not clear. It is possible that some of these species will reevolve from the primary system and be available for release to the environment later in time. The BMI-2104 analyses did not address possible direct interactions between the core debris and the containment atmosphere and the possible implications on source terms and containment failure modes of such interactions. Factors such as the above as well as others notwithstanding, it seems clear that current estimates of accident source terms are significantly lower than those developed for WASH-1400. Thus I have assumed for the moment that the BMI-2104 source terms will not be dramatically altered by subsequent developments. Again it should be noted that NUREG-0956 should provide clarification to such issues.

The BMI-2104 analyses did not attempt to quantify the probabilities of the various possible containment failure modes for each of the sequences considered. The occurrence of specific containmnent failures was assumed for purposes of the source term evaluations where such failure modes were deemed to be plausible. The CLWG and CPWG will provide the necessary background to quantify the likelihoods of the various containment failure modes appropriate to each sequence. Thus I have again attempted to speculate on the likely outcomes of the above working groups and their implications on the BMI-2104 results.

For the important Surry core melt sequences as identified by ASEP, I have estimated the magnitude of source terms for each of the plausible containment failure modes and assigned each to an appropriate WASH-1400 release category. I have also estimated the likelihood of the occurrence of each of the containment failure modes, except for steam explosions; the latter will be discused subsequently. The products of the accident sequence and containment failure mode probabilities were then summed for each of the release categories, akin to what was done in WASH-1400. The results of this evaluation are graphically illustrated in Figure 5; shown are the WASH-1400 "unsmoothed" results previously discussed together with the current perception in terms of probability per reactor year versus fission product release category. The picture that emerges from this assessment is that there are very few entries in the high consequence release categories, with almost all of the core melt sequences involving relatively small releases to the environment. Less than one percent of the core melts as assessed here fall into release categories that may lead to early fatalities. Up to this point the possible influence of steam explosions on the perceived risk remains to be addressed. but the implication is that the risk of core melt accidents is substantially less than indicated by WASH-1400. This reduction in the perceived risk is the result of the combination of the reduction in probability of some key accident sequences, the lowering of the magnitude of fission product source terms, and the currently inferred lower likelihood of early containment failures.

I then returned to the question of how would the occurrence of direct containment failure due to in-vessel steam explosions affect the current perception of reactor risk. If one goes back to the WASH-1400 probability of containment failure due to steam explosions, 0.01, given a core melt, the overall risk as assessed here would be lower than in WASH-1400, but the high consequence events would be dominated by the steam explosion failure mode. The probability of the steam explosion failure mode would have to be of the order of 0.0001 or less, per core melt, for it to be a small contributor to the overall risk. This is graphically illustrated in Figure 6 in which various probabilities for the steam explosion failure mode have been superimposed on the previous results. It may be noted that for purposes of this illustration all the steam explosion failures have been assigned to Release Category 1; the overall picture would rmain essentially the same if steam explosion failures were assigned to Release Category 2 or 3.

The above analyses and arguments are highly simplified and at least somewhat speculative, but the essential conclusion that emerges is that risk due to reactor accidents based on current perceptions is substantially lower than that derived in WASH-1400. As a reault of the prediction of relatively high probabilities of early containment failure due to other mechanisms, steam explosions were not a significant direct contributor to the risk as assessed in WASH-1400. With the decreasing overall risk as inferred from today's knowledge, and due to the large uncertainties that are still associated with the assessment of the likelihood and consequences of steam explosions, this containment failure mode may have a larger relative contribution to our current perception of risk than it did before. Or, from a different perspective, the uncertainties associated with steam explosions may limit our ability to quantify reactor risk. It may also be noted that containment failure due to steam explosions, while not directly addressed in BMI-2104, would still probably represent the upper limit to reactor accident consequences, as it did in WASH-1400.

Related Issues

Regardless of the magnitude of the probability of direct containment failure due to in-vessel steam explosions, the alpha mode, there are a number of related issues important to severe accident behavior that should not be ignored. Some of these issues are: steam and hydrogen generation from corium-water interactions, potential for debris dispersal, enhanced fission product release from the fuel, effects on fission product retention in the primary system, and the passibility of leading to or enhancing the likelihood of other containment failure modes. Some elaboration on each of these is provided below.

The energetic interaction of the core debris with water in the bottom of the reactor vessel is believed to be a likely occurrence in the event of core melting. Whether or not such interactions lead to failure of the primary system and/or containment, substantial rapid steam generation will typically be a result of these interactions. This steam generation should be taken into account in severe accident analyses. Such steam generation can affect primary system fission product transport, contribute to containment pressurization, affect hydrogen burning, etc. Similarly, the interaction of the core debris with water in the reactor vessel head can lead to substantial hydrogen generation. Based on experimental observations, the extent of hydrogen generation from such interactions can be greater than the total in-vessel generation predicted in some analyses. This hydrogen generation has obvious potential implication on containment loads, particularly in the event of burning.

The direct interaction of the core debris with the containment atmosphere has recently been highlighted as a potentially significant phenomenon that may occur during severe reactor accidents. While frequently the likelihood of the dispersal of the core debris into the containment atmosphere is believed to be limited to high pressure core melt scenarios, the energetic interaction of core debris with water may lead to such dispersal in low pressure melt scenarios as well. Among the reasons WASH-1400 stressed the potential importance of steam explosions was the enhancement of fission product release due to air oxidation of the core debris. Debris dispersal into the containment could also lead to the air oxidation of the matallic constituents of the debris, possible enhancement of hydrogen-oxygen recombination in otherwise nonflammable environments, and decreased potential for corium-concrete interactions.

As was previously noted, the generation of the large missile consisting of the reactor vessel head was the mechanism that linked in-vessel steam explosions with containment challenges in WASH-1400. The reality of such threats to containment integrity is, of course, the key issue being addressed by the review group. However, assuming energetic interactions between corium and water in the reactor vessel, there may also be other threats to containment integrity. The possibility of steam generator tube ruptures as a consequence of excessive primary system pressures have been mentioned previously. Interactions that lead to failure of the bottom head of the reactor vessel will likely lead to some motion of the reactor vessel, especially if the primary system is pressurized at the time of head failure. Such motion could lead to indirect containment failures, such as deformation of piping penetrations through the containment boundary. Such considerations appear particularly relevant to the smaller volume containment designs.

Tn order to illustrate the possible influence of steam explosions that do not lead to direct containment failure, I have assumed that one percent of the core melt sequences involve steam explosions that have an adverse effect on overall consequences. While such effects on consequences can be quite dramatic in some cases, for illustration purposes I have only assumed a shift to the next higher release category for one percent of the sequences. This is illustrated in Figure 7; it can be seen that even a small shift in consequences can have a significant effect on the likelihood of the high consequence release categories. This observation is an obvious result of the lack of high consequence releases in the perception of accident risk as developed here. While this illustration in not necessarily compelling, it illustrates the potential sensitivity of the results to even low probability events, if the latter can lead to higher consequences.

The indirect effects of steam explosions issues can significantly influence severe accident behavior and should be taken into account, even if a concensus can be reached that direct containment failure due to steam explosions is very unlikely, or even impossible.

Estimated Probability

In what follows I have attempted to address the development of a quantitative estimate of the probability of direct containment failure due to an in-vessel steam explosion. The process is quite judgemental and cannot be directly supported by reference to experimental evidence or quantitative analyses. I may offer the gratuitous comment that many of the so called "mechanistic" analyses suffer from the identical limitations.

In order to guide my thought processes I chose to separate the probability of containment failure due to steam explosions into a series of four factors. I then attempted to quantify each of these factors individually, with the overall probability being the product of the individual factors. The first of these factors, P1, represents the likelihood that water is present in the bottom of the reactor vessel at the time of core collapse and that a significant fraction of the molten core comes into contact with the water more or less coherently. This factor refers to the likelihood of having the initial conditions that are required for fuel-coolant interactions. The second factor, P2, is the likelihood that the molten fuel fragments upon contact with the water with a significant increase in the available heat transfer area. This factor defines the likelihood of some kind of interaction upon contact between the molten core material and water. The third factor, P3, is the likelihood that the interaction of the molten core with the water leads to the breach of the reactor vessel. Recognizing that a whole spectrum of interactions, from relatively benign to destructive, may be possible, this factor indicates the fraction of all possible interactions that may involve substantial damage potential. The fourth factor, P4, is the probability that the failure of the reactor vessel results in loss of containment integrity. It addresses the likelihood that the kinetic energy of the missile that may be generated will not be dissipated prior to reaching the containment boundary. Thus,

$$P(alpha) = P1 * P2 * P3 * P4$$

The individual factors were quantified on the following basis.

Probability	Definition
1	Certain, precludes the possibility of other paths.
0.9	Very likely, but alternate outcomes cannot be excluded.
0.1	Unlikely, other outcomes much more probable.
0.01	Very unlikely, representative of the tails of a distribution.

0.001 Believed to be impossible, but not demonstrable with existing knowledge.

These are, of course, order of magnitude estimates and little significance should be placed on small differences between individual or combined probability estimates.

Within the above framework the probability of the alpha mode of containment failure was assessed for both the large dry containment PWR and the Mark I pressure suppression BWR. These two types of designs may be considered to represent the extremes of the configurations to be encountered, particularly insofar as the layouts of the containments are concerned. The specific numerical values assigned to each are discussed below.

Large Dry Containment PWR

The question of the likelihood of water being present in the bottom head of the reactor vessel at the time of significant core melting has been addressed on numerous occasions in the past. It has generally been concluded that it is very likely that water will be present in the bottom head at the time of core slumping or collapse. The WASH-1400 analyses assumed that collapse of the entire core into the bottom head would take place when 80 percent of the core was molten. More recent analyses have concluded that it is very unlikely that the core can experience such extensive metling and still remain in the confines of its original location. In the BMI-2104 analyses, for example, initial core slumping was assumed to take place when the lowest node in any radial region became molten, without specifying the fraction of the total core that must be molten. Core slumping was typically predicted at core melt fractions of 40-50 percent. It is interesting to note that the IDCOR analyses for the PWR's assumed core slumping at comparable melt fractions. Based on a considerable history of analyses supporting the above observations, it is concluded that the likelihood of a significant fraction of the core being molten and having the potential of collapsing into water in the bottom head is highly likely; thus the first factor, P1, is estimated to be 0.9. The notable exception to this conclusion would be the occurrence of node-by-node fuel slumping leading to the early boiloff of the water in the bottom head prior to core collapse, thus precluding large scale interactions. The latter scenario is not believed to be representative of what is to be expected in core meltdown accidents.

The fragmentation of the core debris upon contact with water in the bottom head is believed to be almost a certainty. While there are significant questions regarding the extent and degree of fragmentation, the ocurrence of the breakup of hot molten material upn contact with a colder liquid is well established by numerous experimental observations with a variety of materials. Thus the second factor in the evaluation, P2, is estimated to have a probability of 0.9. The alternative would be the sinking of the molten core material to the bottom of the vessel and settling there without breakup; such an outcome is believed to be very unlikely.

While the quantification of the above first two factors appeared, at least to the writer, quite straightforward, the uncertainties in the last two factors appear to be almost overwhelming. The current state of knowledge of fuel coolant interactions indicate that the occurrence of such interactions is almost a certainty, but that the energetics or efficiency for doing mechanical work of such interactions may be quite low. Thus we can think in terms of a whole continum of severity of interactions, with only the most serious being of concern to the present discussion. Such a concept is imbedded in my definition of the third factor, P3, in the alpha mode probability equation; i.e., those interactions which will fail neither the bottom or top of the reactor vessel will not directly threaten containment and need not be further considered within the present discussion. While the concept itself is quite straightforward, assigning a defensible quantitative value to the likelihood of such extreme interactions is quite difficult. The body of analytical evidence available in this regard seems to indicate that interactions of sufficient damage potential to fail the primary system may require extreme combinations of circumstances, but cannot be dismissed as impossible given the available knowledge. In my judgement in-vessel corium-water interactions resulting in sufficient mechanical work to lead to failure of the reactor vessel are very unlikely; within the previously defined framework I would assign a probability of 0.01 to the third factor, P3. Given the limited knowledge on many factors such as the effects of scale, influence of confinement, actual time scales available for interaction, etc., there is obviously substantial uncertailnty associated with this factor; for purposes of discussion I will assume that it could be higher or lower by at least a factor of ten.

In the context of the large dry PWR containment, the fourth factor, P4, in the alpha probability equation is the likelihood that the missile resulting from the in-vessel steam explosion leads to failure of the containment. This factor is also very difficult to quantify and subject to substantial uncertainty. In order to fail containment the missile generated by the steam explosion must have sufficient kinetic energy to penetrate the missile shield and the polar crane with sufficient velocity remaining to damage the containment boundary. The energy dissipated at each step will depend on the nature of the interaction between the missile and the particular barrier. E. g., it requires more energy to penetrate the polar crane structure than to deflect the missile; similarly, it takes much more kinetic energy to completely penetrate the containment than it does to only fail the containment liner. WASH-1400 assigned a probability of 0.1 to this factor. The WASH-1400 analyses of the likelihood of the upper head missile failing containment did not address adequately the energy absorption potential of the polar crane. More recent work by Los Alamos indicates that the polar crane will represent by far the largest energy absorbing potential of any of the structures in the path of the upper head missile. In this light I would judge it very unlikely that even an energetic missile would lead to containment failure and assign a probability of 0.01 to P4. Here again, an uncertainty of an order of magnitude in either direction seems appropriate.

Taking the product of the individual factors estimated above leads to a probability of containment failure due to in-vessel steam explosions of 8E-5. Or, remembering that these are order of magnitude estimates and factoring in the range of uncertainties, the probability, P(alpha), of direct containment failure by steam explosions in large dry containment PWR's is estimated to be approximately 1E-4, with a range of 1E-6 to 1E-2.

Mark I Containment BWR

In the BWR, as in the PWR design, it is believed that there is a very high probability that water will be present in the bottom of the reactor vessel at the time of core slumping or collapse. Because of differences in the way the core is supported in the two types of designs there could be differences in the amount of fuel that is molten at the time that significant quantities of the core start to leave the original core confines. In the BMI-2104 analyses of the BWR sequences the fractions of core molten at the time of core slumping were approximately the same as for the PWR cases, though there were some cases in which the onset of slumping was predicted at lower core melt fractions. The IDCOR BWR analyses were based on the start of core slumping at 20 percent of the core molten. Thus for the case of the BWR's the first factor, P1, was assessed to be 0.9 for the present purposes.

The second factor, P2, the likelihood that the molten core material fragments upon contact with water in the bottom head was again assessed to be 0.9. There does not seem to be any particular basis why the likelihood of interaction in the BWR should be any different than in the PWR; this does not say anything about the extent and degee of interaction at this point.

Because of the different way that the core is supported in the BWR compared to the PWR, as well as retated differences in the geometries of the lower head regions, arguments can be made that the occurrence of very energetic corium-coolant interactions in BWR s are less likely than in PWR's. The fact that the core is supported from the bottom and the more constricted arrangement of the lower head in the BWR would appear to make large coherent interactions less likely. The confinement provided by the lower planum structures could, on the other hand, make any interactions that do take place more efficient in terms of their work potential. The structures above the core in a BWR may offer greater potential for absorbing energy from in vessel interaction; but the larger quantities of fuel and coolant that are present in the BWR offer the potential for greater interaction energies. As previously noted for the PWR case, the uncertainties in this area are very large. Since it is difficult to differentiate among the possible effects of a number of the above factors, each of which is poorly understood, I would judge that the likelihood of the breach of the primary system due to an in-vessel steam explosion is very low and of the same order as was the case for the PWR. Thus again the third factor in the equation, P3, is assigned a value of 0.01. In trying to assess the uncertainties about this value I would judge that it is unlikely that this probability is substantially higher than 0.01, but it could be an order of magnitude lower.

The assessment of the likelihood of containment breach by a steam explosion missile for the Mark I BWR differs substantially from that for the large dry PWR. Whereas there are several obvious barriers between the upper head missile and the containment bounday in the PWR, there are no such barriers in the Mark I design. The top of the reactor vessel is not far removed from the top head of the dry well, the primary containment boundary. Because of this proximity, relatively low energy missiles could impact and thus threaten the containment. Further, because of this proximity of the containment boundary to the primary system, it may not be necessary for the entire vessel head to become a missile in order to threaten containment; other modes of vessel failure may also have this potential. In light of such considerations, I would judge that the likelihood of containment failure in the event of an in-vessel steam explosion leading to the breach of the primary system is low, but quite possible. Thus the factor P4 is assigned a value of 0.1. I am not aware of any recent attempts at quantifying these probabilities for the Mark I containment design. In estimating the range of uncertainties that may be associated with this factor I find it difficult to justify a value appreciably lower than 0.1; but from a pessimistic viewpoint it could be assigned a value of 0.9.

Combining the above individual factors yields a probability of containment failure due to steam explosions of 8E-4. Again, recognizing that these are order of magnitude estimates and taking into account the stated ranges of uncertainty, the probability of direct containment failure due to in-vessel steam explosions for the Mark I BWR containment design is estimated to be approximately 1E-3, with a range of 1E-4 to 1E-2.

REVIEW OF NUREG/CR-3369, AN UNCERTAINTY STUDY OF PWR STEAM EXPLOSIONS

Comments on the Sandia study, "An Uncertainty Study of PWR Steam Explosions," NUREG/CR-3369, are given below.

General Observations

I have reviewed with interest the subject study utilizing the Monte Carlo approach for investigating the likelihood of containment failure due to in-vessel steam explosions. I was previously familiar with the earlier Sandia studies of this type. The approach is quite interesting and potentially powerful and I commend the authors for their efforts. There are inherent limitations in the approach used in the study as well as limitations in the physical models on which the study is based. As the authors of the study recognize, these assumptions and limitations make the results of the study less definitive than is desired.

I view the subject report more as a sensitivity study than as an uncertainty study. The extremely broad view of accident possibilities taken in this report lends insight on the sensitivity of the results to particular variables, but does little to narrow or focus the uncertainties that may be associated with in-vessel steam explosions.

The conclusions in this report regarding the most sensitive parameters affecting the likelihood of containment failure due to steam explosions are fairly obvious and can be derived without the benefit of the rather elaborate methodology. Nevertheless, the insights on the influence of a number of the other parameters are quite useful.

I was troubled by the use of the triangular probability distributions in the earlier Sandia studies using this approach, feeling that the choice of distributions tended to predetermine the results of the evaluation. I am even more troubled by the use of the rectangular probability distributions in the current study; these appear to lead to the unbounded range of zero to one for the probability of containment failure due to steam explosions. In retrospect, the triangular probability distributions would be conceptually much more preferable. The points at which the probabilities go to zere should, however, be broadened from those used in the earlier study; the peak magnitudes of the several variables should also be revised from those in the earlier study.

Specific Comments

1) I have for some time been concerned by the direct application of the conversion ratios or efficiencies as determined from experiments to the analysis of reactor accidents. While the experimentally inferred efficiencies or conversion

ratios are useful concepts, they are not at all precise and do not represent an understanding of the dynamics of the interactions; the use of such concepts is convenient precisely because a fundamental understanding of the processes involved is lacking. It is not clear to me, for example, that the work potential of high pressure steam generated in a steam explosion should differ substantially from thermodynamic limits. The reason why the experimental efficiencies appear to be much lower may be because we do not know how much of the fuel actively participated in the interaction and how much steam was generated and available to do work. I believe it was brought out during the discussions at Harper's Ferry that when attempts have been made to assess how much of the fuel actually participated in observed interactions, the values for the inferred efficiencies increased above the nominal values.

2) In light with the foregoing observation, it is not clear that the concepts of how much fuel participates in the interaction and the experimantally observed efficiencies are independent and should be used in combination. This may introduce some double accounting (or discounting) into the analysis.

3) The amount or fraction of fuel that participates in an interaction with water is not necessarily limited to that which is assumed to initially slump into the water. As the interaction in the bottom head proceeds, the mixture of fuel and coolant will expand and may come into contact with additional fuel, i.e., fuel that did not slump, or, additional material may fall into the water. Even if this additional fuel does not actively "participate" in the interaction, it may contribute energy to the expanding gases. Such additional heating, should it take place, will contribute to the work potential. The study recognizes and discusses the possibility of multiple interactions, but all the actual analyses appear to be limited to a single interaction.

4) The methodology utilized in the study assumes independence among the factors being considered. As the authors of the study recognize, there may be significant interdependencies or correlations among the variables in the study. A particularly significant potential correlation and one that is recognized by the authors is that between the heat content of the melt and the quantity of melt participating in the interaction. Ther are, no doubt, other interdependencies that could influence the results of the analysis.

5) The slug impact model used as the basis for determining vessel top head failure appears to be based on the stagnation pressure developed by the expanding gases. The inelastic collision between the materials in motion (possibly including fuel and structures as well as coolant) and the upper head would undoubtedly result in more energy transfer to the head.

6) The energy required to breach the missile shield is determined on the basis of the upper head penetrating this

shield. Alternate modes would appear to be possible, but may not be completely relevant to the particular design being considered. If, for example, the missile shield is not fastened down, much less energy would be required to move it aside than to penetrate it. If the missile shield is fastened down, what are the relative energy requirements for failing the shield holddown structures as compared to penetration of the shield? I suspect that the missile shield and its holddown, if any, were designed to restrain possible control rod drive missiles and not the top head of the reactor vessel.

7) The failure of the containment is based on the penetration of the containment shell by the upper head missile. Functional failure of the containment would take place with only the containment liner being broken; this would not necessarily require complete penetration of the containment shell by the missile. The differences in the required missile energies for the two cases could be substantial.

8) The potential of the building crane for restraining the upper head missile is apparently not considered in the analysis; this could be quite considerable. I believe that the Los Alamos analyses presented at the Harper's Ferry meeting indicated that the building crane was the largest potential dissipator of missile energy. The complete bypass of the crane by the large upper head missile does not appear to be likely.

REVIEW OF STEAM EXPLOSION EXPERIMENTAL PROGRAM

Introduction

The foregoing discussion indicates that direct containment failure due to in-vessel steam explosions was not a major direct contributor to reactor risk as assessed in WASH-1400, and is probably of even lesser significance in light of today's knowledge. There are, however, many indirect effects associated with steam explosions that can have a significant bearing on the course and consequences of reactor meltdown accidents. Thus it is still necessary to have an understanding of such events for the evaluation of severe accident phenomena. Further, containment failure due to steam explosions, the alpha mode, is still representative of the most severe consequences of reactor accidents.

Background

In reviewing the steam explosion experimental program and considering the need for additional experiments, there are several points of concern that keep coming to mind. These concerns relate to my view of the history of research in this area and are not necessarily directly related to the Sandia proposal; they do, however, impact on my perception of the need for and the type of further work that may be required in this area.

The first of these concerns relates to the perceived explosivity of corium over the past several years. I recall very vividly the results of some eary small scale experiments that indicated corium in water to be nonexplosive. These early observations led many to declare steam explosions to be a nonissue. However, when the interaction experiments were extended in scale to about 5 kg of corium, spontaneous explosions were readily obtained. This history asks the obvious question of how many more surprises remain as we try to move closer to prototypical conditions.

The second concern relates to an earlier steam explosion program review in which I had the opportunity to participate. Among the recommendations of this earlier review, chaired by Dr. Tong, was the need for experiments that simulate the boundary condition constraints representative of those imposed by the reactor vessel. A few such an experiments have recently been conducted (Sandia Rigid Container Test Series). While the results of these experiments are still being analyzed and litle quantitative information is available, it is my impression that the efficiency or work potential observed in one of these experiments appears to be substantially higher than in similar experiments without external constraint. Such a possibility was, of course, one of the principal concerns that motivated the recommendation for such experiments. Assuming that quantitative results of the experiments bear out this impression, it would appear that more attention should be paid to the constraints imposed on future experiments. Similarly, the effects of internal structures should be further considered.

The third concern relates to a point that was stressed by Dave Squarer at the recent Harper's Ferry review meeting, i.e., apparently there have been no experiments conducted with corium in saturated water. If this indeed the case, it would seem imperative that such a combination should be tested at the earliest possible opportunity. While saturated water is not the only condition that can exist during core melt accidents, it appears to be the most likely. In view of the importance of this condition and the extensive history of discussions related to the effect of water subcooling, or the lack thereof, the demonstration of the explosivity of corium in saturated water should be an essential element in the program.

Need For Large Scale Experiments

In judging the need for large scale experiments one must ask the question of what is the motivation for such experiments, or what is to be learned from them. Because of the inherent difficulty and cost of large scale experiments, presumable they would be limited in number, the larger the scale the more limited the number. Thus it would be desirable to have at least some experimental bachground with small scale experiments. In the area of reactor steam explosion research we have the added complication of having to deal with simulant materials, since working with the prototype materials is extremely difficult and may not be possible at large scale.

The objectives of large scale experiments may include the establishment and/or development of a scaling law, demonstration or proof testing, or a combination. Assuming the existence of a data base at some (small) scale and a model that correlates the data, one would want to demonstrate the applicability of the model to both the prototype as well as the simulant materials. Given such a model and demonstrated correlations, the logical step would be to demonstrate scaling by means of large(r) scale experiments with the simulant materials. If the model correlates the simulant experiments at several scales as well as the prototype materials at one scale, there should be reasonable assurance that the model will correctly predict large scale prototype behavior. In the area of reactor steam explosions we have a considerable data base with various simulant materials at several scales (but all relatively small), a much more limited data base with prototypical materials, but lack an adequte model that can correlate (explain) the experimental observations. Given this situation, the conduct of large scale experiments for the purposes of establishing or demonstrating a scaling law would appear to be premature.

Demonstration or proof tests may not necessarily require the existence of a good analytical model; in fact, the inability to adequately model certain complex phenomena may be the motivation for such tests. Examples that come to mind are crach tests of automobiles and the full scale crash tests of spent fuel shipping casks. To be meaningful, however, demonstration tests in the sense considered here would imply the use of large (full) scale and the use of prototypic materials. Such an approach would clearly not be considered for the problem at hand.

Many of the prevailing controversies regarding the likelihood and consequences of in-vessel steam explosions relate to various postulates to limits of fuel-coolant mixing that would tend to limit the extent of interaction at large scale. It is difficult to envision how such postulates or theories can be proven or disproven without the availability of experimental data at suitably large scale. These postulates, however, typically involve assumptions about effective mixing lengths and time scales available for mixing; the former in particular are not a readily observable experimental variables. Thus in the absence of a well established analytical model to aid in the interpretation of the experimental observations, there could be considerable danger of misinterpretation of the results of large scale experiments. If large scale experiments invariably lead to no explosions, or a series of small interactions, one may come away with the feeling that the theories for the limits to mixing have been validated. If, on the other hand, the work potential is observed to increase with scale, but cannot be adequately quantified, one may be forced to conclude that damaging steam explosions are more important than currently believed.

Thus while there appears to be a definite need for large scale steam explosion experiments, the conduct of such experiments should be predicated on the availability of reasonably established analytical models as well as adequate diagnostic tools for the interpretation of the experimental observations.

Recommendations

Sandia has proposed a comprehensive experimental program of steam explosion research involving a number of facilities. A high priority is assigned to the large scale (SEALS) experiments. Based on my view of the significance of steam explosion phenomenology to reactor risk, as articulated in other parts of this report, a narrower and much more focused experimental program is recommended; also, the experimental program must be closely coordinated with the development and application of analytical models.

While the SEALS proposal is attractive in many respects, it is not clear that the experimental diagnotics and the analytical models will be available to successfully interpret and apply the results of such experiments. Thus I view the proposal as being premature at this time.

The types of experiments that are recommended have been noted earlier. These would include demonstration of the explosivity or nonexplosivity of corium in saturated water, the further investigation of the effects of internal and external constraints, and the determination of how much of the available corium or simulant actually participates in the observed interactions. These experiments should utilize existing facilities, e.g., FITS, but some priority should be given to improved diagnostics.

The development and application of analytical models should be performed in conjunction with the experimental program. Further utilization of the Mont Carlo approach in conjunction with improved physical models would appear to be fruitful; recommendations of the several reviewers should be taken into consideration in further application of this methodology. Given the availability of a meaningful model of steam explosion phenomenology, which does not appear to exist at this time, it may be possible to develop much simpler experiments to establish the effects of scale than those that have been proposed up to now. I may note that the steam explosion model need not necessarily take the form of an elaborate numerical simulation; the latter may have the same problems of interpretaion as large scale experiments.

FIGURE 1

RSS RESULTS

MEDIAN VALUES

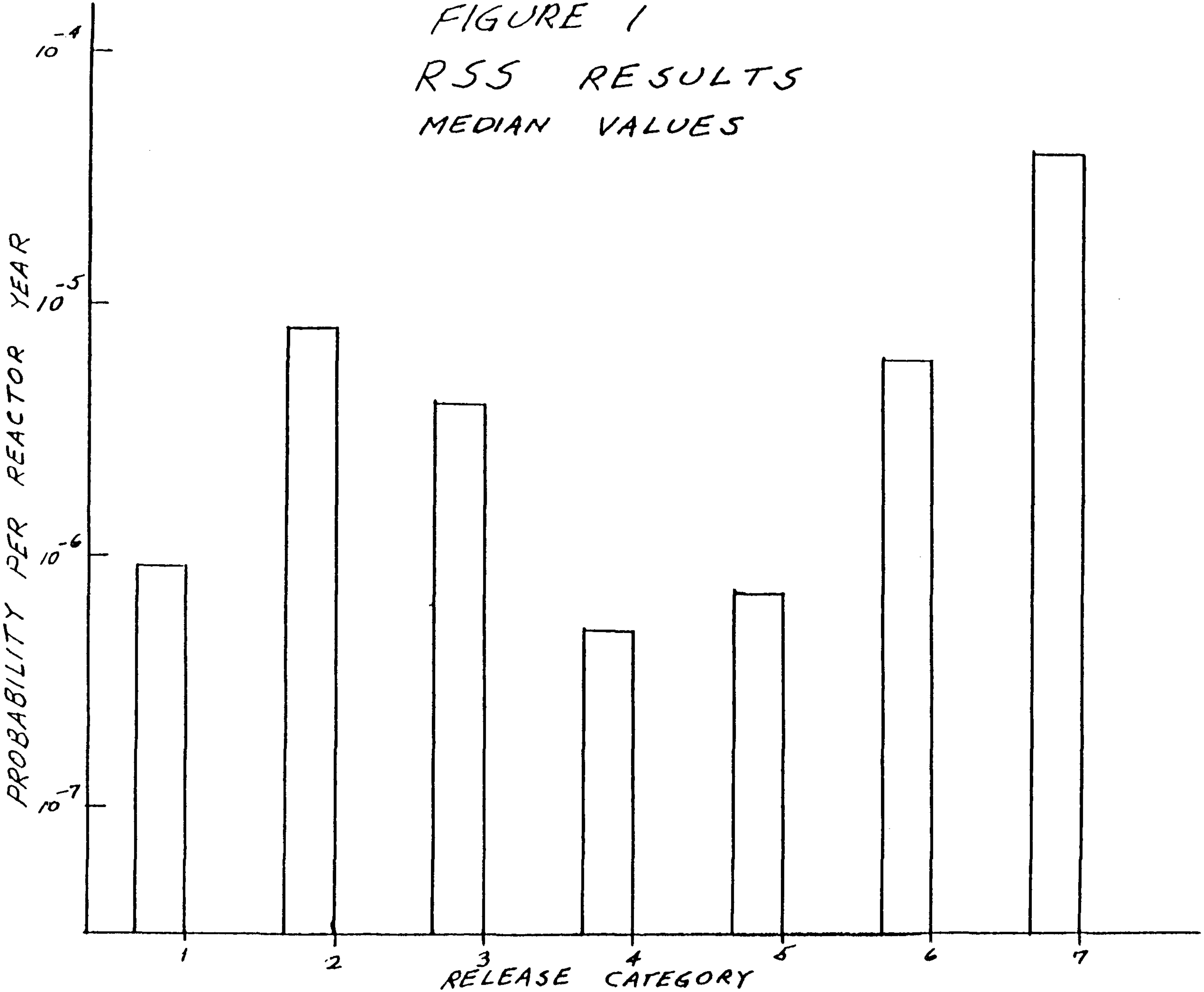
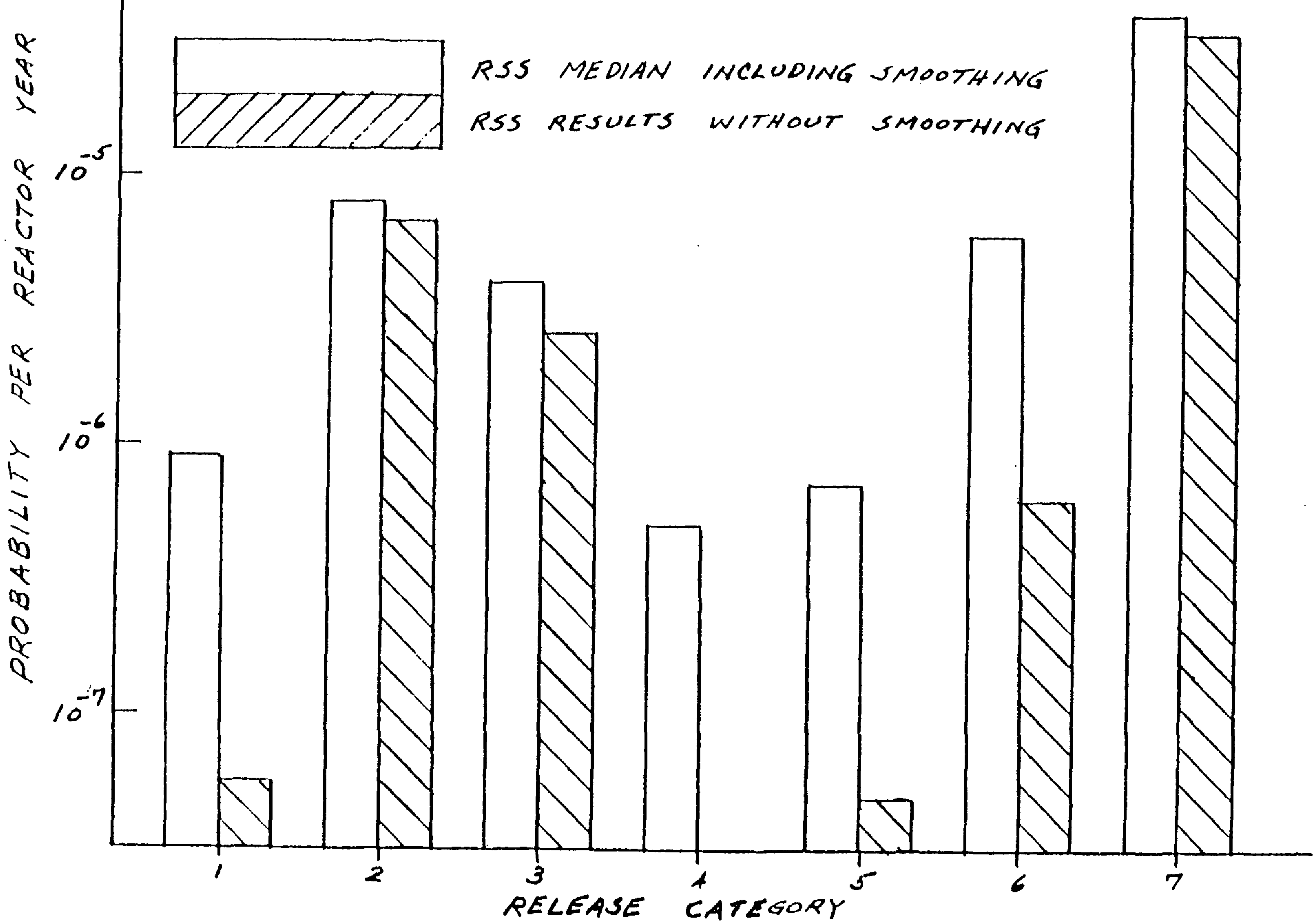
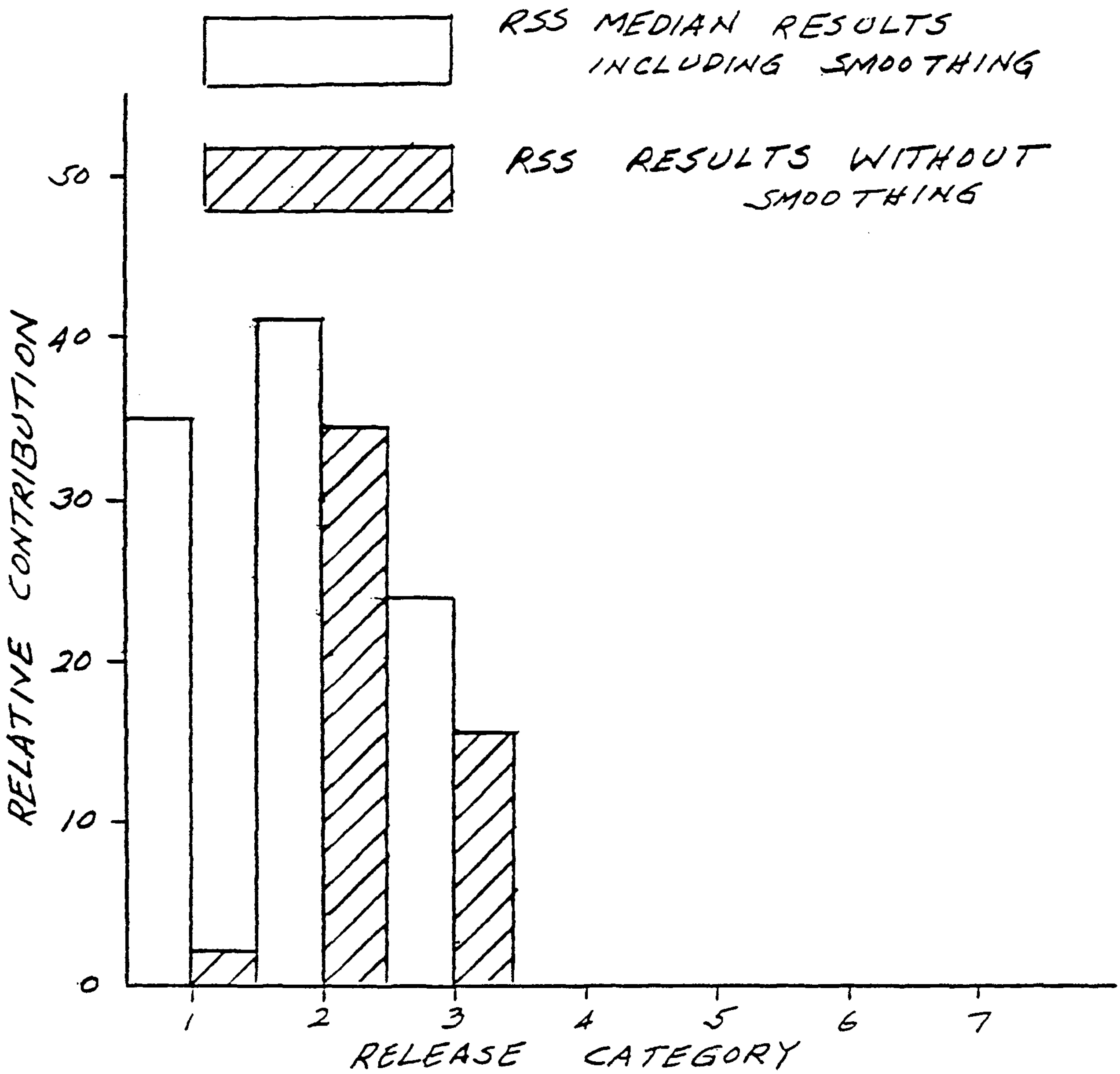


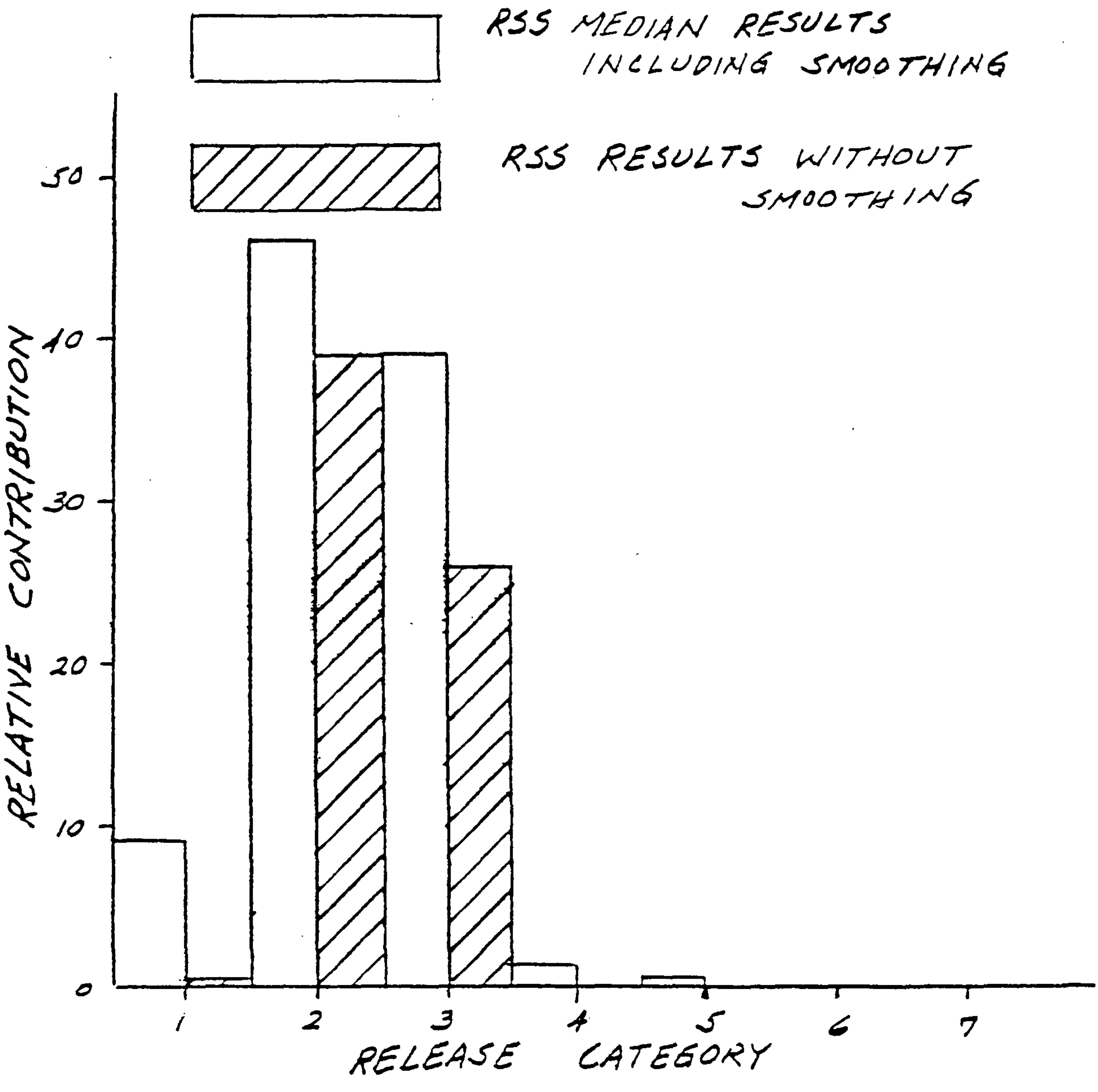
FIGURE 2
RSS RESULTS
WITH AND WITHOUT SMOOTHING





CONTRIBUTIONS TO RISK OF EARLY FATALITIES

FIGURE 3



CONTRIBUTIONS TO RISK OF LATENT FATALITIES

FIGURE 4

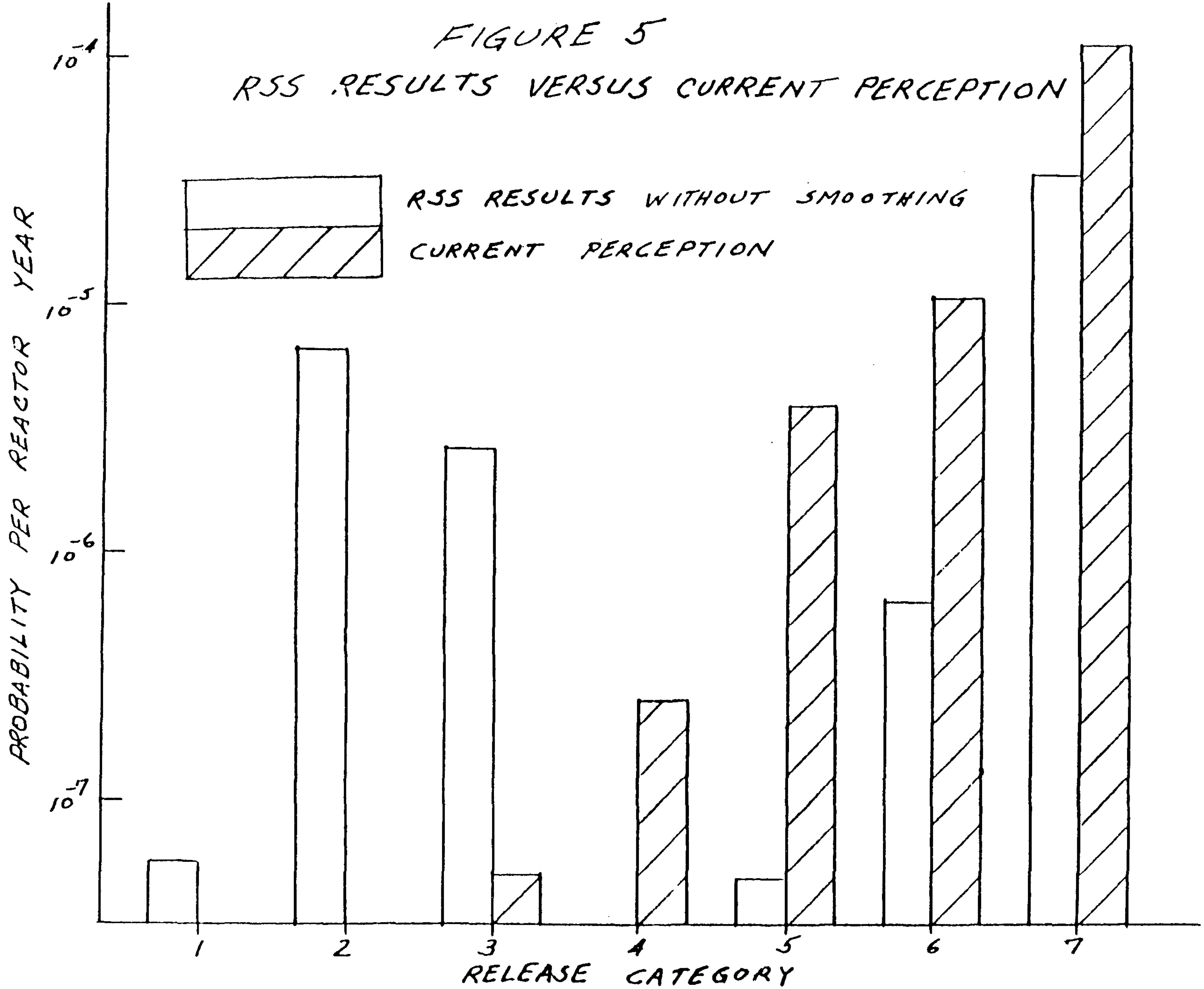


FIGURE 5
RSS RESULTS VERSUS CURRENT PERCEPTION
RSS RESULTS WITHOUT SMOOTHING
CURRENT PERCEPTION
PROBABILITY PER REACTOR YEAR
10^{-4}
10^{-5}
10^{-6}
10^{-7}
1
2
3
4
5
6
7
RELEASE CATEGORY

FIGURE 6

CURRENT PERCEPTION WITH EFFECT OF ALPHA MODE

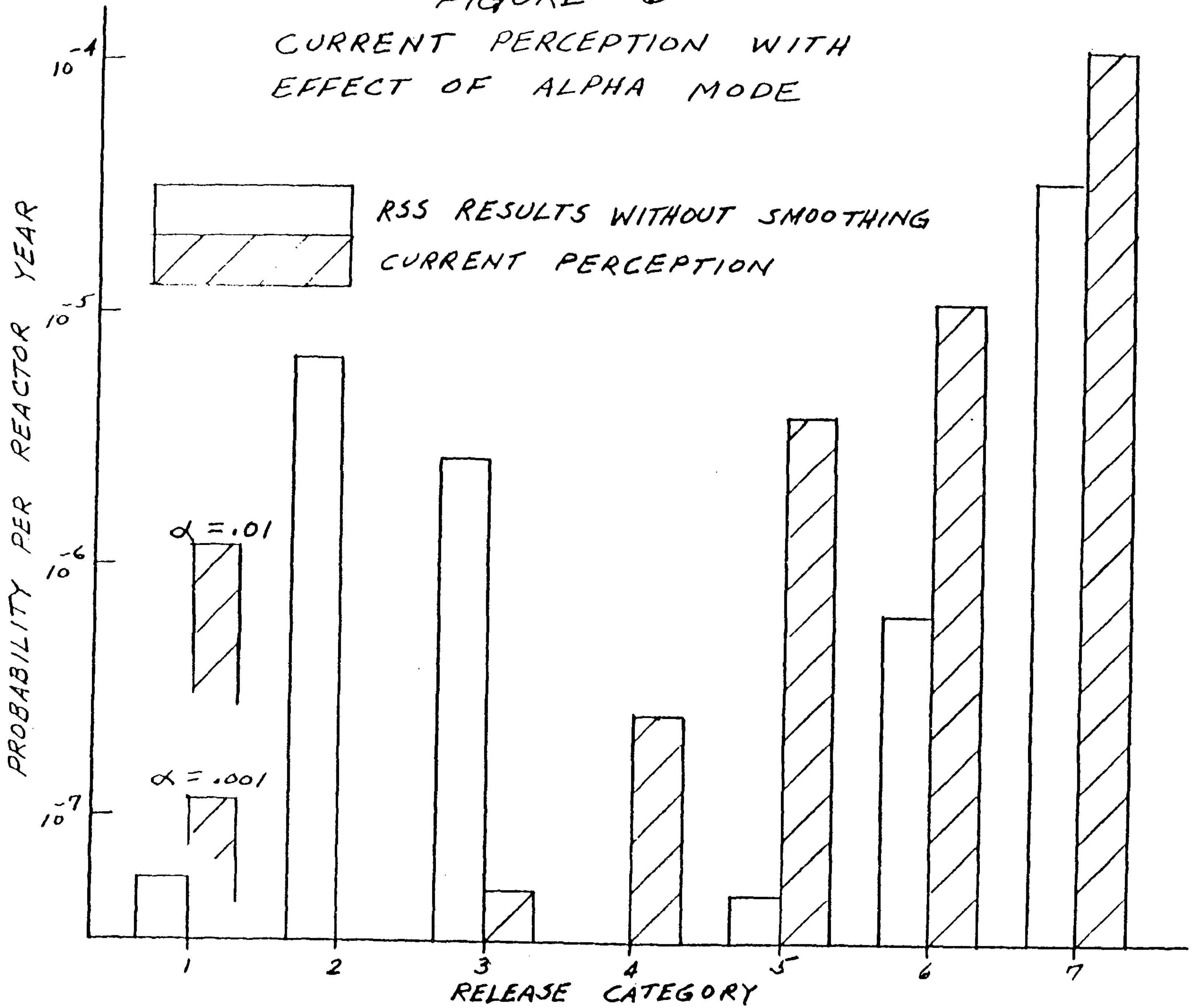
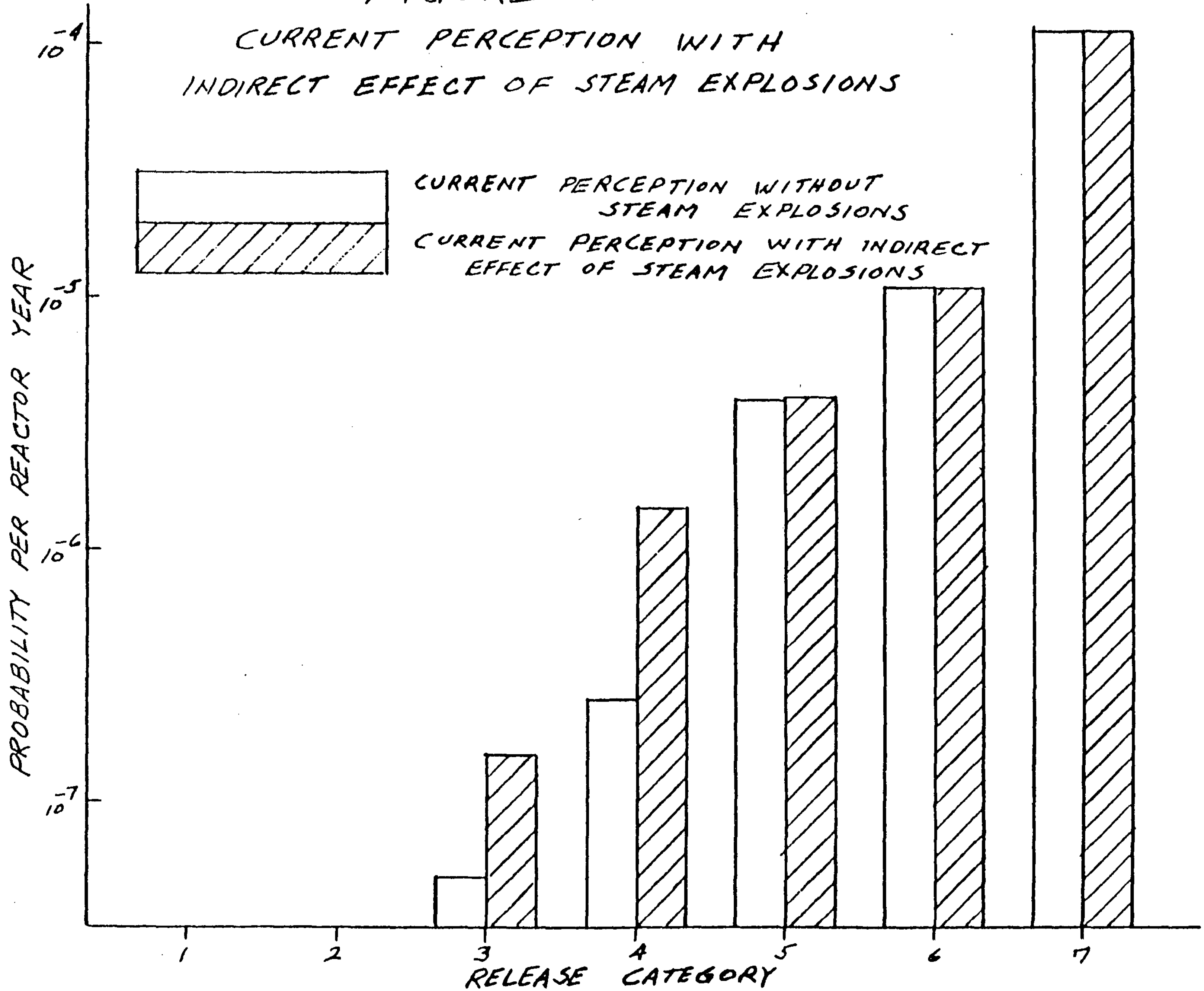


FIGURE 7



CURRENT PERCEPTION WITH
INDIRECT EFFECT OF STEAM EXPLOSIONS
CURRENT PERCEPTION WITHOUT STEAM EXPLOSIONS
CURRENT PERCEPTION WITH INDIRECT EFFECT OF STEAM EXPLOSIONS
PROBABILITY PER REACTOR YEAR
10^{-4}
10^{-5}
10^{-6}
10^{-7}
1
2
3
4
5
6
7
RELEASE CATEGORY

FAUSKE & ASSOCIATES, INC.

November 1, 1984

Mr. Cardis Allen
U.S. Nuclear Regulatory Commission
Mail Stop AR5003
Washington, D.C. 20555

Dear Cardis:

In preparing for the next Harper's Ferry meeting, we are enclosing some additional considerations concerning the steam explosion issue in connection with light water reactor severe core damage accident evaluations.

Given a core melt, we continue to judge that the probability of early containment breach is vanishingly small, i.e. there is no need to include such events in establishing the risk profile in connection with severe core damage accidents.

We consider the above judgement to be generic to all existing reactor plants although we note that the actual details concerning fuel debris - coolant interaction and mixing, heat transfer, steam generation, etc. are generally assessed to be system - as well as sequence-dependent.

In regard to the Sandia study reported in NUREG/CR-3369, we note that the report correctly highlights the most important parameters in the steam explosion process. However, in the absence of mechanistic considerations of these parameters, the report would be better served by characterizing it as a "sensitivity" rather than an "uncertainty" analysis.

If any questions should arise concerning the attached material please feel free to call at any time.

Sincerely yours,

Hans

Hans K. Fauske, President
Fauske & Associates, Inc.

HKF:jab
Enclosure

cc: M. H. Fontana, TEC
E. L. Fuller, EPRI
R. E. Henry, FAI

Corporate Office: 16W070 West 83rd Street • Burr Ridge, Illinois 60521 • (312) 323-8750

STEAM EXPLOSION IN CONNECTION WITH LIGHT WATER REACTOR SEVERE CORE DAMAGE ACCIDENTS*

By

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16W070 West 83rd Street
Burr Ridge, Illinois 60521
(312) 323-8750

HIGHLIGHTS

Additional evaluations are provided concerning the damage potential of a steam explosion occurrence in connection with severe core damage accidents. Special emphasis is given to the potential for fuel jet breakup and the existence of a continuous liquid slug. The conclusion continues to remain the same, i.e. that steam explosions sufficient to challenge containment integrity are not physically credible. In terms of probability, this says that a nonzero probability would only arise as a result of uncertainties in the physical models. Given the fundamental nature of the physical processes modeled and the comparisons with experiments, the uncertainties are second order. With the large margin between the required mass of core debris and the calculated masses which could interact, the probability of early containment breach due to a steam explosion occurrence is assessed to be vanishingly small.

*Material prepared for the Harper's Ferry Meeting on Steam Explosions, November 27-28, 1984.

STEAM EXPLOSION IN CONNECTION WITH LIGHT WATER REACTOR SEVERE CORE DAMAGE ACCIDENTS

1.0 INTRODUCTION

Since the Reactor Safety Study [1] and the German Risk Study [2] appeared in the 70's, a number of more recent assessments have been offered dealing with the damage potential from steam explosion in connection with light water reactor severe accident evaluations.

While Henry and Fauske [3], Theofanous and Saito [4], and Mayinger [5] have concluded that the probability of direct containment failure from a steam explosion occurrence is vanishingly small, Refs. [6] and [7] claim that the uncertainties are still too large to justify such a conclusion.

It is generally acknowledged that in order for a steam explosion to challenge the LWR containment the following two conditions must be satisfied.

- A large fraction of the fuel inventory (several tens of tonnes) must be finely intermixed with the coolant on a relatively short time scale.

- A coherent liquid column must be available in order to effectively transmit the steam explosion expansion work to the upper head in the form of a large dynamic impact pressure.

Both conditions were arbitrarily assumed to be satisfied in the WASH-1400 study and the German Risk Study, and continue to surface in recent assessments [7]. Mechanistic evaluations of these assumptions are summarized below.

2.0 MIXING POTENTIAL

Since the interface contact temperature (T_i) between molten fuel and water is well above the spontaneous nucleation temperature (T_s), coarse fuel-coolant intermixing followed by explosive interaction is possible [8] and has been demonstrated for reactor-like materials on a small scale (10-20 kg) [7]*. As illustrated in Fig. 1, a quasi-stable coarse premixture of fuel (dimensions of order 1 to 10 mm) in water is a prerequisite for a coherent large scale steam explosion [6].

The breakup length of a molten fuel jet entering water in the film boiling mode is to first order given by [9]

$$L \sim d\sqrt{\frac{\rho_f}{\rho_x}} \tag{1}$$

where L is the breakup length, d is the initial fuel diameter, ρ_f is the fuel density and the value of ρ_x depends on the vapor blanket thickness, h. If h is substantially larger than the characteristic wavelength $\lambda \sim \sigma/(\rho_g u^2)$, ρ_x can be set equal to the vapor density ρ_g, where σ is the surface tension and u is the relative velocity. If h is less than λ, ρ_x can be set equal to the liquid density ρ_ℓ.

However, the use of liquid properties in Eq. (1) must be relatively short lived, since it implies fine entrainment of molten fuel with a characteristic size of $\sim \sigma/(\rho_\ell u^2)$, i.e. submillimeter size. This would lead to extremely rapid vapor growth, suggesting the use of vapor properties in Eq. (1).

The rapid vapor growth would appear to be demonstrated by experiments by Anderson and Armstrong [10]. In these experiments, 10 cm jets of Freon-22 were allowed to penetrate into hot water. The system clearly satisfied film boiling conditions ($T_i > T_s$), and a rather thick vapor blanket (> 2.5 cm) was noticed on a time scale of less than 100 ms. Film boiling heat transfer based upon the jet surface area, would suggest a vapor film thickness of the order of 1 mm, i.e., one should clearly use liquid properties in Eq. (1). As indicated above, this would lead to very fine surface entrainment of Freon-22 into hot water and hence explain the development of a thick vapor blanket in a relatively short time.

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*Partial breakup of the fuel due to the "thermite" technique may have occurred in these tests prior to the fuel penetrating the pool of water.

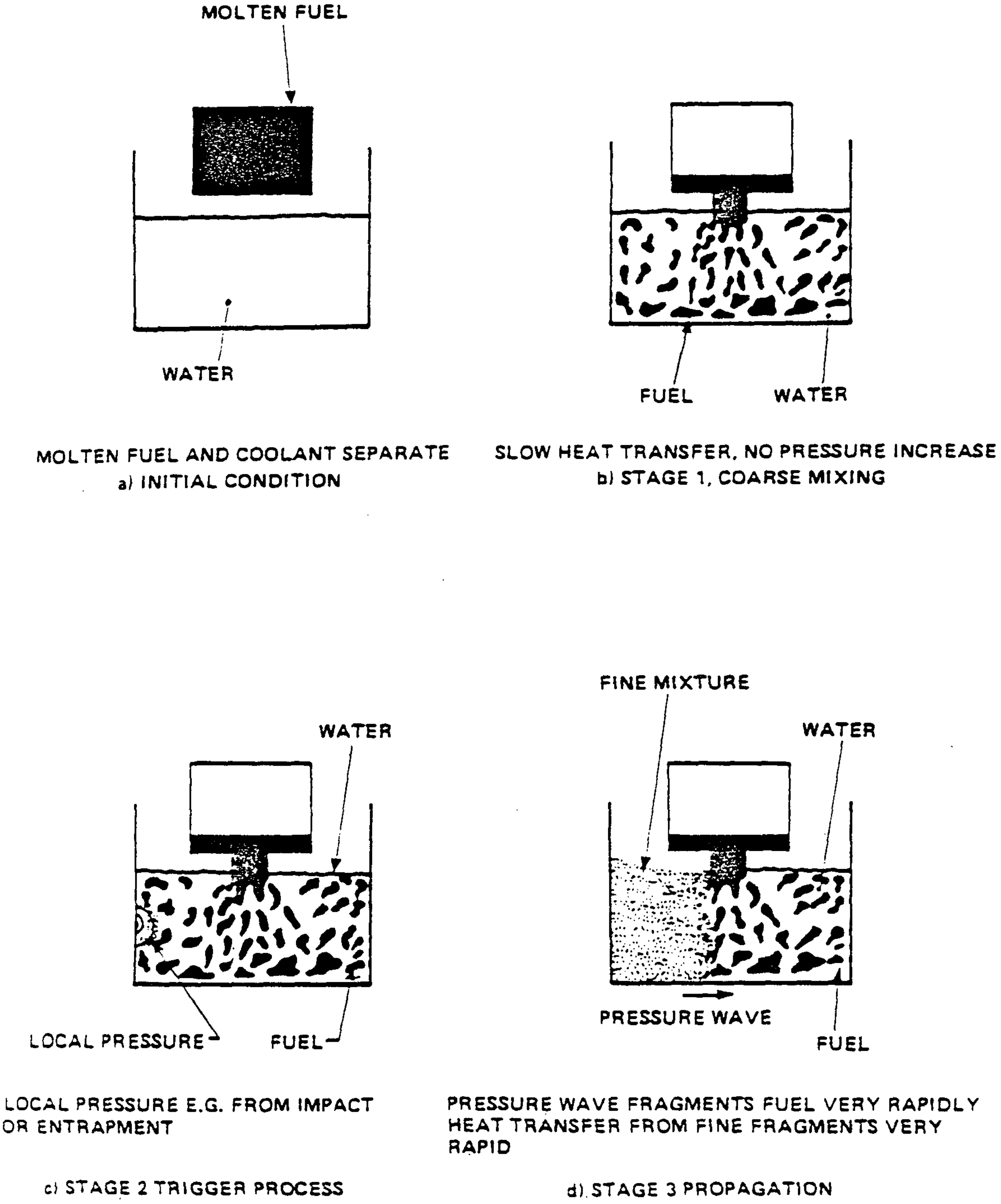

Fig. 1 The stages of a steam explosion (taken from Ref. [6]).

For the fuel-water case, the large film boiling heat flux due to radiation (~ 3 Mw/m^2) may by itself result in a sufficiently thick initial blanket to dictate the use of vapor properties in Eq. (1) [9]. In any case, a small amount of entrainment would assure a sufficiently thick vapor blanket. Considering a necessary pour diameter of ~ 1 m in order to challenge the containment [7], Eq. (1) suggests a breakup length of ~ 108 m, i.e. much larger than the available pour length of ~ 2 m. We conclude that the molten fuel as it penetrates down into the lower plenum remains largely separated from the water prior to potential explosive interaction*.

Instead, the extent of possible fuel-coolant mixing is largely limited to surface mixing following breakdown of the vapor blanket, i.e. the initiation of a possible explosive event (see Fig. 2). This lead Theofanous to suggest an upper bound for in-vessel fuel-coolant intermixing of ~ 3000 kg [4]. Quoting Theofanous directly, "Even for the unlikely large diameter of 3 ft., and an unrealistically high surface depth mixing of ~ 10 cm, a fuel quantity of ~ 10 ft^3 is estimated".

To a first order the extent of surface mixing or hydrodynamic breakup following implosion of the vapor blanket and propagation along the interface between two fluids in an initially stable film boiling mode can be estimated from

$$d \sim \frac{1}{T^*}\left(\frac{\rho_c}{\rho_f}\right)^{1/2} u \cdot \tau \qquad (2)$$

where d is the depth of surface mixing, ρ_c and ρ_f are the coolant and fuel densities, respectively, u is the initial differential mixing velocity, τ is the effective time available for mixing, and T^* is the nondimensional time for complete breakup found to be ~ 3 [13]. Based upon experiments and analysis [10,13] demonstrating propagating explosions including initially separated

*Furthermore, assuming fuel break could occur, hydrodynamic limitations due to boiling (steam generation) also rules out the possibility of fuel-coolant premixing on a large scale [3]. This limitation to mixing has also been discussed by Theofanous [4], Corradini [11] and Bankoff [12].

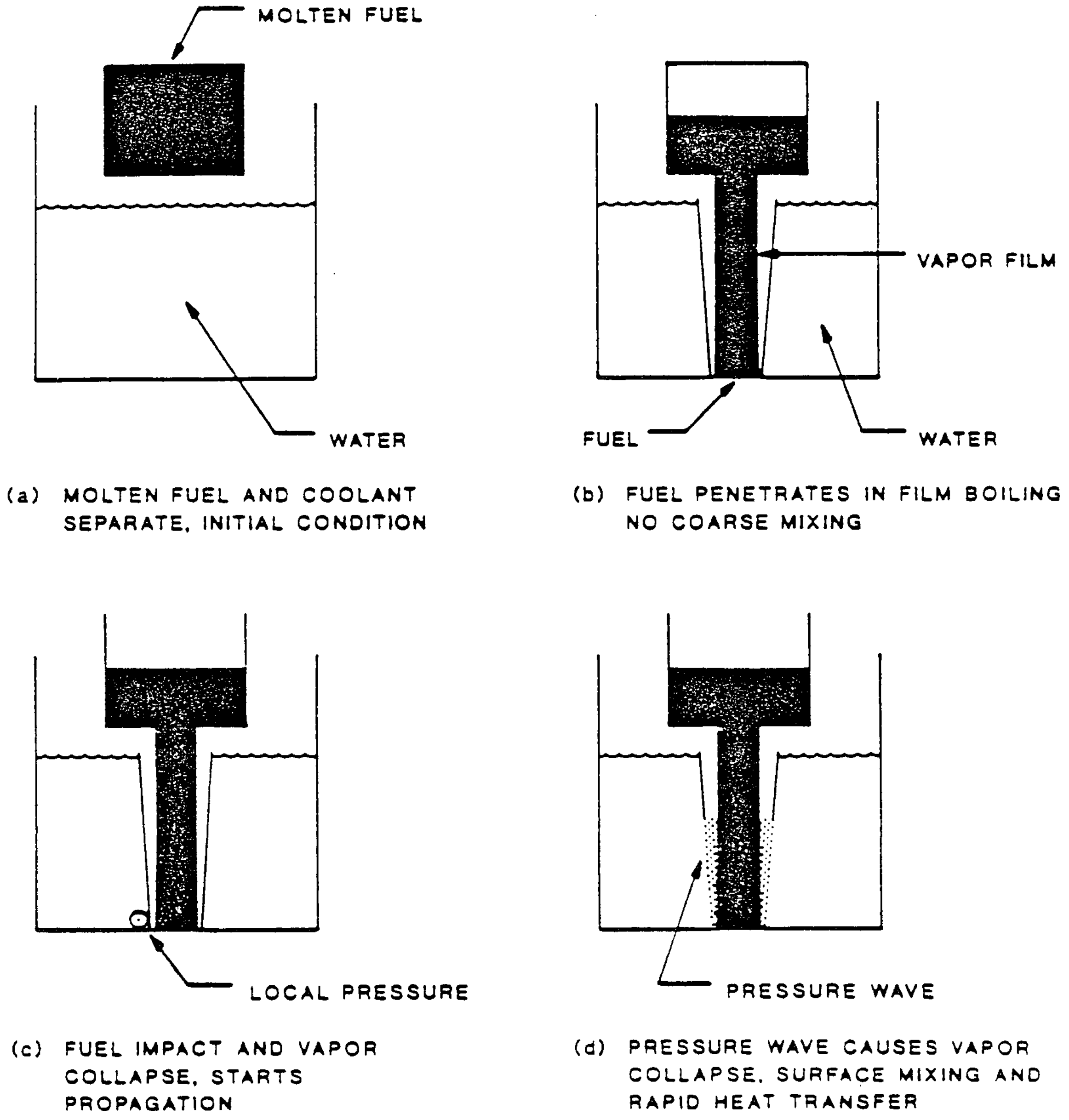

Fig. 2 Illustration of a steam explosion in the absence of coarse premixing.

systems, u is generally found to be less than 100 m/s and τ to be less than 1 ms.

For the highly constrained and well instrumented Freon-water explosion experiments by Anderson and Armstrong [10]* Eq. (2) suggests a surface mixing depth of $\sim$ 0.03 m. This compares well to the experimentally interpreted mixing depth of 0.025 m [10].

Extending the above analysis to the reactor case with $\rho_f/\rho_c \sim 7$, $u \sim 100$ m/s and $\tau \sim 0.001$ s, suggests a surface mixing depth of the order of 1 cm. For a pour diameter of 1 m and pour length of 3 m this amounts to about 700 kg of fuel. But more importantly the rate of fuel mixing is only about 70 kg/ms considering that typical propagation rates are found to be of order several hundred meters per second [6]. Assuming for illustration purpose a hypothetical spherical fuel geometry, the percentage of fuel that has the potential to mix with the coolant is illustrated in Table 1 as a function of initial fuel mass.

In the case of an unreasonably large initial mass of 100,000 kg entering the lower reactor plenum vessel, the mixing potential is estimated to be limited to about 2000 kg ($\sim$ 2% of initial mass). However, taking the propagation velocity to be of the order of 300 m/s, the time for mixing this quantity of fuel is estimated to be about 16 ms, i.e. the rate of mixing is only about

Table 1

SCALE-UP CONSIDERATION OF FUEL-COOLANT MIXING
BASED ON SURFACE MIXING (DEPTH $\sim$ 0.01 m) AND
CONSIDERING SPHERICAL GEOMETRY AND NO PREMIXING

Initial Fuel Mass, kg	% Fuel Mixed	% Fuel Mixed Per ms
10	$\sim$ 40	$\sim$ 50
1,000	$\sim$ 10	$\sim$ 3
100,000	$\sim$ 2	0.1

*In these experiments, the Freon-22 was brought under the water surface as one coherent mass by constraining the Freon in a plastic balloon.

100 kg/ms or 0.1% of the initial fuel mass. This is in contrast to the existing range of experiments (∿ 10 kg of fuel) where a relatively large fraction of the fuel mass (∿ 50%) is calculated to have the potential of mixing with the coolant on a millisecond time scale.

In addition to the low overall mixing potential predicted for the reactor case, the extremely low rate of mixing eliminates any potential for direct bottom vessel failure and further highlights the necessity for collecting and focussing the explosion energy, i.e. the presence of an overlying coherent liquid column is required in order to produce significant damage to the reactor vessel head.

3.0 COHERENT LIQUID COLUMN POTENTIAL

Since at the time of significant fuel melting all the water is already removed above and within the core, the absence of a long water column for containing and directing the steam if it could be rapidly generated is indeed absent.

In this context, it is important to review the WASH-1400 analysis which assumed a one-dimensional piston of fluid with the mass of *half the interacting water and fuel**. According to this assumption relatively low-pressure steam generation, (e.g. up to hundreds of atmospheres) could accelerate the liquid slug over a long time, (e.g. of the order of 100 ms corresponding to a travel distance of ∿ 10 m) (see Fig. 3). The accumulated momentum could thus be transformed into a dynamic impact pressure which might involve pressures of thousands of atmospheres. Such an impact could potentially form a missile out of the upper head resulting in early containment failure and a large escape path for radioactive material [1].

It appears, however, that the WASH-1400 analysis assuming an intact liquid piston can be dismissed by considering the well known Taylor instability phenomena. On this basis it has been shown that a liquid column subjected to acceleration will break up according to [14]

*For all cases considered in Ref. [7] a slug was always assumed to be present.

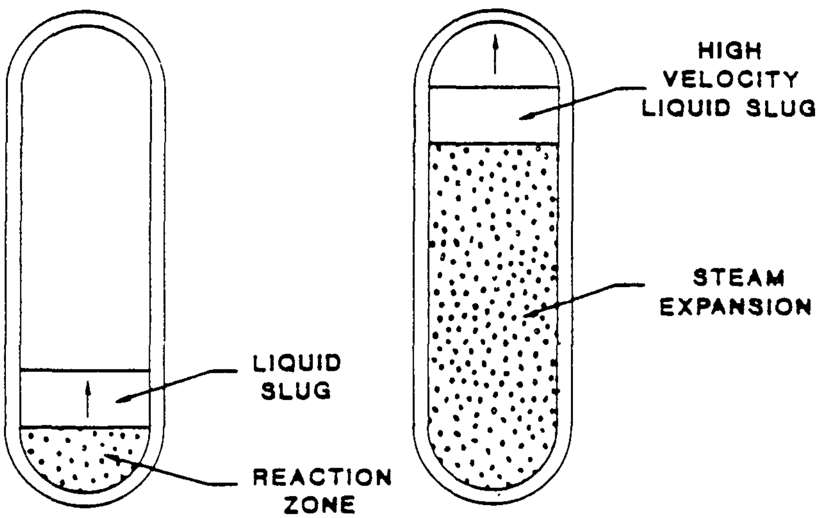

Fig. 3 WASH-1400 assumed liquid slug behavior.

$$s \sim K \frac{\ell^2}{d} \qquad (3)$$

where s is the length over which the liquid slug with initial length ℓ and diameter d has accelerated, at the instant the gas penetrates the depth of the slug, and K is a dimensionless constant approximately equal to 2.6 (see Fig. 4). Setting $\ell \sim 2$ m and $d \sim 4$ m for the reactor case, the assumed liquid piston will remain intact for only ~ 2.6 m travel (see Fig. 5)*. This is to be compared to ~ 10 m of travel before the assumed intact liquid piston in the WASH-1400 analysis would impact the vessel head (see Fig. 3). The presence of numerous internal structures would further accelerate such breakup. This latter effect was also ignored in the WASH-1400 study.

4.0 CONCLUDING REMARKS

Since the controversy over "steam explosion" continues to exist its potential for causing early containment breach in connection with light water reactor severe core damage accidents has received further evaluation with the following results:

*Energy dissipation due to slug breakup and dispersal will largely mitigate the impulse seen by the reactor vessel head as compared to the assumed intact slug case.

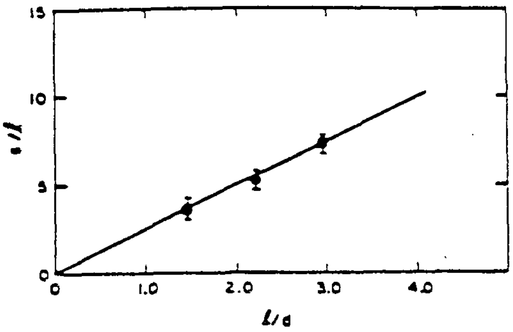

Fig. 4 Experimental data (∿ 60 tests illustrated in terms of dimensionless breakup distance vs. dimensionless column height (taken from Ref. [15]).

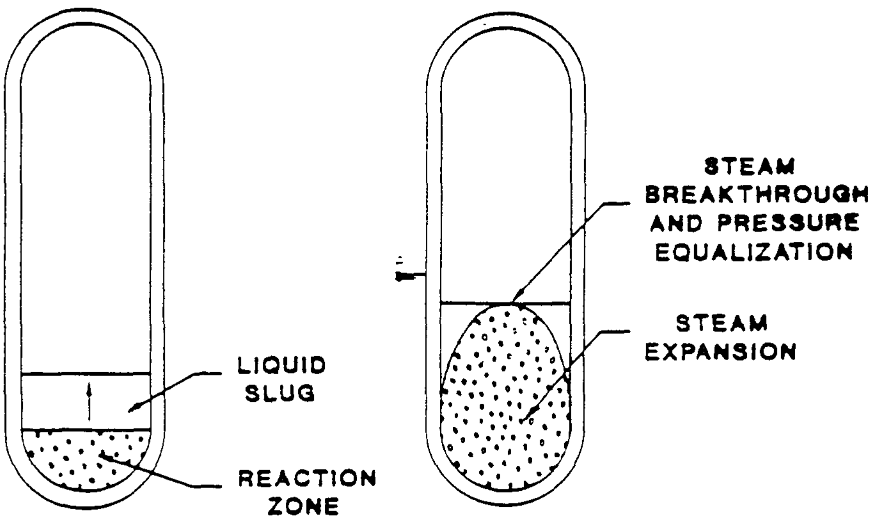

Fig. 5 Predicted liquid slug behavior.

- Little or no quasi-stable premixing is predicted for conditions of interest to the reactor case.

- The fuel-coolant mixing potential associated with a propagating steam explosion in an initially separated system is shown to be considerably less than that required to challenge the containment.

- Furthermore, the <u>rate</u> of fuel-coolant mixing is shown to be relatively slow ruling out any potential for direct vessel failure and further emphasizing the absolute need for inertial constraint, (i.e. coherent liquid column above the interaction zone) to collect and focus the steam explosion energy release.

- The required inertial constraint is shown to be absent due to early slug breakup therefore eliminating any significant damage potential to the reactor vessel head.

We therefore continue to conclude that the potential for early containment breach due to a steam explosion occurrence is not physically credible. Considering the fundamental nature of the processes controlling the masses of material interacted, the extensive margin between the required and calculated masses and the level of uncertainties in the physical processes, the probability of grossly exceeding the calculated masses involved is vanishingly small.

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FAUSKE & ASSOCIATES, INC.

November 19, 1984

Mr. Cardis Allen
U.S. Nuclear Regulatory Commission
Mail Stop AR5003
Washington, D.C. 20555

Dear Cardis:

SUBJECT: THE NEED FOR LARGE SCALE FUEL-COOLANT INTERACTION EXPERIMENTS

REFERENCE: Sandia Steam Explosion Research Proposal, October 29, 1984

During the last several years we have been asked to review the need for large scale experiments concerning the steam explosion potential in connection with light water reactor severe accidents. Copies of these reviews are enclosed for your use and information.

We have also reviewed the subject reference material and come to a similar conclusion. The proposed large scale experiments would be of little value in providing direct extrapolation to the LWR system. Rather, as we have indicated in the attached letters, the actual understanding must come from a basic appreciation of the fundamental processes associated in explosive interactions as opposed to an empirical extrapolation of experimental data.

Sincerely yours,

Hans K. Fauske, President
Fauske & Associates, Inc.

HKF:jab
Enclosure

cc: E. L. Fuller, EPRI
R. E. Henry, FAI

Corporate Office: 16W070 West 83rd Street • Burr Ridge, Illinois 60521 • (312) 323-8750

FAUSKE & ASSOCIATES, INC.

May 7, 1982

Dr. David Squarer
Nuclear Safety Analysis Center
3412 Hillview Avenue
Palo Alto, CA 94303

Dear Dave:

During your visit to the FAI Offices last Friday, we discussed the pros and cons of carrying out large-scale (∿ 200 kilograms) steam explosion experiments to resolve open issues related to such events in LWR safety evaluations. As we agreed during the conversation, such experiments would be very time-consuming and expensive. You asked for our evaluation of doing such experiments considering only the technical feasibility of carrying these out and the influence they would have on any open issues relating to steam explosions. The following discussion outlines our combined opinions on the subject.

When considering the need for such experiments to resolve major technical issues, the basic argument for such tests that would attract most people would be the larger scale of the experiments and, thus, less need for extrapolation to the reactor system. However, when evaluating the justifications for doing such tests, one should consider both the scale and the level of extrapolation as required to comprehend issues embedded in LWR safety arguments, and also the past history involved in performing larger scale experiments. Let us consider these arguments separately.

Steam explosions with sufficient energy release to threaten the primary system integrity in LWR systems require between 2000-4000 kilograms of molten-corium if a perfect thermal interaction is hypothesized. Considering that such interactions are at most 10 percent efficient, then the amount of material that is considered would be 20-40 metric tons of debris. With this amount of material, experiments carried out at a level of 200 kilograms would still require an extrapolation of two orders of magnitude to a realistic LWR system scale.* Consequently, the 200 kg scale experiments would be of little value in providing direct extrapolation to the LWR system; hence, the actual understanding must come from a basic appreciation of the fundamental processes associated in explosive interactions as opposed to an empirical extrapolation of experimental data through two orders of magnitude.

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However, the major argument against doing these very large-scale experiments is embedded in the experiments which have been conducted to date. Surveying the literature, one can find experiments that have been done with grams of molten debris, tens of grams, kilogram scale, and tens of kilograms. Generally speaking, the only in-depth analysis carried out by the experimenters is a correlation of explosion efficiency, and generally this is done with different reference efficiencies by different investigators. Specifically, the experimenters have rarely compared their experimental observations to those key physical processes which have been described in the literature and discussed by many experts throughout the world. While the specific configurations and processes occurring within an explosion are the subject of disagreement between various experts, they would all agree that these basic considerations should be applied to all experiments where possible to determine the potential configurations (explosive and non-explosive) which could be achieved in different systems and test series. In addition, many experiments have been carried out with external triggers which have been characterized as being small compared to the energy released by the explosion. However, the basic considerations regarding mixing energies for postulated, coarsely-fragmented systems clearly show that in many of the experimental configurations that the mixing process itself could be totally dominated by this external trigger as opposed to relying upon an internal propagation mechanism to create the explosive interaction. In essence, the experimental observations could be only the result of the external trigger and not the characteristics of a self-propagating explosive interaction.

Considering the large amount of money spent on large-scale experiments in the United States and abroad, it is apalling that so little information is derived from these experiments. This generally arises from the fact that experimenters are only held responsible for carrying out the experiments opposed to applying basic mass, momentum, and energy balances to the overall system and to the scenarios postulated for establishing and initiating such explosive conditions. We find it difficult to justify additional large-scale experiments when there is a wealth of experimental information that has been accumulated and remains unanalyzed in terms of the basic issues involved in the assessment of LWR system responses.

Performing experiments with ten times the mass that has been used in previous studies is wrought with the same difficulties that saddle all large-scale experiments. They are expensive, difficult to instrument, and consider only integral type of behavior, etc. With the range of accident scenarios addressed for both the boiling water and pressurized water reactors, and the various manners in which different investigators envision material pouring into the lower plenum and interacting with the remaining water, the assessments for LWR systems must be based on first principal arguments which address the major physical processes. If such assessments show that a potentially explosive configuration cannot be achieved by orders of magntidue for LWR systems and if these same arguments predict explosive conditions when applied to the experimental configurations that have produced explosive events, then the major thrust of any further investigation must be to analyze those experimental systems to which this basic analysis has not been applied. We see no need to do further large-scale experiments until those basic analyses have been carried out and presented to the technical community. In addition, such experiments should not be considered unless a major question concerning these first principal arguments and their application to LWR systems arises and it is determined that the resolution of the technical issue can be achieved by these large-scale experiments.

We thank you for this opportunity of providing our assessment of such experiments, and should you have any questions regarding our opinions or the manner in which the basic first principal arguments can be applied to experiments performed to date, please feel free to contact us at any time.

Sincerely yours,

Bob

Robert E. Henry, Vice President

Hans

Hans K. Fauske, President
Fauske & Associates, Inc.

REH:HKF/cjg

FAUSKE & ASSOCIATES, INC.

May 6, 1982

Dr. L. S. Tong
U.S. Nuclear Regulatory Commission
Mail Stop 1016, H Street
Washington, D.C. 20555

Dear Long Sun:

I recently visited the Sandia National Laboratory to review the latest experimental results for large scale steam explosions as well as the other support programs which compliment the large scale tests. As you requested in our phone conversation last week, this letter summarizes my evaluation of the experiments they have conducted to date, their relevance to LWR reactor safety issues, their plans for future experiments, and the manner in which the remaining open questions for reactor safety considerations can be resolved.

The most impressive feature of the Sandia experiments is the extensive range examined; between the largest and smallest experiments a range of 400,000 to 1 has been covered. Efficiency measurements of explosive interactions over this range show essentially a constant value, i.e. between 0.1% and 2% when based upon the thermal energy in the melt. (Recent experiments had been interpreted by the Sandia staff as being more energetic than previous tests, but a more in-depth evaluation of the experimental measurements produced similar results to previous experiments. This was also true for the recent tests where multiple explosions were reported.) Consequently, this five orders of magnitude range in experimental conditions provides the most extensive check on scale dependent considerations by any single organization. As such, it demonstrates that efficiency is not strongly effected by the scales tested to date, and since the particular configuration of interest in a large scale steam explosion, and thus in the evaluation of LWR reactor systems, is of the order of centimeter size pre-mixed particles, such large scale test results provide a great deal of confidence for extrapolating the results to reactor systems.

Other pertinent results from the Sandia test program are a confirmation that an elevated ambient pressure inhibits the initiation of large scale explosions. Experiments carried out with explosive triggers demonstrate that triggers can initiate explosions at levels slightly above where natural termination would be expected. However, other experiments also indicate that very strong triggers are required as the ambient pressure is further increased. This is particularly important since many accident sequences considered for LWR systems result in high ambient pressures at the time that molten core material is assumed to fall into the lower plenum of the reactor pressure vessel. In addition, Sandia experiments have also demonstrated that corium-like materials can explode in water, hence there is nothing inherent in the properties of a corium-water system which preclude explosive interactions.

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Experiments carried out in the FITS facility have used both subcooled and saturated water. With large subcoolings, explosions appear to be quite regular and reproducible. However, when the water is saturated, only small events, called triggers by the experimenters, have been observed and no propagating explosion has been detected. This of course does not imply that explosions cannot occur in saturated water since previous experiments carried out at Sandia by Buxton and Benedick have demonstrated explosive interactions for near saturation conditions. However, it does imply that there is a substantial difference in highly subcooled and saturated systems. This difference is important since most LWR accident scenarios result in either saturated or nearly saturated water within the lower plenum. These experimental results, in a facility that repeatedly produces explosions in subcooled water, evidently illustrate that it is more difficult to establish an explosive configuration with saturated water. This is a key point for evaluation of LWR systems and it is also a major indicator to guide future test conditions.

Models representing the major physical processes in explosive interactions all suggest that fine scale fragmentation and intermixing of hot and cold materials must occur during an explosive interaction. Several different approaches have considered configurations where the molten fuel debris is coarsely fragmented prior to the explosion into sizes of order one centimeter in diameter and during the explosion this material becomes finely fragmented to sizes of order one hundred microns and is intimately dispersed through the water. In fact, for the models this fine fragmentation and intermixing takes place in a liquid-liquid (coolant-fuel) configuration. Additional models have been suggested in the literature for representing the energy requirements in such rapid liquid-liquid mixing and in many of the Sandia experiments, sufficient information is available for comparison of such models to the overall energies available. These considerations do not confirm such mixing models but they do provide sufficient information to form a comparison between the available energies and the necessary energies to determine if a violation of the basic postulates occurs. With such comparisons, the experiments have been found to be well within the necessary criteria posed by these mixing models. This is also a very important result since the ability to establish a premixed configuration and the necessary energies for rapidly mixing molten core debris in water in the reactor system are principle characteristics to be evaluated for the reactor system.

Previous experimental results can be summarized as making substantial contributions to the resolution of major questions regarding the potential for reactor pressure vessel failure as a result of an in-vessel steam explosion. These contributions lie in the demonstration that corium can explode, ambient pressure inhibits explosive interactions, saturated water makes establishment of explosive configurations more difficult, and the relatively constant efficiency for explosive interactions over five orders of magnitude range in melt scale.

With this foundation, the major question remaining is what additional experiments, if any, should be performed? The most frequent question posed in this area is whether larger scale experiments should be done, and if so what scale and how should they be carried out?

Scale provides a difficult feature for many people to comprehend since there is always an inherent fear that something basic has been overlooked. However, for large scale steam explosions, the importance of scale can be simply stated in terms of the ability to achieve small mass configurations (coarse pre-mixing) as opposed to questions regarding the integral behavior of very large masses. Specifically, the centimeter scale necessary to consider explosive release from a postulated melt is far less than the tens of kilograms scale that have been studied to date, i.e. substantial coarse fragmentation is required prior to the explosive interaction in these experiments and the only question for larger scale systems is whether similar coarse fragmentation can occur with even larger masses; not whether the explosion is fundamentally different. As a result, I can see no justification for carrying out phenomenological experiments at larger scales to investigate physical processes which are fundamentally governed on a scale of a centimeter to tens of centimeters.

A more important question is what additional experiments are needed to demonstrate the behavior under postulated reactor conditions and resolve any open issues related to this facet of accident analysis? In this context, some specific experiments can be recommended and these can be carried out in the current test facility, i.e. the FITS apparatus. These experiments should investigate 20 kg melt drops (a corium mixture is preferred) in a confined configuration that would tend to prohibit fine scale fragmentation as predicted for the reactor system by analytical tools used in the Zion Probabilistic Safety Study or other published analyses. To perform such experiments, the interaction vessel should have a core barrel simulation as well as a simulation of the lower vessel head and the downcomer annulus region. The specific issue of interest is whether fine scale fragmentation can occur in a confined geometry. These experiments should be done with saturated water and preferably at pressures of 3 and 5 bars. A typical experimental configuration to represent the PWR would be 20 kg of molten material dropped into an interaction vessel with a 12 in. dia. core barrel and an 18 in. dia. pipe simulating the reactor vessel and a pipe cap simulating the lower head. In addition, the lower core support structure could be simulated by a disk welded on the bottom of the core barrel simulation. This disk should have 2 in. dia. holes drilled through to allow free access of molten debris into the lower regions of the RPV.

The principle goal of this experimental test series would be to provide the molten material into water in a comparatively constrained configuration, as opposed to the phenomenological experiments carried out to date in the FITS facility. The tests should be instrumented to measure the resultant energy release, should an explosive interaction occur, as well as to measure the slower overall thermal-hydraulic interactions occurring during the pre-mixing period. These experiments will provide a considerable amount of insight into the effect of the general configuration and the effects resulting from attempting to mix large quantities of high temperature debris in a comparatively small volume.

Other experiments should also be carried out in this facility to represent the specific below core configuration of BWRs. This could be easily accomplished by inserting a fixture with open pipes on a tight square pitch which represent the CRD guide tubes used to support the core in BWR designs.

If these experiments are carried out, the overall dependence of scale will have been properly addressed, the influence of typical reactor configurations will have been scoped, and conclusions can then be made on the potential threat that such events pose to reactor vessel integrity. It is important that these issues be resolved in a technical manner so that realistic assessments of public risk can be formulated for reactor systems. With the previous results of the Sandia program and other experimental programs throughout the world, and the addition of the other FITS experiments described above, I feel confident that these issues can and will be resolved.

Sincerely yours,

Bob

Robert E. Henry, Vice President
Fauske & Associates, Inc.

REH:jab

REVIEW OF STEAM EXPLOSION ISSUE

Responses to Questions 1 and 2 of
D. Ross' Letter of 24 August 1984

BY

T. GINSBERG
BROOKHAVEN NATIONAL LABORATORY
DEPARTMENT OF NUCLEAR ENERGY
UPTON, NY

Submitted to

Steam Explosion Experts Group
Cardis Allen, Executive Secretary

November 1984

Revised January 1985

RESPONSE TO QUESTION 1

WHAT IS THE PROBABILITY OF CONTAINMENT FAILURE
BY A STEAM EXPLOSION?

1. Approach

The approach taken to attack this problem was to, first, trace the postulated steam explosion sequence through in mechanistic terms. An attempt was made to keep the rudimentary analysis as much as possible a "best estimate" or "best guess" exercise. Because of time limitations, only a low pressure PWR case was worked through. Section 2 below presents the mechanistic analysis of the problem.

The probabilistic approach, which is based on insights from the mechanistic analysis, is described in Section 3, where the probability of containment failure by a steam explosion is actually computed. Section 4 presents a summary of the calculational results.

2. Mechanistic Analysis

2.1 Conceptual Approach

With the method adopted here, an attempt is made to trace the development of the accident progression from the time of core meltdown to contact of the melt with water, subsequent mixing, explosive energy release and conversion to mechanical energy. Where possible, a best judgment approach is used in actually performing calculations. In some calculations, where the complexity of the physical processes did not allow for an estimate based upon a best judgment calculation, a conservative assumption would have to be implemented. The objective here was to calculate a best judgment loading on the reactor vessel. It is recognized that the analysis is severely restricted on account of the crude state of mathematical models and the lack of convincing, large scale experimental data.

Figure 1 traces the accident sequence as pictured here. Figure 2 is a schematic diagram of in-vessel structures and dimensions. During the core meltdown process the melted core material relocates in the downward direction. The melt may flow downward to the lower plenum as quickly as it melts, or it may be held up in the core region as a result of flow blockages. In the latter case the melt would develop as a molten pool. The entire pool would then relocate downwards as frozen blockages and crusts remelt. The molten mass would encounter the lower core plate as the first obstacle below the core region, and would have to penetrate it before it could continue on downwards. This penetration would either occur as a result of meltthrough or a mechanical failure. The diametrical scale of the penetration(s) that would develop in the core plate is unknown, and no obvious scale is suggested by the geometry. Following penetration of the core plate, melt would flow through available flow paths, while the penetration(s) would grow with time as a result of melting processes. The melt stream(s) would encounter water either between the diffuser plate and the lower core plate or between the diffuser plate and the core support plate. The melt would likely impact the diffuser plate and spread laterally to some extent before issuing from one or more of the holes in the diffuser plate as gravity driven jets. The diffuser plate would melt locally as the melt flows throught it. The melt stream(s) would continue to flow downwards and through the holes in the core support plate. If an explosion has not already occurred, the melt would continue to flow until it encounters the lower vessel head. At this point the melt stream can accumulate as a pool within the lower head. Steam explosions can occur at any time during the sequence described here. It is likely, however, that the explosion would occur when the melt stream impacts one of the structures it encounters in its path to the vessel head.

Since the melt flow distributions are highly uncertain in the process described above, the analysis which will be presented below attempts to parameterize some of the basic unknown factors (e.g., the penetration diameter at failure of the lower core plate.) Judgment is used to specify the best guess of the value of the parameters and the effect of variations about the best guesses is considered.

The nature of the postulated melt streams is used to make quantitative estimates of the extent of breakup of the melt during the premixing phase of the postulated explosion. Based upon these estimates the quantity of melt mixed with water is arrived at along with an estimate of the volume of water mixed with the fuel. The conversion ratio is arrived at using a combination of the above mixing estimates, consideration of the available experimental data and calculations of maximum thermodynamic yield.

The above best guesses are then combined to yield an estimate of the explosive energy yield. This yield is then compared to estimates of energy required to fail the vessel and to generate missiles.

The "base case" considered below deals with the low-pressure scenario ofa steam explosion in the Indian Point PWR. The discussion focuses on: (i) the extent and conditions of the core melt, (ii) mixing of melt and water, and (iii) the conversion ratio.

2.2 Core Melt Extent and Conditions

Any description of the core meltdown processes in LWR's must be considered speculative. The particular interest in the context of steam explosions is the extent of core melting and accumulation of core melt in the core region prior to the development of flow paths to the lower plenum. Computer codes to predict this portion of the accident progression are in an early state of development and we must rely on previous experience and judgment to establish conclusions pertinent to the current problem. Core melting is accompanied by relocation of the melt under gravity to the lower portions of the fuel assembly where relatively cold structure is encountered and where the molten material is likely to refreeze and remain until decay heating leads to remelting. This type of process was established by much work related to fast reactor accident analysis. It is judged that the initial molten material would relocate in this fashion rather than flow directly to the lower plenum. Thus, molten corium would accumulate in the core region, supported by blockages of solidified material. The blockages would not be long-lived and the molten "pools" would move downward in a succession of melt and resolidify cycles. It is estimated that the temperature of the molten region would lie somewhere between the dissolution temperature of the $UO2/ZrO$ system and the melting temperature of $UO2$. A temperature of 3000K is taken as being representative. This leads to a store energy content of 1.6 MJ/kg of the melt, using the water saturation temperature as the reference temperature.

The mass of molten core inventory which would accumulate in the core region and be supported by resolidified blockage material prior to development of penetrations leading to the lower plenum is highly uncertain. It would depend upon the radial coherency of core melting, the nature of the melt relocation and resolidification processes, the strength of the frozen blockages, etc. The total core mass is 130,000 kg. It is my understanding that newly available analyses of core melt which incorporate in-vessel circulation calculations suggest that the melting patterns may be more coherent than would be indicated by the radial power profile. It is here assumed that "significant quantities" of molten corium are located in the core region, supported by frozen blockages and that this mass becomes available for release under gravity to the lower plenum once flow paths become available. Actually, as will be seen later, the results presented here are relatively insensitive to the assumed molten pool mass, since it is felt that any explosion would involve the mass of melt suspended at any instant, rather than by the total inventory of available melt. Only if the suspended mass could physically approach the assumed mass of the molten pool would this assumption become important to the analysis.

2.3 Pour Penetration Development

It is very likely that we will never know the mode of contact of molten corium with the available water supply in the lower plenum. It is envisioned that the molten material, as discussed above, would be surrounded and supported by masses of resolidified core material. The entire melt mass would propagate downwards under the influence of gravity as blockages weaken due to internal heating. The melt mass would eventually encounter the lower core plate.

It is assumed that the water level can exist anywhere beneath the lower core plate. Because of the relatively small diameter of the holes in the lower core plate, it is assumed that the melt would not directly flow through them. Rather, it is assumed that the plate would be either melted through or would be so weakened by high temperature that it would fail "locally." Upon failure, it is assumed that the melt will begin to pour through the resulting penetration. The penetration size would increase with time during the pour as a result of ablation processes. It is conceivable that more than one penetration would develop simultaneously. In order to accommodate the major uncertainties in determination of the initial penetration(s) or "pour diameter," this quantity will be considered below in a parametric manner.

2.4 Melt Breakup and Melt-Water Mixing

2.4.1 Approach

It is assumed that the explosive phase of the interaction is preceeded by a well-defined pre-mixing phase, as is now suggested by much of the literature. The pre-mixing phase would be terminated when a trigger of sufficient magnitude initiates the explosion. The corium is envisioned as being delivered to the lower plenum in a "pour mode" of transport. One or more "streams" of melt are envisioned as becoming available at some time for delivery of the corium. A "stream" is defined here as a roughly cylindrical jet of some diameter. The available data suggest that an explosion can be triggered at any time following arrival of the leading edge of stream to the water surface. Surface triggered explosions have been observed as well as explosions triggered at arrival of the melt to the bottom of the experimental test vessel. It is assumed here that the most likely time for triggering an explosion would be upon arrival of the leading edge of the melt at a structural surface within the lower plenum. The maximum quantity of melt which could participate in the explosion would be that mass which is suspended within the lower plenum pool of water at that instant in time. It is noted that this is quite a restrictive assumption which limits the potential molten mass involvement to a fraction of the melt available in the core region. Since the explosion is postulated to occur rather early in the pour, no mass of corium has yet had a chance to collect in the lower vessel head and, therefore, to participate in the resulting explosion.

The explosion which is triggered as described above may, or may not fail the reactor vessel. If it does not fail the vessel the possibility exists for subsequent encounters of corium and water and, hence, for additional explosions. It is here assumed that the initial explosion would disperse both water and melt and, if vessel failure does not occur, that the first explosion would be the most energetic. A later, more energetic explosion, with mixing driven by an early interaction, cannot, however, be ruled out at this time.

With the above discussion in mind, the analysis proceeds by postulating a failure diameter in the lower core plate, which also serves to define the diameter of the postulated melt pour stream. The melt is assumed to flow downward under gravity where it would encounter first the diffuser plate, then the core support plate and then, finally the lower vessel head. An attempt is made to identify the potential stream breakup mechanisms imposed either by the structure or by hydrodynamic mechanisms. The scale of the "coarsely" fragmented melt is estimated for each of the mechanisms.

2.4.2 Molten Stream Breakup Considerations

Assume that the melt would pour through a penetration of diameter D in the lower core plate. The melt would then encounter the diffuser plate, which contains holes of 20 cm diameter. If D is less than 20 cm the stream could impact a solid region of the diffuser plate and spread laterally and flow through several of the surrounding holes. The resulting streams would be of diameter less than 20 cm. The resulting streams would then continue downwards where they would impact the support plate, and perhaps be further dispersed.

the streams would then reach the vessel head. If the penetration in the lower core plate is greater than 20 cm then the melt would flow to the diffuser plate where it would likely be split into several streams of 20 cm diameter. These streams would then reach the support plate and flow through the 20 cm holes in similar size pour streams to the vessel head. Thus, it appears as if the geometry imposes two jet stream diameter scales: (i) D - the assumed diameter of the penetration in the lower core plate and (ii) the holes in the diffuser and core support plate of diameter 20 cm.

It has been postulated that the explosion would take place upon contact of the melt stream with one of the structural surfaces within the lower plenum. If the molten pool height within the core pool is of the order 1 meter, then the stream velocity at the core plate penetration would be approximately 2 m/s. Since the distance between the core plate and the bottom of the vessel is about 3 m, then the transport time of the leading edge of the melt to the bottom of the vessel is of the order 1 second. Within this time scale the structures in contact with the melt would experience negligible additional melting (steel wall melting rates of less than 1 cm/s are expected.) The above diameter scales, therefore, would not vary greatly during the time period prior to the explosion.

Based upon the above assumptions and arguments, the maximum inventory of melt entering a possible steam explosion can be computed. This would be given by the total mass of melt contained in a cylindrical jet of diameter D and height H, where H corresponds to the distance between the lower core plate and the vessel head. This "suspended melt mass" is given in Fig. 3. Note that the core diameter is 3.26 m. Since there is no obvious choice for a "realistic" D, it is recognized that Fig. 3 does not impose any limits on the quantity of melt which can participate in the interaction. The figure does, however, give a feeling for the masses involved.

Since the "pour" is envisioned as a gravity-driven jet, it would appear that the initial melt breakup mechanism would be governed by principles involving jet breakup, as has been proposed by Theofanous. An extensive literature is available pertinent to breakup of liquid jets in gaseous environments. There is very little data relevant to liquid-liquid jets and almost negligible data relevant to liquid-liquid jets with interfacial film boiling. It will be assumed here that the vapor film which would envelop a corium jet acts essentially as an infinite medium of vapor and that the available breakup literature is applicable. It is noted, however, that this assumption must be evaluated. Figure 4 shows the qualitative behavior of the breakup characteristics of liquid jets, where L/D represents the dimensionless breakup length of the jet. Several regimes are shown, including the laminar, transition, turbulent and atomization breakup regimes. The breakup lengths are expressed as functions of the jet Reynolds and Weber numbers, as are the available criteria for regime boundaries. The Reynolds number is in the range 10^{-5}-10^{-6} while the Weber number is in the range 10^{-3}-10^{-4} for conditions of interest here. This puts the jet breakup mechanism in the turbulent regime of Fig. 4. The mechanism of jet breakup in this regime is not characterized by the Rayleigh mechanism of the first regime of Fig. 4, which produces droplets of the order twice the jet diameter. Here, droplets are produced as a result of stripping from the jet surface and the resulting droplet sizes are much smaller than the jet diameter. The details of the process are not well understood.

For this regime, three available correlations are presented in Fig. 5 for the liquid jets of corium of interest here. There is considerable discrepancy between the three predictions, with the Lienhart prediction showing the largest difference in behavior. The data upon which this correlation is based, however, were obtained from jets created from a sharp-edged orifice, a condition significantly different from the other two systems. It is felt that the Miesse and Phinney correlations are appropriate to the jet stability conditions of interest here. Both of these correlations predict that in the range of parameters of interest here, the molten jets of corium would breakup at L/D > 10.

Figure 2 shows the distances between the structures in the lower plenum region. The distance between the diffuser plate and the core support plate at its lowest position is 2.4 m. Thus, if a pour of diameter greater than 20 cm is broken up at the diffuser plate into a number of 20 cm streams, then there is just enough length available for them to begin to break up further due to jet instability in the turbulent jet regime. It appears unlikely that significant hydrodynamic breakup will occur and that jets of diameter 20 cm would reach the vessel head largely intact. In addition, supporting this argument, it is noted that the melt stream first traverses the region between the diffuser plate and the core support plate, then it traverses the remaining distance to the vessel head. For a 20 cm diameter stream, each of these distances correspond only to 5 diameters. It, therefore, appears highly unlikely that significant jet breakup will occur as the melt stream(s) flow through the lower plenum.

Jet breakup is envisioned as the first step in hydrodynamic breakup of the melt mass as it flows through the lower plenum. If jet breakup cannot occur, then continued hydrodynamic breakup due, for example, to drop breakup under inertial and surface tension forces (Weber breakup) also would not occur.

The picture which emerges from the above considerations is that of the penetration of multiple streams of melt into the pool of coolant as jets of diametrical scale 20 cm. This configuration does not at all parallel the conditions of the FITS tests as thus far conducted. In these tests the melt was not constrained to a jet configuration, since the melt entered the pool already broken up to a scale less than 20 cm. From the standpoint of melt breakup, it is felt that those experiments are representative of what might be the leading edge of material during a large scale pour and are not, therefore, representative of the behavior of a sustained jet entry into a pool of coolant.

An additional possible mode of breakup of the molten streams of corium which has been suggested is due to Taylor instabilities at the leading edge of the stream as it penetrates the pool of coolant. Using Taylor's linear theory, one computes a critical Taylor wavelength of 1.6 cm and an instability growth time scale of approximately 26 msec. Since the transport time of the leading edge through the pool is about 1 second, there appears to be sufficient time for the growth of the instability and, perhaps, for detachment of droplets of diameter less than 1.6 cm into the flow stream. Corradini's model for entrainment due to Taylor instability was then used to compute the breakup of the melt stream. The relationship was derived based upon observations of experiments with water and air. Here it is assumed to apply to the case of corium flowing into a pool of water.The volumetric entrainment rate is given by

$$Q_L = 4.65\, A \sqrt{g \lambda_{CR}}$$

where A is taken as the jet cross-sectional area, g is the acce leration of gravity and λ_{CR} is the critical wavelength. The entrained volume of liquid, given in terms of the equivalent to diameter of the jet is then given by

$$\left(\frac{L}{D}\right)_E = 4.65 \sqrt{g \lambda_{CR}}\ \tau / D$$

where τ is the leading edge transport time. For the 20 cm jet, a mass of 232 kg, equivalent to 4.5 diameters, could be broken up by Taylor instability, according to application of the above theory, in the region between the diffuser plate and the lower support plate, and a similar mass between the support plate and the vessel head. The relationship is being applied to the leading edge of a jet, for which it was not intended, and the validity of the application is uncertain. Transient jet development observations are needed for jet diameters significantly larger than the Taylor wavelength for the system. The detached droplet diameters would likely be less than the Taylor wavelength of 1.6 cm, and would tend to break up further as they adjust to local steam-melt flow conditions.

The analysis presented above suggests two pictures of the behavior of the molten streams of corium as they pass through the lower plenum. These are characterized schematically in Fig. 6. Figure 6(a) presents the scenario assuming no Taylor breakup and no breakup due to jet instability. A stream of diameter D penetrates the lower core plate and drops to the diffuser plate with no breakup. The diffuser plate (assuming no explosion at this point) breaks the initial stream up into a number of individual streams of diameter 20 cm. These penetrate without breakup to the support plate and from there the corium penetrates without breakup to the vessel head. The second case, shown in Fig. 6(b), considers Taylor breakup in the regions below the diffuser plate. One additional configuration, shown in Fig. 6(c), could occur if the trigger were delayed for several seconds beyond arrival of the melt to the base of the vessel. During this delay time, the diffuser and lower support plates would melt through around the contact area with the melt by ablation due to the pouring streams. The result of this meltout would be elimination of the breakup into the 20 cm streams. The characteristic diametrical scale of the pour stream would then be of the order of the assumed diameter of the penetration in the lower core plate, D. In addition, a molten pool of some depth may have already accumulated within the lower vessel head. These three configurations are considered as possible fuel configurations at the instant of the postulated explosion.

The major difference in three configurations shown in Fig. 6 is the "courseness" of the "premixture" at the instant of the postulated trigger. It is believed that the configuration of Fig. 6(a) is the most likely of those shown. The 20 cm scale of the premixture of Fig. 6(a) presents a great deal of difficulty. On the one hand we know from experiments at SNL, BNL and elsewhere that explosions can occur in situations with initially well-separated melt and coolant. The efficiency and extent of melt involvement of such explosions are, however, not documented. On the other hand there is some feeling that explosions resulting from "extremely coarse" mixtures are either very inefficient or are not physically realizable. Bankoff claims that "...it is questionable whether fragmentation of the 0.1-m particle into 10-μm particles can be achieved on explosive time scales..." Theofanous also suggests a "...maximum premixture dimension..." of approximately 10 cm. Fauske and Henry speak of a "...necessary premixing scale of the order of 1 cm..." The energetics work of Cho et al., can also be interpreted to imply that it would be difficult to mix water and fuel from scales of 0.1-m down to sizes necessary for energetically efficient explosions. Experimental evidence does not help us here, since we do not have data of the scale required to draw relevant conclusions. Data suggest that at least 1-cm objects are explosive.

Considering that all of the above arguments are of the "order-of-magnitude" variety, the 20-cm scale falls right at the edge of the 10-cm scale addressed by several of the above investigators. The essence of the practical problem here is to determine what fraction of the jet can be involved in the explosion. I have seen neither data nor arguments which would help with making this judgement. The only jet explosion data I have seen were done by Witte, with jets of diameter 1/32-inch.

The configuration of Fig. 6(b) pictures a more highly broken up melt stream than the others with the possibility of the existence of centimeter-scale melt droplets. If this is indeed the case, then the configuration would be greatly affected by the potentially high steam generation rates which the existence of such droplets imply. Such arguments related to the hydrodynamic limits on melt-water mixing have been proposed previously. If one presupposes the a pour of centimeter-scale melt droplets, then two limits suggest themselves. In one case it is assumed that the coolant remains well-coupled to the droplets and that each droplet transfers heat via radiation and film boiling to the surrounding water. If one postulates that the droplets remain confined radially to a diameter equal to the diameter of the stream which they were fragmented, then upon submergence of some relatively small quantity of melt, sufficient steam is produced such that the velocity would exceed the fall velocity of the droplets. The conclusion is reached, therefore, that additional droplets which would be on their way downwards, would be blown away. Figure 7 was developed using this principle, and demonstrates that for a 20-cm jet fragmenting into 1-cm droplets, that less than 100 kg of mass would be submerged before the "fuel flooding limit" would be reached. If, however, the fragmentation process were postulated to continue, what would happen to the droplets that continued to be formed? Would they be blown out of the pool, or to another location within the pool where they could participate in the explosion? Thus, this argument, while intuitively appealing, does not lead to what I feel is a substantive limit on the availability of melt at the time of the explosion.

The second limiting condition which has been proposed is based upon the observation that large steam velocities resulting from droplet-coolant heat transfer would "fluidize" the water prior to the fuel droplets. Corradini has shown that this coolant fluidization mechanism is even more restrictive than the fuel fluidization mechanism in limiting the mixing of fuel and water. The implication here is that, given an efficient melt breakup mechanism, only a fraction of the resulting droplets would be surrounded by sufficient coolant to lead to an efficient explosion. I believe that this argument is reasonable, but have not performed the calculation for the scenario discussed above.

Of the scenarios presented in Fig. 6, the one which I will pursue here is the first, since I think that this is the most likely one.

2.5 Specification of Molten Mass Invlovement in Explosion

In Section 2.4 arguments were presented which support the molten jet configuration shown in Fig. 6(a) as being the most likely one to exist at the instant of triggering of the explosion. While the upper limit on the mass involvement in the postulated explosion is determined by the molten mass suspended in the coolant at the instant of the trigger, the distribution of coolant also has a strong bearing on the mass involved.

It is generally accepted that liquid-liquid contact is a necessary condition for a steam explosion following the pre-mixing stage during which the melt is vapor-film blanketed. It follows, therefore, that the melt surface must be immersed in a continuous liquid environment. As the melt streams pour into the coolant in the lower plenum, it is likely that the water will tend to be dispersed away from the melt due to the large vapor generation rates. It is assumed, nonetheless, that water enters the region between the jets as their leading edges penetrate the water, and that the steam produced flows up axially in the same region. It is expected that the void fraction in this region will increase monotonically as a function of distance up the axis of the jets. At some position the void fraction will exceed a value beyond which it becomes unreasonable to assume the presence of a continuous liquid environment. Only that fraction of melt below this position would, therefore, be able to participate in the interaction upon triggering. A void fraction of 0.9 is postulated as being the cutoff of continuous liquid.

It is assumed that each jet is surrounded by a region of coolant, as shown in Fig. 8. The melt transfers energy by radiation to the saturated liquid and the vapor flux increases with z as

$$j_g = \frac{h_r (T_c - T_{SAT}) \pi d}{A \rho_g h_{fg}} z$$

where h_r is the radiative heat transfer coefficient, T_C the corium temperature, A is the jet area, ρ_g the steam density and hfg the heat of vaporization. A steady-state, one-dimensional, churn-turbulent flow regime is postulated, in which case the superficial velocity is related to the void fraction through

$$j_g = u_\infty \frac{\alpha}{1-\alpha}$$

where u_∞ is the bubble rise velocity. For the conditions defined previously, one finds that a void fraction of 0.9 is reached after a vaporization length of 0.14 m. If a region of 0.14 m is postulated to exist both above the diffuser plate and above the core support plate, then of the two meters between the diffuser plate and vessel head, only 0.28 m of the length would have a continuous liquid region adjacent to it. Only these regions would be able to sustain liquid-liquid contact during the explosion.

Of the fraction of jet length which can participate based on the above criterion, it remains to specify the fraction of the diametrical area of the jet which can participate in the explosion. The difficulties involved were discussed in the previous section. In the absence of convincing counterarguments I assume that all of the 20-cm jets are involved in the explosion, while any streams larger in diameter than 20 cm are involved in the explosion to a depth of 10 cm. Only the outer 10 cm of a 0.5-m jet would, therefore be involved in the energetics of an explosion. This assumption has a stong impact on scenarios such as that shown in Fig. 6(c), in which the stream is quite large in diameter and a large inventory of melt is suspended.

2.6 Specification of Conversion Ratio

It remains to specify the conversion ratio of the mass of fuel which is postulated to interact.

The conversion ratio characteristic of the FITS tests have been in the range 0-5%, with much of the data in the range 1-2%. Corradini indicates that the explosions in these tests have taken place under "coolant-rich" conditions. His calculations have indicated that the conversion ratios for the melt-coolant conditions in the experiments have been on the order of 25% of those characteristic of the "maximum thermodynamic yield" under these conditions.

The initial conditions of the explosion as envisioned in Fig. 6(a) are certainly significantly different from those of the FITS tests. The postulated configuration of Fig. 6(a) is certainly a "coarser" premixture than encountered in the FITS experiments. I would expect that the conversion ratio characteristic of the configuration of Fig. 6(a) would be lower than those of the FITS test. Due to the lack of appropriate data, however, it will be assumed that the conversion ratio would also be 25% of that computed on the basis of maximum thermodynamic yield for the melt-coolant conditions postulated here.

Fig. 9 presents Corradini's calculations of conversion ratio based on thermodynamic calculations of isentropic expansion of either the coolant or fuel-coolant mixture down to 1 atmosphere pressure, where the initial fuel temperature is assumed 2700K. If it is assumed that only the coolant between the jets are involved in the interaction, then the ratio of coolant to melt mass is 0.16, which gives a conversion ratio of 0.2 on the coolant curve of Fig. 9. A conversion ratio of 0.05 was initially chosen to be used here. It was subsequently pointed out that the expansion of the fuel-coolant mixture should be taken to the available vessel volume, rather than to a pressure of 1 atm. This gives a conversion ratio approximately half that shown in Fig. 9. A conversion ratio of 0.03 was chosen to be used here.

2.7 Calculation of Explosion Energetics

The assumptions and calculations pertaining to melt stored energy density, conversion ratio and mass of melt involved in the explosion are combined into a calculation of the explosive energy release. The energy release is given by

$$E = e\, m_f\, \eta$$

where e is the melt stored energy, m_f is the melt mass participating in the interaction, and ε is the conversion ratio. The melt mass is the sum of the mass contained in the region beneath the core plate, m_1, and the diffuser plate and the mass contained beneath the diffuser plate, m_2. The quantity m_1 is given by

$$m_1 = \frac{\pi}{4}\left[D^2 - (D-2t)^2\right] h_1 \rho_\ell$$

where t is the depth of jet consumed in the explosion, ρ_ℓ is the corium density, and h_1 is the distance between the core and diffuser plates. It is assumed that the pour jet of diameter D breaks up into N jets of diameter d such that the total flow area of the smaller jets equals the pour penetration area. The mass contained in the N jets which enters the interaction is given by

$$m_2 = N \frac{\pi}{4} d^2 h$$

where h is the axial distance along the jets which enters into the explosion and can take on any value up to the distance between the diffuser plate and the vessel head.

With the above assumptions, the energy release is given by

$$E = \frac{\pi}{4} e \eta \rho_\ell \left[\left\{D^2 - (D-2t)^2\right\} h_1 + N d^2 h\right]$$

where t is the depth into the jet which is involved in the interaction.

For a pour of diameter D uniformly filling the entire pool height and with the entire mass entering the interaction, the energy release would be

$$E = \frac{\pi}{4} D^2 h \rho_\ell e \eta$$

Figure 10 presents the energy yield and melt mass involved in the interaction as a function of the axial distance along the region beneath the diffuser plate. The maximum depth physically plausible is 2.3 m. Results are shown for core plate pour diameters ranging from 0.5 to 2.0 meters. If the explosion were to occur as the leading edge of the pour jet of diameter D reached the lower core plate, then the energy would by given at z=0 in Fig. 10. If the jets beneath the core plate only interact where calculations indicate liquid-liquid contact is possible during the explosive phase of the interaction, then one would select the energy yield at z=0.3 m. The solid lines represent calculations using a "mixing depth" of 10 cm, while the dashed lines represent calculations which assume all the suspended mass is involved in the explosion.

2.8 Energy Deposition Phase

A major assumption in the above analysis is that the explosion is triggered upon contact of the leading edge of the melt with a solid surface. This occurs within approximately 1 second following initiation of the pour into the lower plenum. As a consequence most of the melt is still retained in the core region. It appears likely, therefore, that the fuel would serve as the slug material and eventually transmit its kinetic energy to the upper head.

It is assumed here that all of the explosion energy released would be available for structural damage. In actuality, there is reason to believe that as the fuel layer is accelerated upwards it will lose energy as a result of dissipation along structure. Even more important, however, the slug itself may break up as a result of encounter with structure and, in addition, as a result of Taylor instability at the leading edge of the accelerating molten fuel slug. An incoherent slug impact, therefore is expected with the upper head. The above factors were, however, were not taken into account.

2.9 "Best Guess" of Explosion Energetics

The results of Fig. 10 indicate a strong dependence on the pour diameter, as has been suggested in many other places. We will likely never have an estimate with any high degree of certainty of this parameter. My feelng is that the lower core support plate will fail due to a meltthrough condition as the melt mass held up in the core approaches the plate. I envision a local meltthrough of a diametrical scale of tens of centimeters. During the pour up to the time of the postulated explosion at approximately 1 second after pour initiation little ablation of the initial pour penetration would have time to take place. My "best guess" would be that the pour diameter would be approximately 0.5 meter. At the outside I would say 1 meter. In addition, I would expect that much of the suspended melt mass would be water-starved since the water would be pushed out of the way by the steam generated as the melt progresses into the lower plenum. Using the criterion of possible liquid-liquid contact, as discussed above, a mass of 1250 kg is predicted to interact with an energy yield of approximately 60 MJ. If the entire melt mass in the 0.5-m diameter jet interacts, this would involve approximately 3700 kg of melt with a yield of 180 MJ. At what I consider the "outside", with a jet of diameter 1 meter the maximum yield according to these calculations would be 780 MJ. Based upon the above, my "best guess" is that the explosive yield would be in the range 60-80 MJ, with the interaction involving between 1250-3700 kg of melt at temperature 3000K. The conversion efficiency is assumed to be 0.03 and the melt energy density is assumed to be 1.6 MJ/kg. While I believe that even this estimate is on the conservative side, several factors are so difficult to rule out completely that I cannot say that this estimate is bounding in any way. It is simply my best judgement, involving some conservative assumptions and some assumptions which appear to be "realistic" but which could throw the result the other way if my judgment of "realistic" is wrong.

It has been estimated that a 1000 MJ yield would fail the lower vessel head and that 3000 MJ would fail the upper head. The "best guess" yields predicted here would be one order of magnitude lower than required to fail the upper head even if all of the energy were applied to the upper head. The yield would also be considerably below that required to fail the lower head.

3. Steam Explosion Containment Failure Probability

3.1 Approach

Based upon study of the available literature and supported by mechanistic arguments such as presented in Section 2, it is my judgment that containment failure by a steam explosion is "highly unlikely." I can think of, and have read of, no single argument, which would convince me that the failure probability is zero. I have also come across no rigorously defensible method to predict the containment failure probability. Since the request has been made to supply the NRC with a quantitative representation of the failure probability a great deal of subjective judgment is required to express the likelihood of containment failure by a steam explosion.

The method that I adopted here, used by several others, is what Berman calls "Category 2- Propagating Engineering Judgement." Using this method, the sequence is divided into several stages. A probability is defined for each stage and a judgement of the probability is made on the basis of physical and intuitive arguments. Assuming that each stage is independentof the others, the probabilities are multiplied to yield an estimate of the probability of the sequence outcome, in this case the probability of containment failure. The numerical value of failure probability will depend, among other things, on the number of stages and the individual probabilities. I would like to address myself briefly to the individual probabilities.

The value assigned to the individual probabilities will depend on the individual's confidence of the outcome of a particular stage. On a scale of probability of zero to one, some people will choose to assign probabilities in units of tenths, while others will feel comfortable working on a logarithmic scale and assigning to individual processes probabilities of .01, .001, etc. Whether one chooses tenths or the smaller "logarithmic scale" probabilities may simply depend on a person's working familiarity with extremely small probability events. The possible difference in results is obvious.

Bearing in mind all of the above qualifications, I nevertheless use this method to come up with my estimate of containment failure probability. The analysis which follows in the next section is based on the accident sequence as postulated in Section 2.1.

3.2 Stages of the Steam Explosion and Probabilities

The sequence of stages considered here are shown in Fig. 1 along with the symbol for the probability associated with each stage.

The first stage is defined as including the core meltdown process, the development of path(s) from the core region to the lower plenum region and the initiation of the "pour" into the lower plenum. The major issue here is whether during the meltdown process the melt will trickle down to the lower plenum, or whether the melt will refreeze as it proceeds downwards, forming a solid crust capable of supporting additional melt over it as the core above it liquifies. It is my judgement that the melt will refreeze as it relocates to colder regions of the core and will, therefore, not be able to trickle slowly to the lower plenum. The major question in my mind is the stability of the crusts under continued decay heating, and their ability to support large quantities of melt. Let

P_H = the probability that melt can be held up in the core region of sufficient melt mass to possibly lead to a significant steam explosion loading.

This probability is assigned a value of 0.75.

The next stage in the process involves the flow of the melt into the lower plenum, breakup of the stream and mixing with the available water. Figure 11 presents a mapping of conversion ratio and fraction of core participating in the explosion with the computed energy yield. I believe that it is virtually impossible to coherently involve tens-of-thousands of kilograms of melt that would be necessary to fail containment if the conversion ratio turned out to be in the low range of a few percent. This is based upon the analysis presented in Section 2. A potentially more probable scenario would involve quantities of the order 10,000 kg with conversion ratios in excess of 0.10. I, therefore, let

P_m = the probability that the melt-water mixing would lead to melt masses of significantly larger than 10,000 kg suspended in the water upon triggering, coarsely fragmented to scales less than 10 cm and well-mixed with water,

P_c = the probability that the conversion ratio, given the melt distribution, of the mass which enters the interaction is greater than 0.10.

Based upon the arguments presented in Section 2, I would expect the quantity of melt in intimate contact with water to be limited by (i) the diameter of the pour, (ii) the extremely coarse mixture due to the lack of jet breakup, (iii) the dispersal of water away from the melt location. Based upon these factors I assign $P_m = 0.05$. Since the melt is expected to exist as jets of 20 cm diameter, since it is possible that fine fragmentation of such a mass on the time scale of the explosion may not be feasible and since data supporting conversion ratios in excess of 0.05 are rare, I selected $P_c = 0.1$.

Based upon evidence from FITS test and tests in the U.K., I assumed that corium does explode in water, and that a trigger of sufficient magnitude would be generated, at the latest, when the leading edge of the melt reached the base of the vessel. A selected $P_t = 1$.

The final stages of the sequence involve the slug dynamics, impact of the slug with the upper head, possible lower head failure, missile generation and possible containment failure. I believe that there are several mitigative mechanisms during this portion of the explosion sequence. These include slug dissipation and possible breakup upon encountering structure during its acceleration through the upper plenum. I have not done any mechanistic analysis in this area, however, and I will not, therefore, give any credit for these potential mechanisms.

The subjective probabilities defined above are multiplied to give a subjective probability of containment failure of 0.0038. This represents the result of combining a collection of "best guesses" of the individual probabilities. There is, of course, an uncertainty connected to this quantity, which is even more difficult to define and to arrive at an estimate of than the probability estimate itself.

4. Summary of Quantitative Results

4.1 Mechanistic Calculations

The result of a "best guess" mechanistic analysis of the steam explosion sequence of events is that the explosive yield would be in the range 60-180 MJ. The interaction would involve between 1250-3700 kg of melt at a temperature of 3000K. The conversion efficiency is assumed to be 0.03 and the melt energy density is assumed to be 1.6 MJ/kg.

4.2 Probabilistic Estimates

A subjective estimate of the probability of containment failure is arrived at by dividing the explosion sequence into several stages. A subjective probability is defined for each stage, and a judgment of the probability is made on the basis of physical and intuitive arguments. Assuming independence, the probabilities are multiplied to yield an estimate of the containment failure by a steam explosion. A subjective probability of 0.0038 is computed as the "best guess" for the low-pressure PWR case considered here. As an upper limit I would increase this by an order of magnitude to 0.038 to account for the many uncertainties that I perceive in arriving at such an estimate.

RESPONSE TO QUESTION 2

A REVIEW OF "AN UNCERTAINTY STUDY OF PWR STEAM EXPLOSIONS"

1. Approach

The authors identify two major goals of their study: (i) to provide an estimate of uncertainty in the prediction of the conditional probability of containment failure and (ii) to identify, using sensitivity calculations, of the major contributors to the uncertainty. The Monte Carlo technique is used, together with various techniques for establishing the ranges of the various uncertain parameters and the probability functions for them, to compute the probability of containment failure and the associated uncertainties. The review which follows is an evaluation of the report in the light of the stated objectives of the work.

Section 2 below is a discussion of the Monte Carlo technique and its use by the authors. The parameter estimation methods used in the report are discussed in Section 3. Recommendations are made in Section 4.

2. Monte Carlo Method and Uncertainty Analysis

The authors compute the steam explosion energy deposited in the upper head using an algebraic expression of the form

$$E = e\, m_f\, \eta\, f$$

where m_f is the mass of melt mixed, e is the stored energy in the melt, η is the conversion ratio and f is a quantity which characterizes several processes involving the moving slug, impact with the upper head and impact of a missile with the containment wall. Each of these quantities involve a great deal of uncertainty because of a combination of a lack of experimental data and analytical tools and difficulty in scaling existing data to prototypic conditions. Containment failure can be defined as occurring if the energy applied to the vessel head is sufficient to fail the head, to accelerate it, and cause it to reach the containment wall with sufficient energy so as to penetrate it.

The basic problem facing anyone who is trying to predict the probability of containment failure is: Given the probabilities of all of the quantities on the right-hand side of the above equation, determine the probability that the computed energy would exceed the failure limit, posed in terms of energy, of the containment building.

If one has estimates of the probability density functions for each of the variables on the right-side of the above equation, then the Monte-Carlo technique provides one method of computing the probability density function of the product of the variables. The authors' choice of the technique, therefore, is a reasonable one. In the context of the current problem, the Monte-Carlo technique provides an estimate of the probability density function of the vessel head kinetic energy as it hits the containment dome. The usefulness of this computed function depends entirely on the validity of the input probability distributions of the individual variables. The power of the method, however, is that the computed distribution function contains all the statistics about the vessel head kinetic energy at impact that one would want. One can easily obtain, for example:

(i) the probability that the kinetic energy would exceed a given containment failure criterion,

(ii) the uncertainty in the probability that the kinetic energy would exced the criterion.

No other calculations are necessary in order to establish the uncertainty in the containment failure probability. What is needed, however, are estimates of the probability functions for the individual variables. If one can establish "best estimate" distributions, then one can come up with a "best estimate" containment failure probability and uncertainty. If one is not sure of the individual distributions then one can use the Monte-Carlo technique to test the sensitivity of the computed impact kinetic energy probability function to variations in the input distribution functions.

The authors have apparently chosen their "full width" probability distributions for the individual variables (conversion ratio, fraction core molten, pour diameter, etc.) based on an evaluation of the steam explosion literature. They chose the range of the variables and chose a flat distribution for all the parameters (p. 34 of report). This is apparently the authors "best estimate" or "best guess" of the distributions as of the date of the report. They then took these distributions and used the Monte-Carlo method to sample the distributions and to compute the number of failures per 10,000 trials. This then gave them an estimate of the containment failure probability. This is presented as the authors' Case 1 in Table IV on p. 35. The computed failure probability ($V>90$ m/s) is 0.03. The authors, however did not present the probability distribution for the head kinetic energy (actually velocity in the Table). The uncertainty in the computed containment failure probability which should have been extracted from the head energy probability distribution was not presented and not discussed.

The authors conclude that the uncertainty in containment failure probability is such that the "...range of probability of containment failure" (for Zion PWR's) "...span the range of probability from 0 to 1." This conclusion was arrived at by using the Monte-Carlo technique in conjunction with selected portions of the "full-width" probability distributions. It should come as no suprise that if one selects extreme values of the individual variables for the calculations that one will compute extreme values of the containment failure probability (this follows in this case since the "full-width" distributions cover quite broad ranges of parameters.)

Thus, the conclusion that the failure probability lies anywhere between 0-1 is not too suprising, given the selection of probability functions chosen. What is missing here, however, is some judgement on the part of the authors as to what the most likely distributions would be and, therefore, what values of failure probability between 0-1 are most likely.

I believe that the Monte-Carlo approach is reasonable and can be used effectively to estimate the containment failure probability and its uncertainty. It should, however, be used as a best estimate tool using the best available information to generate best estimate probability functions for the various parameters. These can be updated with time as better information becomes available. I believe that the authors should have used it in this fashion.

3. Parameter and Probability Distribution Estimates

The estimation of parameter ranges and probability distributions is, to a large, extent subjective, given the current status of the steam explosion literature. The authors adopt the approach whereby they choose the upper and lower bounds of the individual distributions based upon either physical limitations of geometry (pour diameter and length) or based upon the available data base (conversion ratio). These choices are, in general, reasonable, considering all of the uncertainty. The authors, however, then assign uniform probability distributions across the entire range of variable space. This procedure bypasses judgemental calls which may be based upon physical arguments which may be reasonable, but which cannot be confirmed by experiment.

An example of the above lies in the ranges of poured masses, taken as a function of the pour diameter and pour length. The authors recognize that arguments can be made which would limit the mixing of melt and coolant and discuss the available models. The large discrepancy between the models is pointed out. They also point out that the available data base is insufficient to judge the validity of any of the models. Thus, they conclude, "...evidence does not exist to allow an upper limit to be imposed on the mass of melt mixed that is less than all the available melt." Of the models the authors quote, the most conservative predicts 20,000 kg of melt mixed. The available melt if the entire core melted would be about 100,000 kg. Some bias in the distribution towards the lower end of the distribution would, I believe, be appropriate as a judgement call.

My general comment here, then, is that some degree of judgement, beyond that which is presented in the report would render the method used by the authors more useful in establishing a more accurate representation of the current state of affairs with respect to the probability of containment failure and its uncertainty.

4. Recommendation

If the authors believe that the range of parameters of Case 1 and its associated probability functions represent the current state of steam explosion knowledge, then they should use this as the basis for a "best estimate" calculation of containment failure probability. They could then present the complete probability function for the missile velocity and present the uncertainty in the failure probability as computed from the distribution function. The method can then be updated as information is developed in the future.

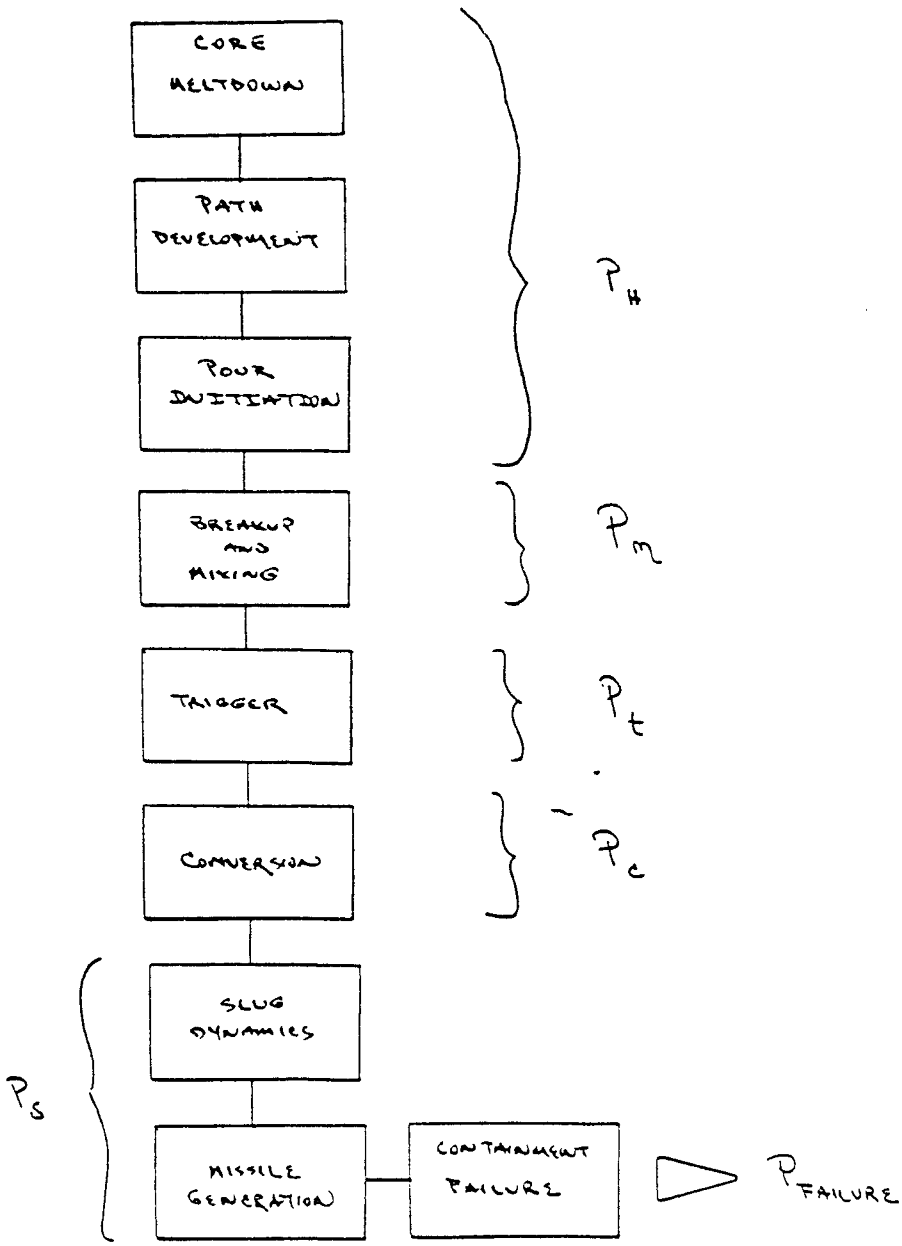


Figure 1 - Steam Explosion Sequence Flow Chart

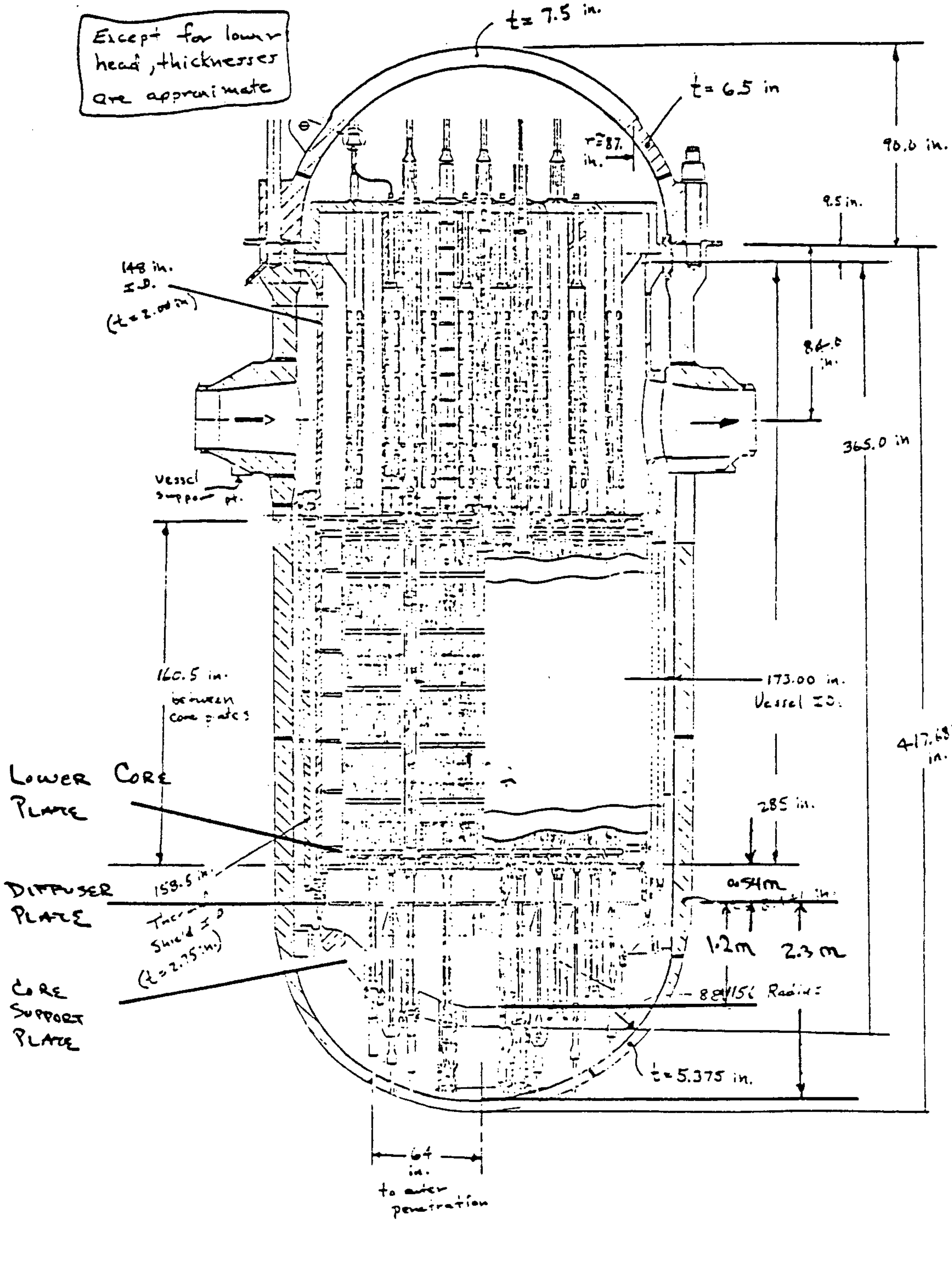


Figure 2 - In-Vessel Schematic

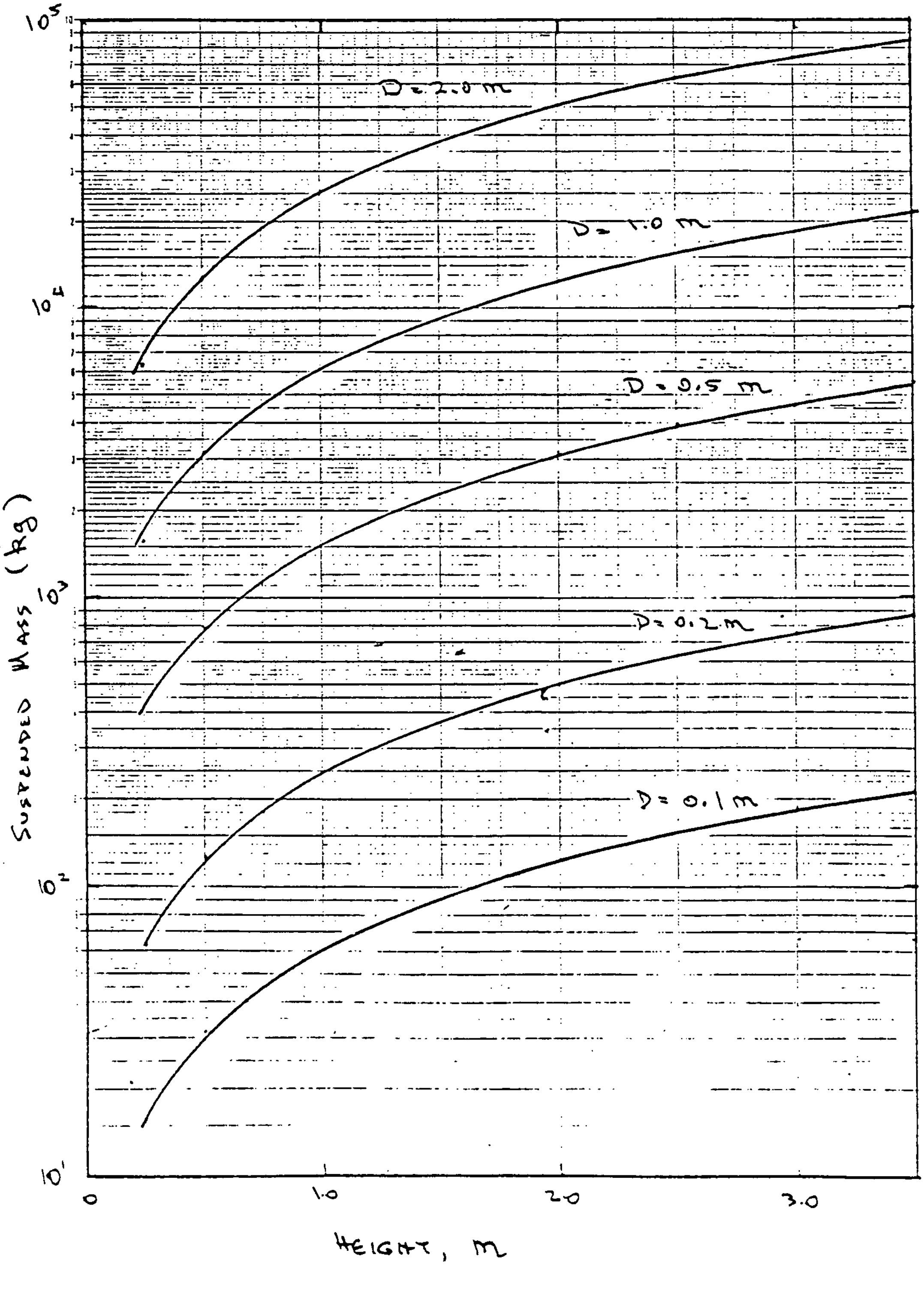


FIGURE 3 - SUSPENDED MELT MASS

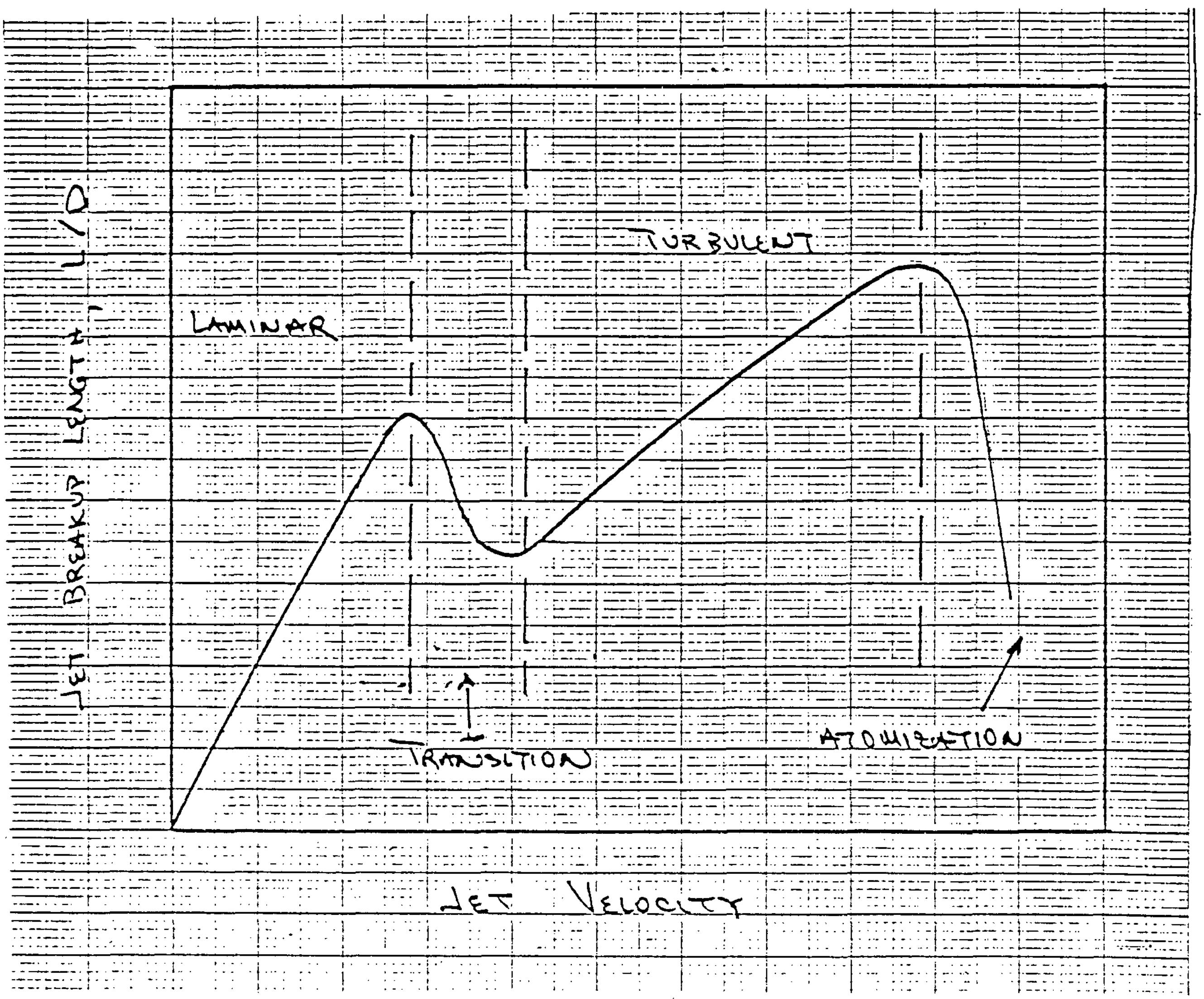


Figure 4 - Jet Breakup Qualitative Behavior

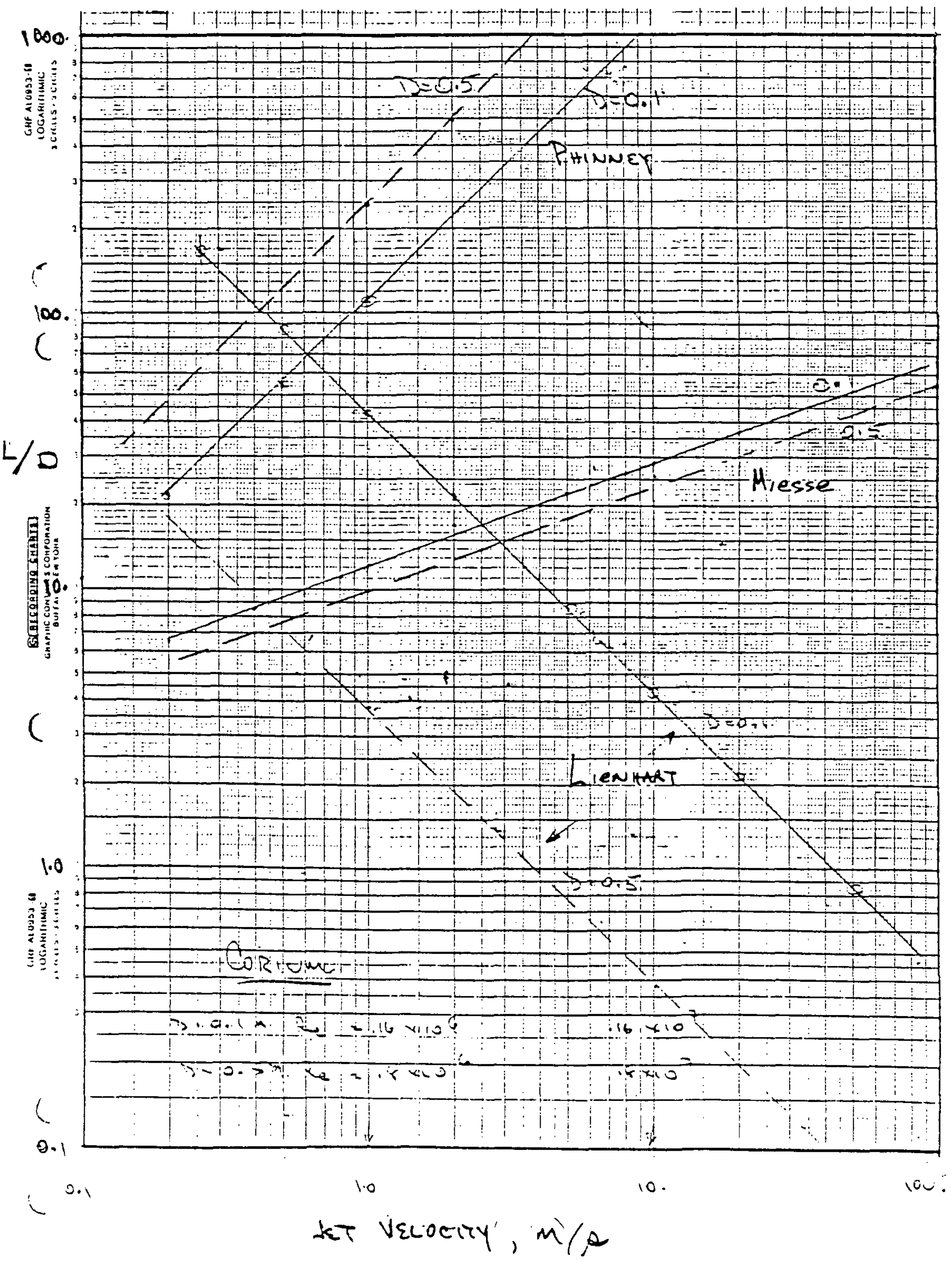
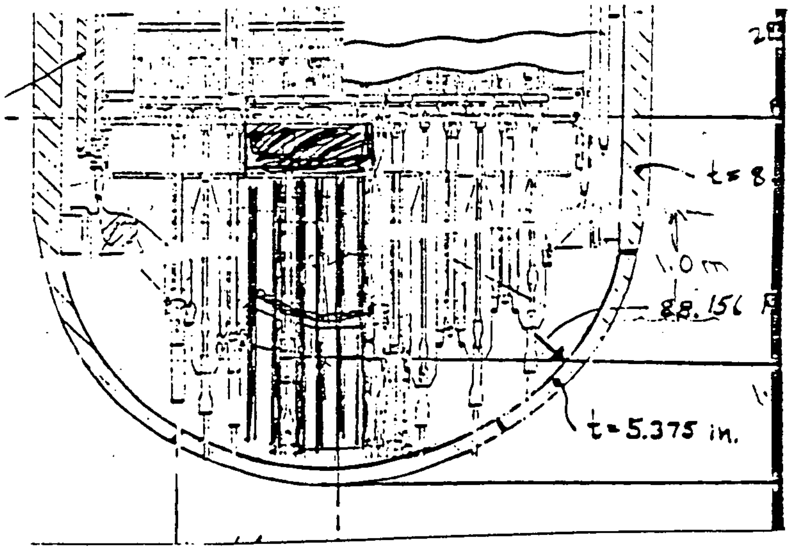
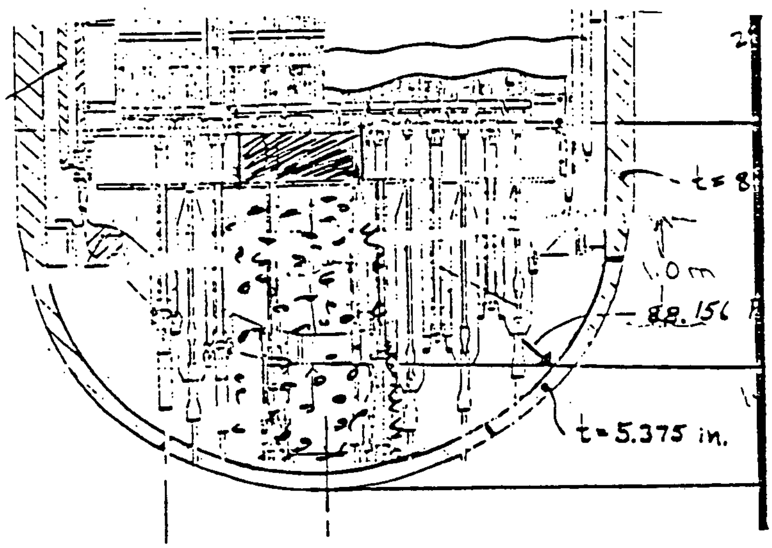


FIGURE 5 - CORIUM JET BREAKUP LENGTH

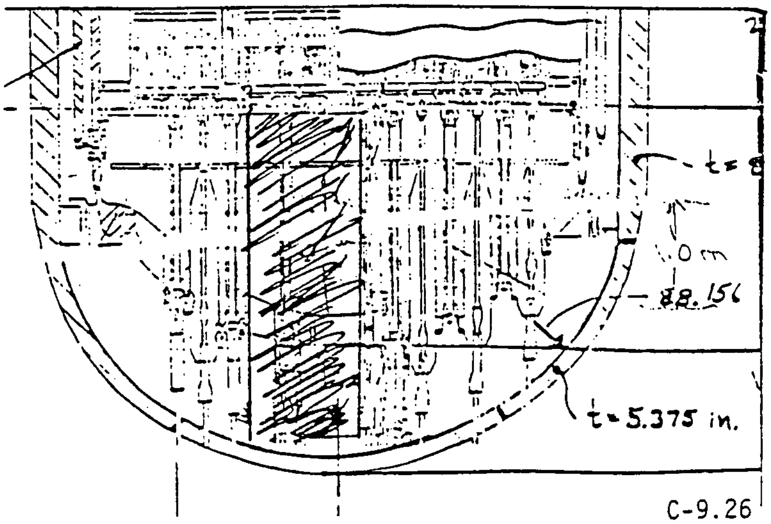


(a) Multiple Jets

Figure 6 – Schematic Melt Configuration At Instant of Explosion



(b) Coarse Dispersion



(c) Single Jet

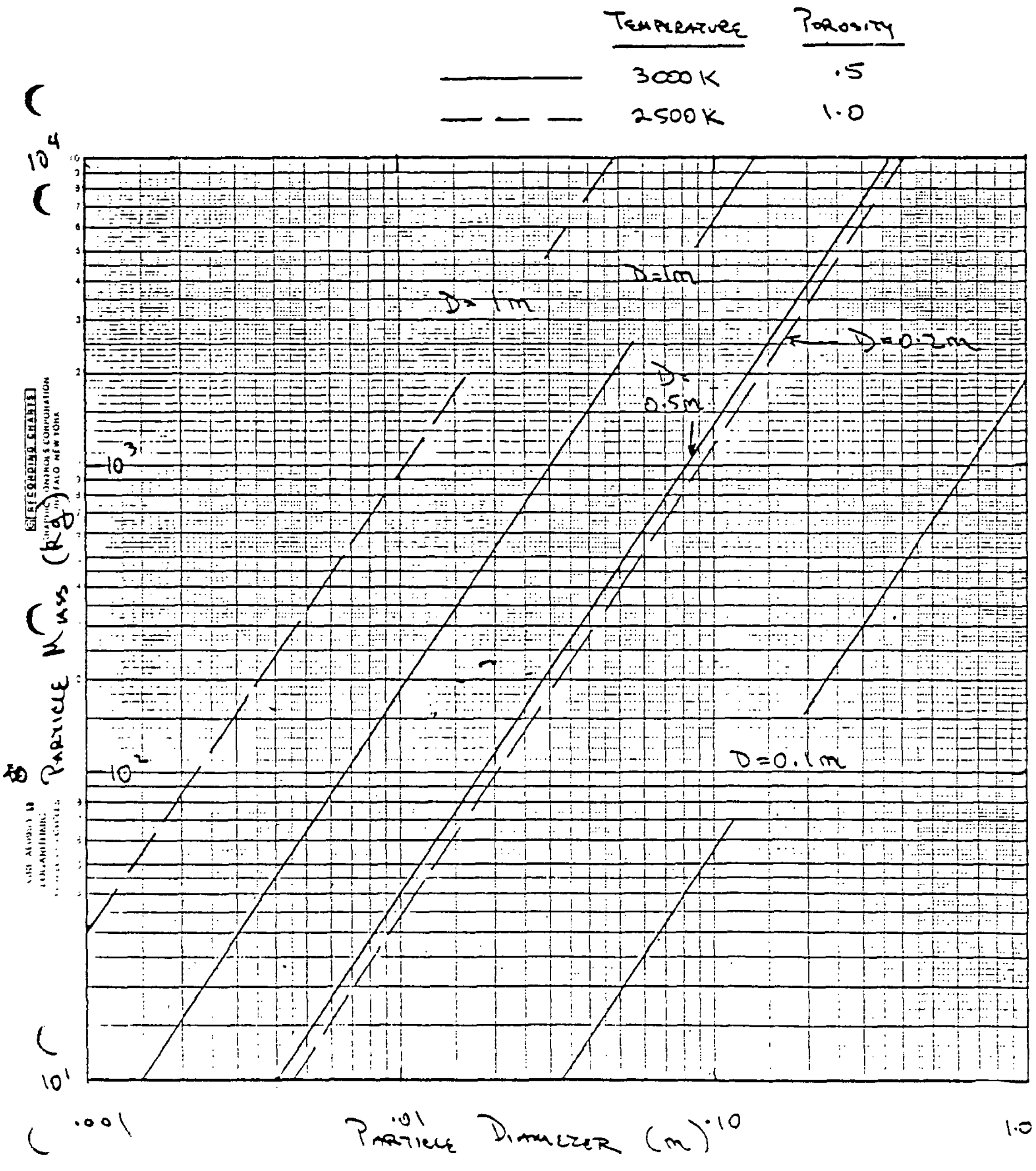


FIGURE 7 - PARTICLE FLUIDIZATION LIMITED MELT MASS

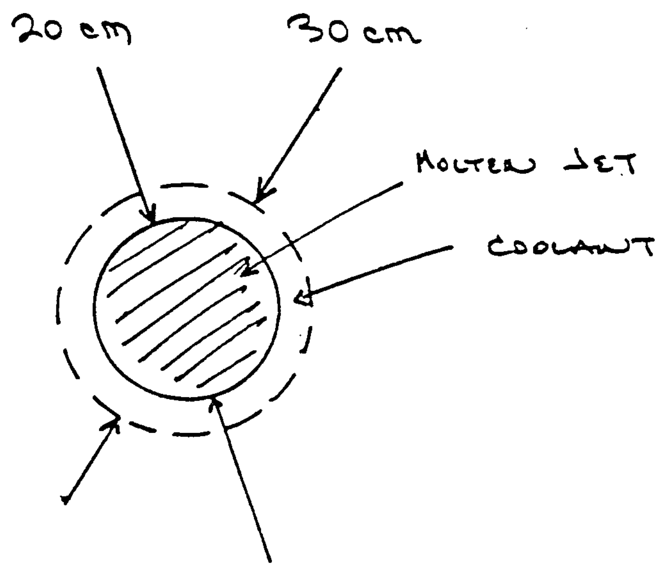


FIGURE 8 - TYPICAL JET-COOLANT GEOMETRY

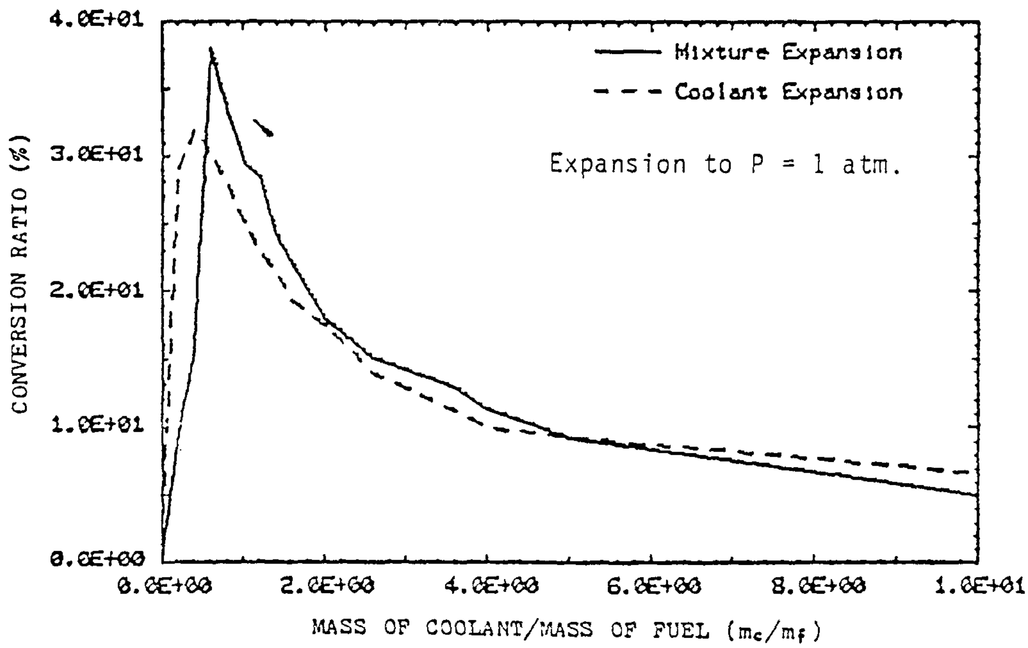


FIGURE 9 - THERMODYNAMIC LIMIT CONVERSION RATIO (CORRADINI)

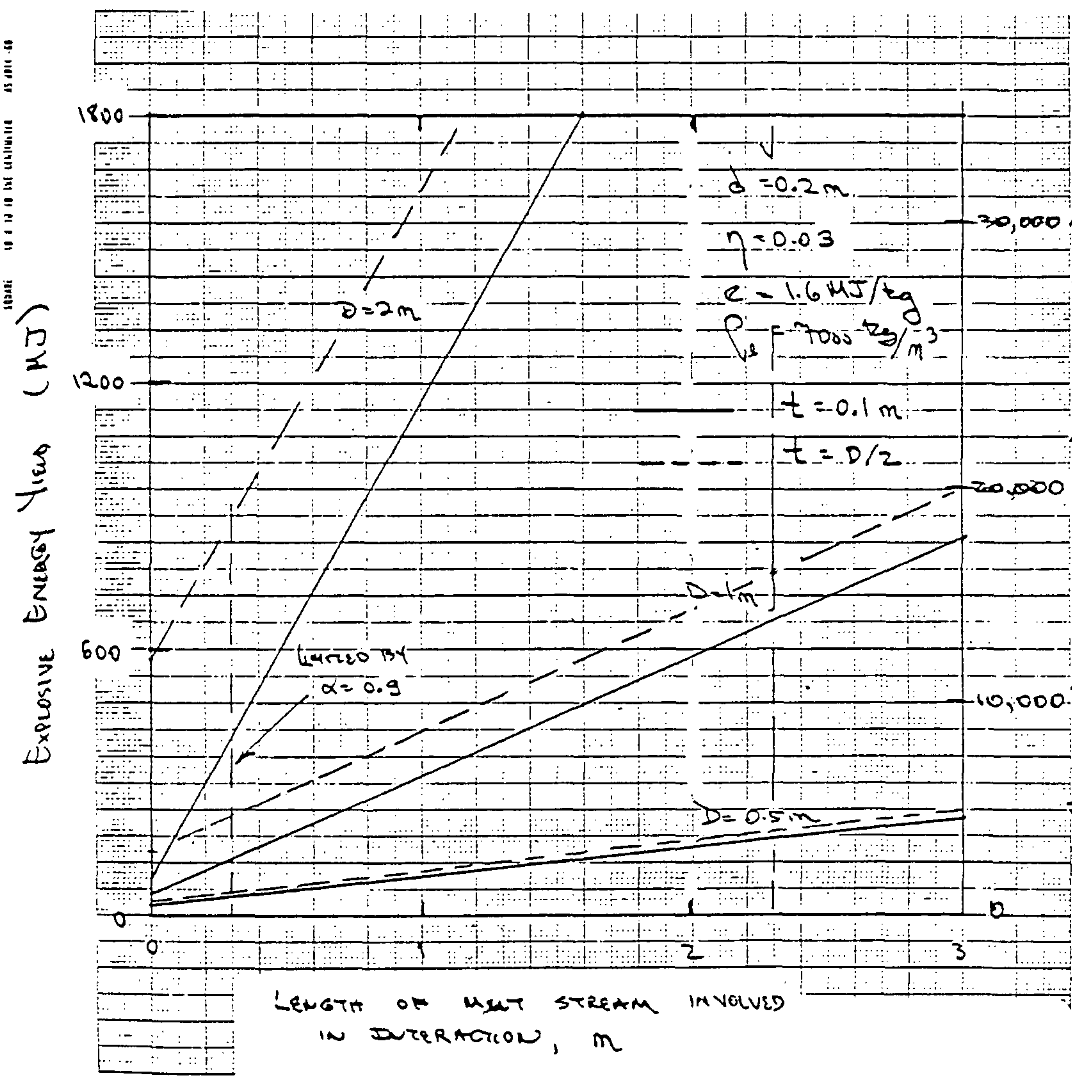


FIGURE 10 - COMPUTED ENERGY YIELD AND MASS MELT INTERACTED

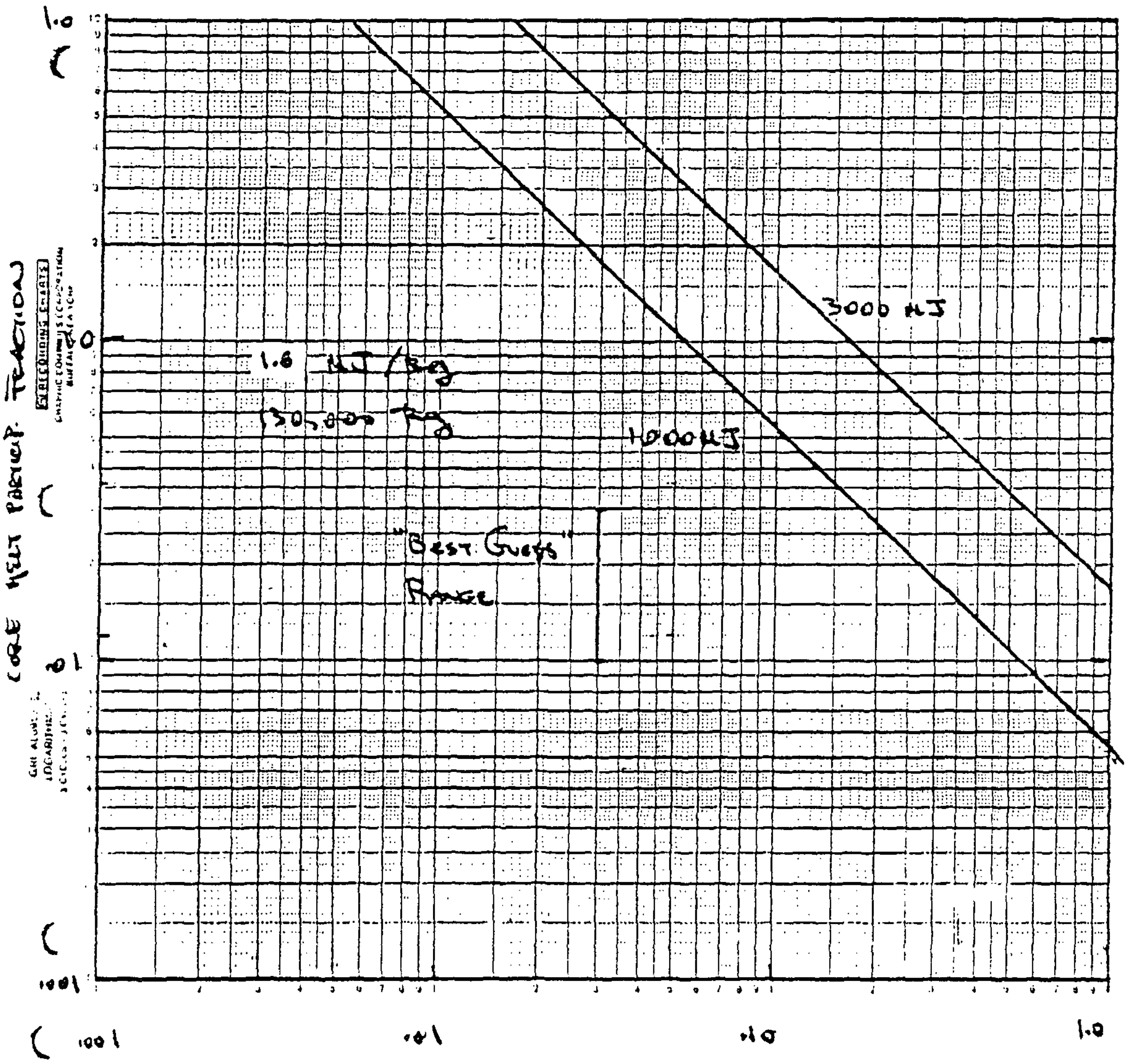


FIGURE 11- PARAMETRIC REPRESENTATION OF ENERGY YIELD

REVIEW OF STEAM EXPLOSION ISSUE

Response to Question 3 of
D. Ross' Letter of 24 August 1984

BY

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Submitted to

Steam Explosion Experts Group
Cardis Allen, Executive Secretary

November 1984

RESPONSE TO QUESTION 3

A CRITIQUE OF THE PROPOSED SNL LARGE-SCALE STEAM EXPLOSION EXPERIMENTS

1. Background for Response

Based upon my responses to Questions 1 and 2, I envision a pre-mixing stage of an in-vessel steam explosion in which one or more molten streams of corium pours into the lower plenum. The pour streams have the characteristics of turbulent jets and may break up as a result of an encounter with structure or as a result of hydrodynamic forces. The melt streams are likely to have an effective diameter of 10's of cm and may involve upwards of thousands of kilograms of melt.

The available data base of experiments involving masses of up to 25 kg are of too small a scale to allow us to reach relevant conclusions regarding the behavior of large melt streams. None of the available experiments adequately simulates a coherent jet of melt as it encounters a pool of coolant. The small masses are equivalent to the leading edge of a large scale pour. I believe that the physics of breakup and mixing typical of existing experiments are not applicable to coherent jets of melt which encounter water. The major objective of large scale experiments should be to permit "observation" of the interaction of coherent gravity-driven jets of corium and/or simulant melt with a pool of water. The basic parameters of the experiments should cover, with obvious compromises with cost and, perhaps, with instrumentation possibilities (discussed further below), those represented in Table 1.

I view the major scale factor as being the pour diameter. The significant mass quantity would be the submerged mass at the instant of the explosion, but is a dependent variable. It would depend on the pour diameter, the delay time prior to the explosion and the depth of water available. The major scale questions as I see it are:

(i) as the pour diameter increases, is the stream less likely to break up due to insufficient time (or L/D) as the diameter increases at constant water depth, resulting in reduced pre-mixing and lower conversion ratio,

(ii) if the stream does not break up as a result of hydrodynamic processes during pre-mixing, how does the "mixing depth" vary with scale, and how does the conversion ratio vary with diameter,

(iii) does the availability of structure enhance or diminish the extent of pre-mixing and conversion ratio, as a function of pour diameter.

The two experimental "observations" which are most crucial are:

(i) the extent of melt-water mixing prior to the explosion,

(ii) the conversion ratio,

where both of the above are functions of the scale of the experiment. Two possible experimental outcomes are envisioned. Either the pour stream will undergo extensive break up, or it will flow coherently to the bottom of the interaction vessel and begin to collect there prior to the explosion. In either case we need to have measures of the mass of corium submerged in the pool, the geometric configuration of the melt and the intimacy of mixing of melt with coolant at the instant of the explosion. If one can establish the initial conditions for the explosion, with some of the above measurements, then one has a chance at making some sense of the measured conversion ratio.

2. Judgement Criteria

The following defines my idealized criteria for a "large scale" steam explosion experimental program which would provide the quantitative data required for model development and for assessment of the effect of scale on mixing and conversion ratio.

Melt Delivery System

(i) generate and deliver a single-phase, coherent melt stream to a pool of water under gravity-driven conditions (see Table 1 for "ideal" range of parameters.)

Experimental Measurements

(ii) estimate the mass of melt submerged at the instant of the explosion,

(iii) determine boundaries of the interaction zone,

(iv) determine mass of water in interaction zone at instant of explosion,

(v) define geometric configuration of melt at time of explosion (pool, coherent jet, dispersion, or combination),

(vi) melt mass involved in interaction,

(vii) conversion ratio.

It is recognized that the above "wish list" becomes more and more tenuous, the larger the scale of the experiment. My feeling, however, is that we really don't yet have data even from "small scale" experiments which combine most of the features listed above. I also believe that unless we have some experiments which tell us something about the distribution of melt and water at the instant of the explosion, then we will not really be able to connect the measured conversion ratio to well-defined mixing mechanisms.

3. Critique of Proposed SNL Experiments

I will here deal only with the SEALS program, since the role of ELVIS is not clear.

I believe that we need an experimental program in order to attempt to resolve the mass scaling question with respect to steam explosions. I believe that all mixing limited explosion concepts will remain as "plausibility arguments" until data are available to substantiate any one of them. We will not, with any honesty, be able to put the problem to rest until we have demonstrated the validity of a premixing model from a suitably designed set of experiments and are able thereby to use the model to scale up to reactor conditions.

There appear to be two basic premises underlying the approach presented by SNL:

(i) Small-scale (50 kg) tests (FITSX) are or will be available to provide separate effects information on mixing and conversion ratio. These experiments presumably contain sufficient diagnostics including, perhaps, X-ray photography, to separate the two stages of the explosion. The data will be adequate to develop models for the two stages.

(ii) The function of the SEALS program is to provide a vehicle for evaluating the effect of mass on the conversion ratio as integral measure of the entire explosion sequence. The appropriate scale parameter is the delivered mass, with constant pour diameter.

With respect to the first premise: I believe that a major element of the future explosion work should be to do a 50-100 kg experiment which satisfies the criteria discussed above in Section 2. As of yet, however, I have not been convinced that this has been accomplished. It is not clear, at this point, that we have adequate diagnostics even for this small-scale test effort to separate the pre-mixing and conversion stages of the explosion. I think that X-ray photography would be a major element of this effort.

Concerning the second premise: The choice of delivered mass as the independent variable at constant pour diameter leads, I believe, to "unrealistic" pour conditions for the low end of the mass delivered range. For 500 kg of melt delivered, for example, the length-to-diameter ratio of the delivered pour stream would be about 0.2. This is no longer a jet pour mode (gravity-driven) and would lead to non-prototypic melt breakup conditions. Furthermore, with such a short L/D, the resulting vapor fluxes are likely to be low and this would bias these results to little breakup. For 2000 kg the L/D would be about 1.0 and the stream would be roughly the order of the pool depth. The pour would have the characteristics of a coherent jet pour. I think that what should be scaled here is the pour diameter, at constant total melt available. The mass entering the interaction would be governed by the parameters described above.

I believe that large-scale verification tests are necessary, but that they should be done in the light of much improved small-scale experiments with the diagnostics necessary to separate pre-mixing from conversion.

With the above caveats and basic objective in mind, I think that the general parameters of the SEALS design, as listed in Section 3 of the SNL report are reasonable. The system, however, should be more flexible in terms of the diameter of the pour stream. While 0.85 m is reasonable as an upper bound, the system should be able to simulate smaller melt stream diameters, which have a better chance of breaking up due to instabilities. It is quite likely that a 0.85 pour stream will not break up in the 1 to 2 jet diameters available to it during its transport through the pool. Minimum pour stream diameters of 10 to 20 cm would be reasonable. (Such diameters would be the upper limit attainable in the 50 kg test program.)

I believe that the SEALS program should proceed as rapidly as possible from SEALS1 to SEALS3, with SEALS1 serving as the instrument to develop the pour technique and to provide visual observations as much as possible. SEALS3 provides a closed vessel and the ability to collect and size debris, an advantage over SEALS2. I'm not sure that SEALS2 is a desirable intermediate stage. For all test phases, attempts should be made to develop methods of measure the parameters quantities listed in Section above.

4. Recommendations

I support the concept of development of a large-scale steam experiment as confirmation of pre-mixing and conversion models developed from small-scale experiments. The priorities of the research program should be as follows:

(i) Develop method for coherent pours for 50-100 kg experiments, leading to simulated pour streams of up to 10-20 cm in diameter.

(ii) Develop X-ray photographic technique for probing interaction zone. Lead to quantitative assessment of fuel distribution during premixing phase. Perhaps two views for 3-D reconstruction.

(iii) Proceed with SEALS1 design, with scale parameter being the pour diameter. Pour length should be at least the depth of the water pool. Develop methods to measure such quantities as the submerged mass at the time of the explosion.

Table 1

Range of Test Parameters

Pour Diameter (D)	0.1 - 1.0 m
Water Pool Depth (H)	1 - 3 m
Pour Length (L)	L/H > 1
Water Temperature	Saturated, subcooled
Simulant Melt	Fe/Thermite Corium
Melt Mass	∿ 2000 kg

STEAM EXPLOSIONS AND THEIR RELEVANCE FOR PROBABILISTIC RISK ASSESSMENT

F. Mayinger

NRC Workshop November 27-28, 1984

1. Introduction

There is some disagreement about the role of steam explosions in probabilistic risk assessment. Former risk studies /1,2/ assume and impute with a certain probability that a steam explosion could damage the containment during a severe accident. In this case an early radioactive release with severe consequences would occur.

In the meantime there is - thanks to experimental and theoretical efforts - at least agreement in the International Nuclear Safety Community that the risk from steam explosions was originally overestimated by several orders of magnitude. These conclusions are still based on hypothetical assumptions for a hypothetical event, and one should really argue how reasonable it is to treat the sequences and phenomena of a hypothetical accident in a physically often unrealistic way.

In the following an attempt is made to draw a conclusion, whether a steam explosion can or cannot endanger the integrity of the containment and/or the reactor pressure vessel of a pressurized water reactor. The deliberations are based on three reports /3,4,5/ being recently published, or being in the process of publishing in the Federal Republic of Germany. As far as special design criteria are concerned, the conclusions may mainly or only be relevant for German PWR's.

2. Status of Knowledge

Worldwide numerous experimental and theoretical research activities are under way to study the phenomena and the consequences of steam explosions. Here only a few of them, being mainly important for risk deliberations, shall be briefly discussed.

The experiments performed can be roughly subdivided in four categories, depending on the aim of the study, namely in experiments looking for
- melt water contact in the pressure vessel,
- melt water contact outside of the pressure vessel,
- fundamental aspects,
- influence of system pressure.

Here only a few newer experiments shall be discussed; olders are very well reviewed in /6/.

The Sandia-PITS-Experiments /7-10/ had the aim to get a better understanding about explosions with larger masses of melt. The lessons learned from these experiments were:
- With increasing melt mass no trigger is necessary to initiate the steam explosion. It usually starts 0,5 to 3 s after the beginning of pouring in.
- In many cases the interaction started before the total mass of melt was in the water tank.
- The efficiency of the explosion is decreasing with the diameter of the fragmented particles.
- The maximum efficiency found in the experiments was 1,34%; however, with 90% of all experiments the efficiency was lower than 0,5%. With hot or boiling water an efficiency of only 0,3% was reached.

This series of experiments was the basis for ongoing studies which were better instrumented. The FITS-Experiments, also performed at Sandia, had the intention to study a variety of influencing parameters /11-15/.

These research activities showed that:

- Steam explosions occur not only with molten metal and thermit but also with CORIUM.
- The self-ignition at ambient pressure is only depending on the melt mass; with CORIUM about 4 kg.
- With melt falling into water the position where self-ignition starts may vary, sometimes it was observed already at the surface or on a vertical wall; latest, however, at the bottom of the water vessel.
- At higher pressures steam explosions are suppressed unless a triggering mechanism is used.

In the Federal Republic of Germany KWU performed a series of steam explosion experiments to study the interaction between water and melt during the so-called "fourth phase" of a core melt down process. In this phase, due to the penetration of a wall in the pressure vessel-cavern , water is flowing over the melt. The experiments showed that as long as the water level above the melt is not too high, a steady evaporation of the water without steam explosions will usually occur with the melt surface being liquid. With increasing water level the melt starts to freeze at its surface with periodically violent eruptions, followed by strong evaporations which, however, are no steam explosions /16/. A similar experience was made by the author of this paper himself /17,18/ in experiments, where in addition gas was blown through the melt to imitate the H_2-production during the interaction between melt and concrete. Also in this case only sudden evaporations but no steam explosions were observed. A similar kind of flooding experiments were also performed at Sandia, however, with small amounts of melt /19/. Here one can argue, whether due to the small amount of melt or due to the flooding process no steam explosion occured without a triggering mechanism.

Newer experiments within the FITS-series, also with flooding the water over the melt, showed a very violent and eruptive evaporation rather than a steam explosion. The conditions in these experiments, however, were not quite comparable with the reactor situation because the temperature of the melt would not be as high as 3000°C as it was in the experiments.

A special scenario discussed in the United States is the imputation that melt is blown out under high pressure through a hole of the pressure vessel into a water reservoir. This situation is physically impossible with German pressurized water reactors because there is no water in the cavern below and around the pressure vessel. The melt jet would only hit a thick concrete wall.

The influence of the system pressure and, by this, the influence of a high pressure atmosphere in the reactor pressure vessel was researched in a series of experiments performed at EURATOM Ispra /20,21/. From these experiments the general conclusive statement can be drawn that with system pressures higher than 2 MPa, steam explosions could only be initiated with very strong detonative triggers. This, in general, is also confirmed by the MFTF-experiments /22/, even with some of the test results apparently being not in agreement. Here one has to be aware of the fact that the cover of the MFTF-vessel hits the bottom of the vessel, which acts as a trigger for the steam explosion.

3. The Steam Explosion During and After a Catastrohpical Failure of the Core

There is a large number of partially highly sophisticated theories describing the phenomena in connection with steam explosions and trying to extrapolate from experiments the mechanical action on reactor components and, by this, the possible or not possible damage due to steam explosions. It would by far break up the frame of this short report to discuss them with all their benefits and draw-backs. Therefore, briefly only one theory -comprehensively described in /23/- shall be mentioned, the so-called "detonation theory". Comparisons with a THERMIR-experiment /24/ showed good agreement with respect to the pressure-time-behaviour, as well as to the expansion of the shock-front. The theory also shows that above a system pressure of 2 MPa no detonation wave can develop and that in case of a CORIUM-water-system no detonation situation could be predicted in which the maximum pressure of the wave-front was larger than the layout pressure of the pressure vessel. However, one has to be aware of the fact that also this theory - as all other theories - starts from the assumption that the melt is homogeneously mixed with the water before the detonation is initiated. To do this premixing additional forces - i.e. momentum forces from jet flow - have to be available. Risk studies concerning the impact of steam explosions very often also assume that the premixing and pre-fragmentation is a given situation and do not spend many thoughts whether such a situation is physically possible for a large amount of melt.

All theories, however, agree that the following eight conditions have to be simultaneously fulfilled to enable the development of a large steam explosion with serious consequences:

1) There must be a sufficient and as good as possible homogeneous premixing between melt particles and water, which stays long enough with a large amount of melt.

- 6 -

2) During this premixing period the steam explosion must not start too late, otherwise the premixed and pre-fragmented particles would cool and freeze due to film boiling.

3) In the experiments the delay-time after a steam explosion started was always below 3 s, which means that the pre-fragmentation and premixing has to be completed for a large amount of melt within this period. This needs extremely high momentum- or viscous-forces for the mixing process.

4) The heat transfer area between melt and water must be extremely large for a catastrophic steam explosion, which is only possible if the molten material in a second step undergoes a fine-fragmentation resulting in particle diameters in the order of 10^3-10^6 m.

5) These microscopically fragmented particles of the melt must have very close liquid contact with the water, which is only possible if each of the fragments is surrounded by a small volume of water, approximately equal to its own volume, and if all fragments are homogeneously distributed in the water. This microscopic fragmentation has to occur in an extremely short period - a few milliseconds - and this for a large amount of melt.

6) The liquid contact between melt and water must be long enough without any boiling phenomenon at the interface, in order to transfer enough energy for the subsequent steam explosion.

7) The melt water mixture must be completely homogeneous because any discontinuity would deflect, retard or damp the shock-wave, which would result in a strong reduction of the steam explosion impact and would rather produce several small steam explosions instead of a large one.

8) There must be not only enough melt available but also enough water,which during several sequences of a core melt process is not the case, and if it is the case, there is not enough momentum force to premix melt and water.

All these eight conditions have to be fulfilled to make a large steam explosion possible.

We have now to discuss what happens in the reactor and what pathes of severe accident sequences do we have to follow up. Experiments showed that steam explosions in the pressure vessel above a pressure of 2 MPa must not be taken in account at all and that also in the region of 0,5 to 2 MPa a steam explosion is only possible if a strong trigger exists. Finally the melt can come in contact with water if the concrete wall of the biological shield around the pressure vessel fails. However, then we have the situation of flooding water above the melt. So we have to take in account three pathes:

- Low pressure path: self-ignition of a steam explosion if the melt from the core flows or falls into the water in the lower plenum.
- High pressure path: in case of a small leak or a station black-out there must be a trigger with enough energy to start the steam explosion.
- Containment situation: the interaction between melt and flooding water after damaging the concrete wall of the biological shield has to be taken in account.

The question, whether a steam explosion can endanger or damage the pressure vessel can be attacked from two sides:
First one can argue, what is - under pessimistic assumptions - the maximum amount of melt which could interact with the water in the lower plenum during a core melt down and can the pressure vessel withstand the impact of this reaction?
The second possibility is to look for the maximum allowable mechanical load onto the pressure vessel, and then to ask what would be the corresponding mass of melt to produce this mechanical impact?
Both ways were gone in German studies.

Körber /3/ studied the maximum mass of melt which could flow into the water of the lower plenum until a steam explosion occurs and which would be available for the melt-water-interaction.

He took in account the freezing of originally molten material in lower parts of the core and made deliberations, how stable a crust or a frozen layer above the lower fuel element endboxes could be,before it would be penetrated by the melt lake.
He also calculated the down-flow velocity after opening of the crust and assumed that a hole suddenly opens which has the cross section of 2 fuel elements.

How difficult it is to keep only a few hundred kg of hot CORIUM-melt in a vessel, is well known by all experimentalists doing research in core melt down and in steam explosions. In spite of this experience it is often assumed that several tons of liquid melt could be collected above a frozen layer and that this frozen layer would then fail over its total cross section. This is physically impossible; the melt will,furthermore,continuously flow through the lower endboxes into the water, due to its low viscosity. A continuous flow of melt into the lower plenum would result in a mass flow rate of approximately 100 kg/s. However, Körber /3/ in his study made pessimistic assumptions and, based on strength and stress calculations /25,26/ under high temperature, as well as looking to the failure mode of the core, he predicts with the assumption of re-freezing and crust formation, with pessimistic assumptions a maximum melt flow into the water of 1700 kg/s.

From the experiments it is well known that the ignition of the steam explosion with large melt masses starts automatically usually after the first contact of the melt with the water, however, latest when the melt hits the bottom of the pressure vessel. Taking this in account, Körber /3/ comes to a maximum mass of melt of 2000 kg, which could react in a steam explosion. Here it has to be emphasized that, in addition to the availability of this mass of melt, all eight conditions mentioned before have to be fulfilled.

The mass of melt which could react with the water is also a function of the mass of water being present in the lower plenum. With decreasing water level in the lower plenum, even with very large amount of melt being available, only a part of it could react.

In Fig.1 the dependency of the reacting melt mass on the water level in the lower plenum is shown. In addition, one has to realize that the lower plenum is not an empty volume, where the shock waves of a starting steam explosion could expand unprevented. There is a structure supporting the core in the lower plenum, as shown in Fig.2. This supporting device in German PWR's guarantees that the core structure and, by this, the frozen layer cannot break down at once, because it is still cooled by water until the steam explosion starts. The supporting device, however, also is conducting heat from the lower fuel element endboxes into the water, which produces boiling and so the falling down melt would not find an ideal water pool, as it is the case in the experiments, but a foaming two-phase mixture which is much less favourable for steam explosions.

Wagler and co-workers /4/ went the other way in a recent study. They looked for the maximum allowable mechanical load on the pressure vessel of a 1300 MW German PWR. They took in account most of the experiments performed in the last years, started from very pessimistic assumptions and most favourable conditions for the steam explosion. Based on these pessimistic assumptions they found that the pressure vessel of the above mentioned reactor could withstand a steam explosion, where 50000 kg melt would interact with water at once, without being damaged. Under less pessimistic assumptions the allowable amount of melt reacting with water would increase remarkably, as shown in Fig.3, which is taken from /4/.

The study by Wagler and co-workers /4/ is based on the newly developed computer code KODEX. 50000 kg melt reacting with water in a steam explosion are far away from any imaginable physical possibilities.

After melting through the reactor pressure vessel, the melt does not come in contact with water immediately. This only takes place after the failure of the biological shield due to melt-concrete-interaction. Studies /5/ showed that the increasing volume, which is a consequence of the failure of the biological shield, helps to reduce the pressure wave of a potentially arising steam explosion. The containment would not be dangered in any case.

One could now argue that there may be a failure of the core and of the frozen layer in a pressure range between o,5 and 2 MPa, due to any highly unprobable reason. Here we first have to realize that the accumulators feeding in emergency core cooling water open at 2,8 MPa (German PWR) or 4,4 MPa (US-PWR) respectively. This means that the core was cooled down before it can start again to heat up and to melt. This heating process is rather slow because only the decay-heat is available. In this slow process heat conduction and radiation will evaporate the water in the lower plenum, and if the core finally would fail, there is almost no water present in the lower plenum. However, even if we assume a steam explosion in this pressure range, it would not damage the pressure vessel. Certainly the elastic reserves of the pressure vessel are smaller at this elevated pressure, however, the plasticity of the pressure vessel structure is increased due to the higher temperature. Therefore, even with this higher system pressure the pressure vessel could withstand approximately the same interacting core melt as with the lower system pressure. This is also valid for higher system pressures up to approximately 10 MPa.

4. Consequences for Future Actions

A detailed survey of the international literature and also studies performed in our country showed that the integrity of the pressure vessel or of the containment structure of a modern German pressurized water reactor would not be endangered due to a steam explosion. This statement is valid without raising a loan, from probabilistic studies or from deliberations, with what probability which course of any severe accident may occur. So in risk studies steam explosions leading to an early contamination of the environment should not play any role for future.

Another question is whether research activities in steam explosions should be continued or not. There are several phenomena of great general interest connected with steam explosions, which are up to now not well or almost not understood. Steam explosions are not only a matter of nuclear safety, they can occur and occurred in foundries, in paper factories, and they may also happen with handling liquid methan or any other deeply frozen fluid. The emphasis of these tests, however, should be put on the understanding of the mechanisms and not so much on demonstrating the powerfulness of artificially scaled up and initiated steam explosions.

In nuclear safety the habit developed that it has always to be proved with what probability or improbability a sequence of a severe accident can occur. Perhaps it would be sometimes wise to turn around the question and to ask how it is imaginable that a hypothetical sequence of a severe accident leading to a catastrophical failure could be verified, if one would get the task to do it. I think, everybody would be overcharged if he would get the task to bring several tons of hot melt homogeneously and simultaneously to react in a powerful steam explosion.

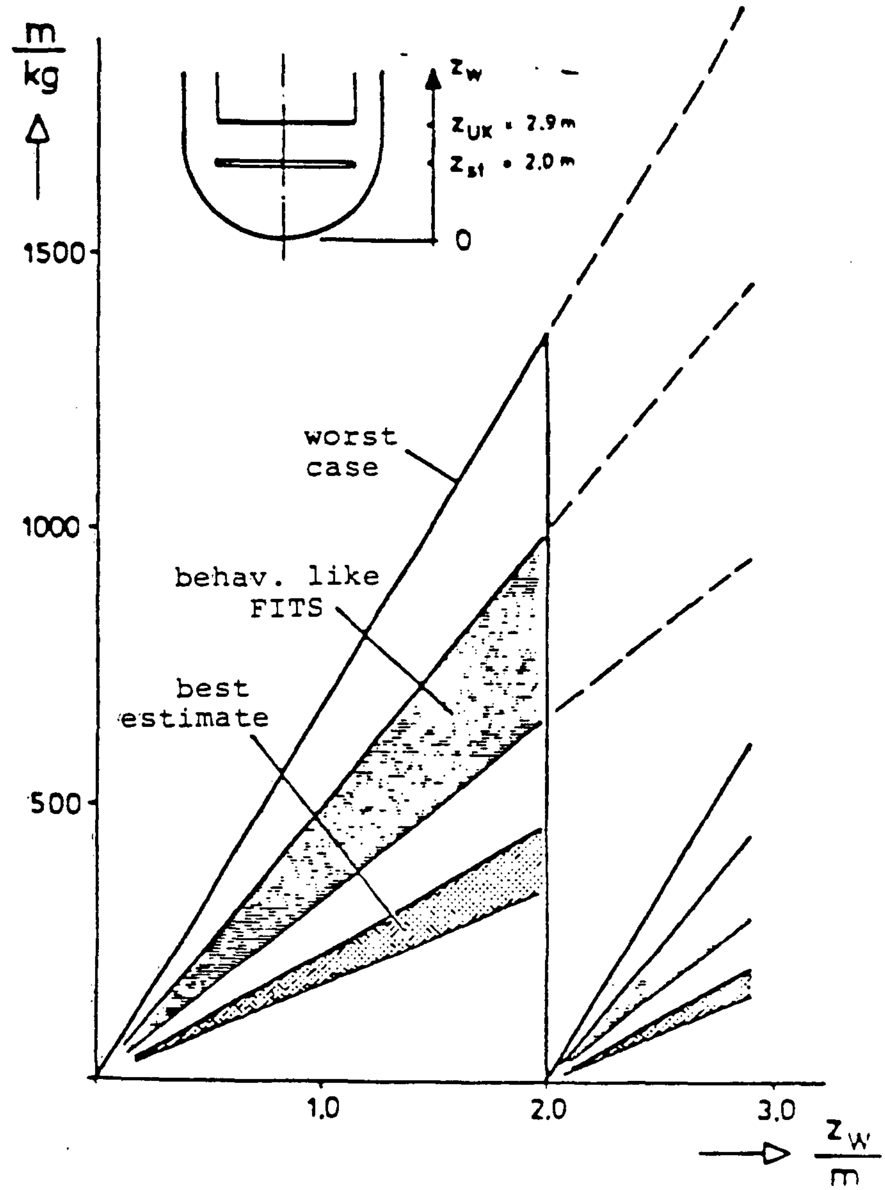
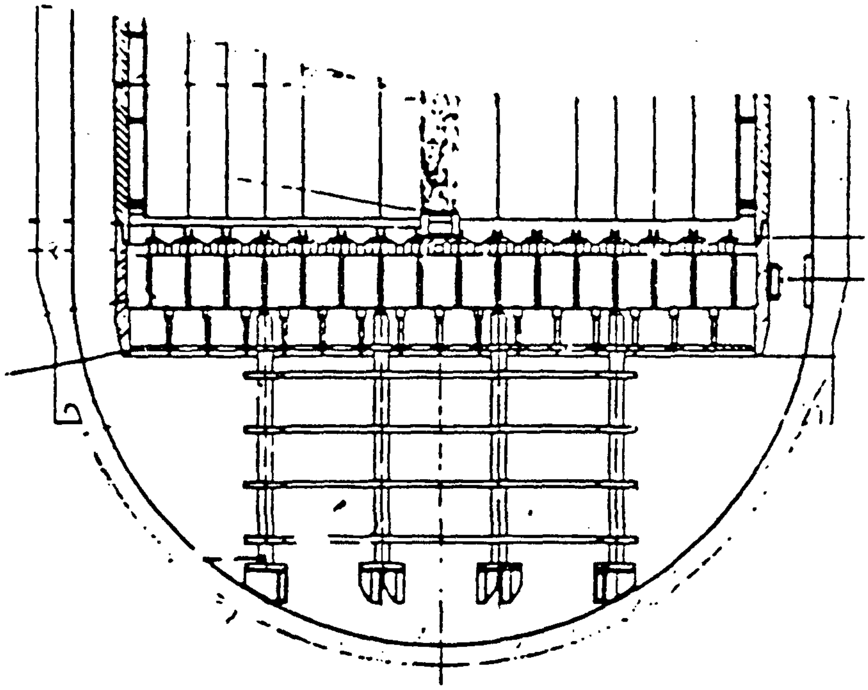


Fig. 1: Mass of melt available for steam explosion during core melt down depending on water level Z_w in lower plenum /3/



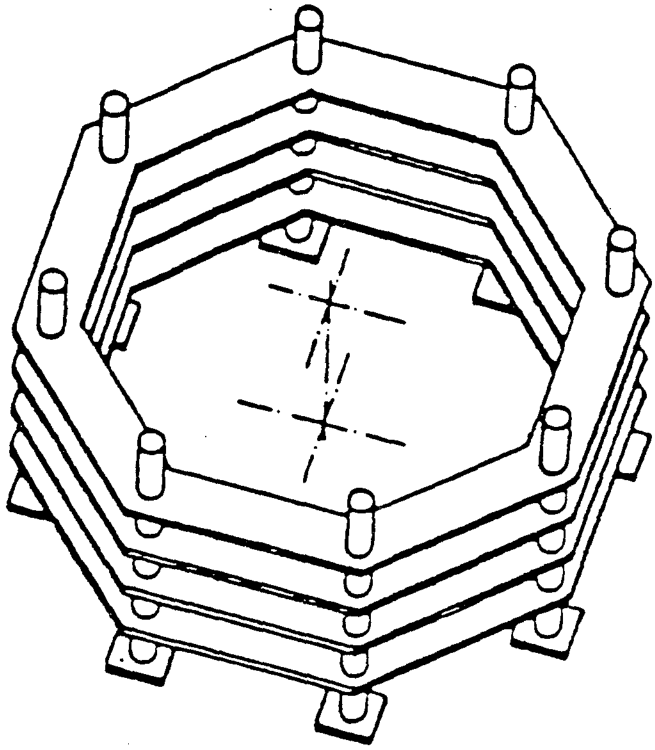


Fig.2: Core support structure of a German 1300 MW PWR

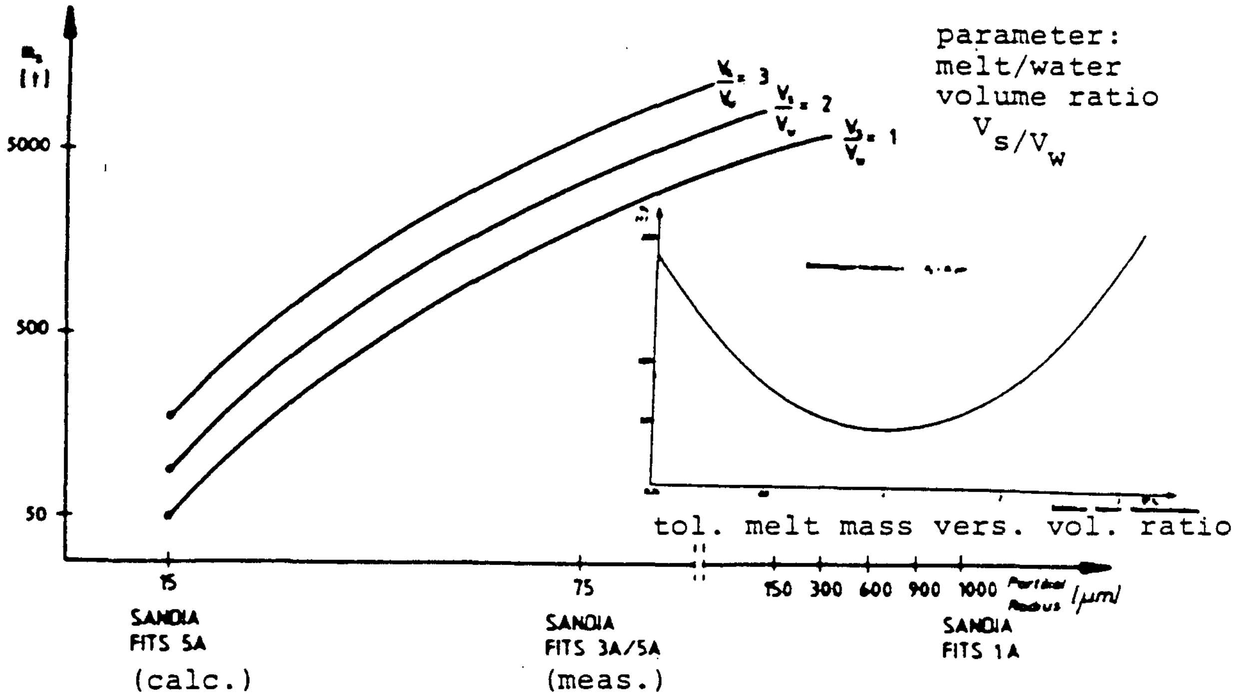


Fig.3: Tolerable melt mass for steam explosion in the pressure vessel of the German 1300 MW design (low pressure core failure case)

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NS-RAT-DS-84-003

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Subject: Containment Failure Probability by an in-vessel steam explosion.

Dear Dr. Ross:

In response to your letter of August 24, 1984, I have reviewed again all the relevant material at my disposal. I have also planned to carry out an additional computations, however because of other constraints I was unable to do so as of this writing. I hope to be able to submit the results of additional calculations at the forthcoming Harper's Ferry meeting.

1.1 Re-evaluation of Reference (1)

In response to your first question, my judgement for the best estimate value for the probability of containment failure arising from a steam explosion, given a core melt, is of the order of 10^{-4} per core melt.

This value is judgmental and contains some uncertainty, however my judgement is based on published scientific work relevant to the subject, and where data are lacking, on engineering judgement alone.

The following discussion would describe in brief the supporting material (a copy of Reference 1 is attached for your convenience). In Reference (1) an attempt was made to define the above mentioned probability, and the reasons for following the event tree depicted in Figure 1 of Reference (1) were discussed in some details based on published scientific literature.

The estimate of the probability of containment failure by an in-vessel steam explosion, given a core melt, was judged to be of the order of 10^{-4}. This estimate should be re-evaluated if any of the assumptions made in Reference (1) are contradicted by new information. We have quoted several references (including results of experiments at Sandia) which indicate that saturated water is less likely to trigger a steam explosion. The recently issued Reference (2), (on which I will comment below in response to your second question) questions our supposition on subcooling (on P. 17) and quotes spontaneously triggered explosions in hot water.[3,4]

I have reviewed the above references [3,4] and noticed that all the experiments were carried out with iron/alumina thermites rather than corium. Even if we assume that the thermite generated iron/alumin melt is a good simulant for corium (which I am not quite convinced that it is), Reference 3 still noticed a reduction in spontaneously triggered explosions in saturated water. Furthermore, the early thermitically generated melts at Sandia were associated with a large amount of gas dissolved within the melt, and the importance of the dissolved gas was realized at a later time. Thus it is quite conceivable that the effect of water subcooling on triggering was masked by the method of melt preparation and by the chemical composition of the melt. The more recent CM series of experiments at Sandia[4] (which reference (2) used to dispute our supposition on the effect of subcooling on triggering) were also done with iron/alumina and nearly saturated water. This CM tests have resulted however in surface interactions when the melt contacted the saturated water, causing the expulsion of the melt. Quoting Reference 4 (May - June, 1983) "If surface interactions occur in saturated water at ambient pressure in the reactor case, as they have in the CM tests, then it may be very difficult to have a large-scale steam explosion, under those conditions". In other words, our event tree[1] could be terminated at that point and the probability of containment failure would be negligibly small. In the September - October 1983 status of core melt programs[4] it was suggested that perhaps surface expulsions are common to iron/alumina interaction with saturated water, and that melt composition, preparation or delivery may have changed from the previous tests to produce this surface events. In fact the recent CM and OM tests at Sandia[4] (which were unavailable to us at the time of the publication of Reference 1) cast doubt on the capability of iron/alumina thermitic melt to simulate corium melt.

The issue under discussion in the above paragraphs is the effect of water subcooling on the triggering (and perhaps the propagation) in reactor material. The integral nature of the simulant experiments at Sandia has failed to resolve this question and has even demonstrated that under some circumstances surface interactions with saturated water could prevent large scale steam explosions. An additional evidence[5] that has become available since the publication of Reference (1) supports the supposion that water subcooling promotes triggering. In fact, the recent experimental study[5] at the Berkeley Nuclear Laboratory at the U. K., which has employed molten tin and water, has successfully used this concept (of triggering an explosion by subcooled water) to design the testapparatus. Molten tin fell through a 1 meter long section of nearly saturated water without exploding and was triggered in a subcooled water section at the bottom. In addition, a recent sophisticated theoretical analysis at Winfrith[5] has demonstrated the crucial effect of water subcooling on film collapse and hence on triggering.

The above discussion, which is based on recent evidence unavailable at the time Reference (1) was published, supports our early conclusion that a spontaneous explosion is less likely to be initiated in saturated water, and therefore the absence of subcooled water justifies a reduction in the conditional probability (see Figure 1 of Reference 1). I draw this conclusion in spite of the recent study at Winfrith[6] where a single test was apparently triggered spontaneously in saturated water when 24 kg of UO_2/M_0 thermite generated melt has interacted with water. I believe that the particular mode of melt release in the Winfrith experiments[6] (melt confined in a catch pot) may be responsible for triggering an interaction irrespective of water subcooling. Surely, the Winfrith melt release mode is a prototypic, requiring further analysis and understanding of the melt/water mixing mechanism.

In Reference (1) we have not reduced the conditional probability at several stages before fuel-water slug generation. The reason being that some uncertainty remained in the understanding of the physical processes involved. Uncertainty still remains, however it is not unreasonable to reduce the conditional probability at each of the following stages of Figure 1: low RPV pressure, adequate coarse premixing, trigger present, and energy absorption by internal structure. Little new information was published on any of the above subjects that can conclusively justify a reduction of the probability, and confirmatory research may be required, however based on current knowledge and using engineering judgment a reduction of the probability in these stages is justified. In particular, suppression of explosions at high ambient pressure (to reflect just the fraction of high pressure sequences), availability of adequate trigger at high pressure, adequate coarse premixing, energy absorption by internal structure, and of course the absence of subcooled water as discussed above, justify in my judgment the reduction of the conditional probability from $10^{-2}P$ to $10^{-3}P$ as depicted in Figure 1. Although recent analyses of fuel/coolant coarse mixing by the University of Wisconsin and the Northwestern University[7,8] are more rigorous than previous analyses[9,10], non-of the analyses and no melt-coolant interaction experiment have addressed the coarse mixing stage in the presence of substantial lower plenum structure and clutter. It is quite plausible that the presence of structure in the lower plenum would act to prevent rapid and effective large scale mixing. Furthermore, the recent mixing analysis by North Western University[11] has demonstrated that a substantial mass of the interacting fuel is in a region of a high void fraction of water, and thus would result in an inefficient explosion involving only fraction of the melt.

The last stages in Figure 1 relate to the coupling of the energy between the explosion and the containment, i.e. to the generation of a large missile (e.g. the RPV upperhead) by a water/fuel slug impacting upon the upper head. No recent evidence was published which indicates that a coherent unvoided slug could impact the upper head; and based on the previously published analyses by Sandia and LANL (which was used in Reference 1), I believe that the probability of containment rupture by a large missile should be reduced from $10^{-3}P$ to $10^{-5}P$ as shown in Figure 1. Admittedly, the energy delivered by a slug to the vessel head is quite sensitive to the volume fraction of steam in the slug, however the previously assumed void fraction[12] is not unreasonable and perhaps somewhat conservative, based on recent mixing analysis[11]. Of course the slug impact model itself is quite conservative since it does not account for: slug breakup by the remaining structure and by Taylor instability, multidimensional effects, and the higher energy required if the lower plenum fails

Another obviously conservative assumption concerns the mode of failure of the upper head and the missile propulsion aspects of it. Previous analysis at Sandia[13] has indicated that the top of the RPV head fails before the bolts fail whereas a more recent analysis[2] indicates that failure at the studs is possible without failure at the top of the head. If indeed failure at the top of the head occurs first, the effectiveness of the RPV head as a large missile would be reduced. Furthermore, in order for the RPV head to serve as an effective large missile all bolts must fail simultaneously and the head must be propeled vertically upward. It is quite conceivable that the RPV head would not be an effective large missile because of its failure mode[2], although an accurate supporting evidence has not been published.

Two recent experiments at Sandia and at Winfrith have indicated that under some circumstances steam explosion can be relatively efficient. They are the RC-2 test in a confined geometry (Reference 4 - September-October) and experiment SUW09 of reference (6). It appears that the conversion ratio (or efficiency) of test RC-2 could not be determined accurately from the available information, although apparently a higher efficiency than that of previous test results was obtained. Experiment SUW09 at Winfrith was carried out at an ambient pressure of 10 atmospheres and resulted in an overall energy yield of 0.9 MJ but not in a higher efficiency (as defined in Reference 6). The reason for the high total yield is that larger fraction of melt mass participated in the explosion (as defined in Reference 6) at high ambient pressure, and at the specific a prototypic melt/water mixing geometry.

These results require further attention in order for the uncertainty depicted in Table I (below) to be narrowed.

Base on the above arguments, in my judgement, the probability of containment failure given a core melt is of the order of 10^{-4}.

1.2 Comparison With other Probabilistic Evaluations

In order to check the subjectivity of the above assessment, a comparison with other similar evaluations is merited. The difficulty however with such a comparison is that substantially new information has been generated continuously, so that the data base of each study is different. Nevertheless the comparison is instructive.

(a) WASH-1400[14] - (1975) - very small data base reflected in the large uncertainty. The probability of containment failure by a steam explosion given a core melt was estimated at 10^{-2} with an uncertanty range of 10^{-1} to 10^{-4}.

(b) German Risk Study[15] (1979) - data base not much larger than WASH-1400. Median value of the probability at 10^{-2}, with a range of 10^{-1} to 10^{-3}.

(c) The Swedish Government Committee on stream explosions[16]: (December 1980) - in vessel steam explosion cannot lead to containment failure.

(d) Theofanous and Saito[10] - (Jan. 1981) "Steam explosion induced containment failure probability is judged essentially incredible, i.e. at least two orders of magnitude lower than 10^{-2} estimate given in WASH-1400", namely order of 10^{-4}.

(e) Sandia National Laboratory:

Corradini and Swenson[13] (June 1981) - Best estimate value 10^{-4} and upper bound 10^{-2}.

Swenson and Corradini[12] (October 1981) - Best estimate is calculated to be two orders of magnitude smaller than WASH-1400, i.e. of the order of 10^{-4} or less for PWR. For BWR both best estimate and upper bound were determined at 1.2×10^{-3}.

Berman, Swenson and Wickett[2] (May 1984) - The authors do not give "best estimate" value. They only claim that their results "span the range of probability from 0 to 1, namely <u>they do not know the answer</u>.

As indicated below (in response to Dr. Ross's second question) I conclude from Reference (2) that the probability of containment failure is between 4.6×10^{-2} ("full range" in Table IV of Reference 2). It may also be concluded from Reference 2 that the authors have erroneously determined the uncertainty to be between 0 and 1, since there is a finite probability (of 0.004) that each of the combinations listed in Table IV will occur. The fact that the uncertainty was expressed as the range within which the probability value lies contributes of course to the resulting wide range.

(f) Henry and Fauske[9] (November 1981) - Steam explosion does not threaten the reactor pressure vessel or the containment.

(g) The U. K. (Gittus) Committee[17] (April 1982): "Despite recent work the uncertainty properly recognized by the Reactor Safety Study and the German Risk Study has not been significantly diminished and we see no reason to adopt a narrower range than that of the Reactor Safety Study, 10^{-1} to 10^{-4}. We could not justify a conditional probability of zero."

(h) Westinghouse's Zion[18] and Sizewell B[19] (1981)studies - There is no threat to the containment from an in-vessel steam explosion.

(i) Briggs[20] (December 1983): The upper limit for the overall probability for containment failure given core melt is of order 10^{-2}.

All these results are summarized in Table I.

TABLE I

SUMMARY OF THE PROBABILITY OF CONTAINMENT FAILURE, GIVEN A CORE MELT

Authors	Reference	Median or Best Estimate	Uncertainty Range
WASH-1400	14	10^{-2}	10^{-1} to 10^{-4}
GRS	15	10^{-2}	10^{-1} to 10^{-3}
Swedish Government	16	0	
Theofanous & Saito	10	0 (10^{-4})	
Corradini & Swenson	13	10^{-4}	Upper bound 10^{-2}
Swenson & Corradini	12	PWR: 0 (10^{-4})	
		BWR: 1.2×10^{-3}	1.2×10^{-3}
Berman et al.	2	Not given	0 to 1
my interpretation of Ref. 2 (see discussion in text)		4.5×10^{-2} to 2.7×10^{-2}	Much narrower than 0 to 1
Henry & Fauske	9	0	
Westinghouse	18,19	0	
Gittus	17	Not given	10^{-1} to 10^{-4}
Briggs	20	Not given	Upper limit 0(10^{-2}) low limit 0
Squarer & Leverett	1	0(10^{-4})	

0 ( ) = order of

The above listed comparison indicates that the engineering judgement we employed in Reference 1 is quite compatible with others and therefore must be regarded as rather objective.

2. Comments on NUREG/CR-3369 "An Uncertainty Study of PWR Steam Explosions" by M. Berman, D. V. Swenson, A. J. Wickett (May 1984).

2.1 General Comments

The first comment on a study that results in a "range of probability from 0 to 1" is of course that it is useless, even though it implies that the remaining uncertainty is large. Our objective should be to narrow the range (from WASH-1400) not to widen it.

Secondly, under the assumptions and engineering judgment of the authors certain flat probability distributions were assumed for five "first set parameters" namely: fraction of core molten, pour diameter, pour length, conversion ratio, and condensed phase volume fraction in slug. using the assumed flat probability distributions and constant values for four other second set parameters, the authors have determined by Monte Carlo sampling of the full range the probability of containment failure. The result shown in case 1 of Table IV ("full width") is 0.046 for a large missile with V > 50 m/s and 0.027 when V > 90 m/s. This therefore must be considered the authors "best estimate" value, and as shown in Table I is somewhat higher than WASH-1400.

The authors then proceed to determine the uncertainty range by selecting different combinations of the parameters at their high, medium and low ranges. The number of such combinations is $3^5 = 243$ of which 23 are listed in Table IV. Because of the assumed flat probability distribution, the probability of each combination is $1/243 \sim 0.004$. Therefore, the probability that "all high" for example could exist is just 0.004, and since Table IV indicates that "All High" would always cause containment failure (p=1), its contribution should be 0.004. Similarly, the contribution of "all low" would be 0, and the contribution of "all Middle" would be nearly zero. When all these contributions are added, the answer would probably be close to the "full width" result of case 1 (i.e. 0.046).

One should determine the 5th and the 95th percentile of the assumed probability distribution of these combinations in order to determine the range of uncertainty. To claim that "the results span the range of probability from 0 to 1" is totally inadequate.

The third general comment concerns the selection of a uniform probability distribution. Certainly the assumed distribution has a very dominant effect on the probability of containment failure, as was demonstrated in Reference (12) by changing the distribution of void fraction from a uniform to triangular distribution. However, it should be recalled that the selection of a certain probability distribution as well as the range of the variables is determined by engineering judgement and therefore cannot be flatly disputed.

The fourth general comment concerns the selection of two set of parameters namely, pour diameter and pour length to yield the mass of melt participating in the explosion as well as the fraction of core molten, and taking the minimum of these two values. The consequence of this treatment is that the importance of each of the parameters F_m d_p, lp, ρ_m is not clearly defined. It also prevents clear definition of the mass of melt participating in the explosion which is the important parameter. For example the range of mass in cases 5, 6, 7 and 9 of Table IV is 0-31, 0-14, 0-28, and 7-56 tonnes respectively. Also, it is not clear why the jet of pour melt was not assumed to contain void.

Lastly, the wide uncertainty exhibited in the Gittus report[17] estimate of the conversion ratio (thus effecting the overall uncertainty of containment failure) does not impact the U. K. reactors to the same extent that U. S. reactors are affected, because of a substantially higher (by two orders of magnitude) core melt frequency in the U. S. reactors. Consequently, a genuine effort must be done in the U. S. to narrow the range of uncertainty. The basis for the large uncertainty in the conversion ratio in the Gittus report is calculations with the SIMMER code. This assessment should be re-evaluated in view of more recent information (e.g. "calibrate" the SIMMER calculations to more efficient and recent tests and compare the results with previous predictions).

2.2 Specific Comments

(a) The assumption that large fraction of a molten core (up to 75%) can be retained by a frozen crust before the crust breaks is a crucial one and should be amenable to analytical solution under some simplifying assumptions. If such an analysis is pursued it could also indicate if the melt pour would be coherent or a slow pour as well as how much water could boil away before the interaction.

(b) I believe that the assumption that the upper limit of the effective pour diameter is the full core diameter (3.4 m), is too high.

(c) The authors assume (P.15) that all the melt in the water at the time of triggering is assumed to be mixed and to participate in the explosion. This assumption requires re-evaluation in light of the recent test results of Winfrith[6]. It is quite plausible that only part of the mixed melt participate in the explosion.

(d) The effect of water subcooling (P. 17) was discussed earlier in response to question 1.

(e) Mixing limitations (P. 17-19) - It is extremely difficult to justify an assumption that up to 94 tonnes of melt can pre-mix with water in view of all other estimates which do not exceed 20 tonnes. This high estimate is partly due to the assumption that the melt mass of cylinderical geometry has a bulk density of $7t/m^3$, which is physically impossible because of the void fraction in both a free falling jet and within the interaction zone[11].

(f) Fraction of water that mixes (P. 19) - The ratio between the masses of water and melt participating in the explosion has a large effect on the conversion efficiency. To be consistent, this should be reflected in the assumption of the fraction of water that mixes, and not only in the mass of the slug.

(g) Conversion ratio - I do not believe that 16% upper bound of the conversion ratio is justified.

(h) Slug composition - This is an extremely important parameter[12], and the assumption that the slug contains no void can not be justified (See Figure 7 on p. 24).

(i) P. 33 - "Since the selection of the sampling scheme used was essentially arbitrary, the reader is cautioned against attributing special significance to any calculated probability number." Does this mean a very low level of confidence in the results?

(j) Results of main calculations - Table IV - The "all middle" range (Case 3) yielding a probability of 10^{-4} is reasonable (although the assumed upper range of melt mass of 7-56 tonnes is too high). The "High" range is generally physically unreasonable and conservative. A comparison between cases 1 ("full width") and 3 ("all middle") indicates that a narrow probability distribution would almost result in no containment failure (10^{-4}). Table V - additional calculations: high heat content (case 29) is reasonable but the melt mass participating is too high.

(k) Other areas of uncertainty - (p.42-48) - It is generally believed that high ambient pressure would suppress the initiation of steam explosions. Uncertainty in head becoming a missile should have been accounted for in the evaluation of the probability since it may preclude containment failure. Geometric effect, in particular cluttered lower plenum should have been accounted for. Section 5.5 "Effect of model parameterization" leaves the impression that perhaps the resulting wide range of uncertainty is due to modeling, parameterization and selected probability distribution and thus does not necessarily reflect the uncertainty in containment failure due to in-vessel steam explosion.

(1) Discussion - (P.50) - effect of scale on conversion ratio: recent test results at Winfrith[6] have demonstrated that, under their test conditions, the conversion ratio (as defined in Reference 6) does not in:rease with scale.

I hope to furnish additional supporting evidence at the Harper's Ferry Meeting.

Sincerely,

David Squarer

David Squarer

cc: A. Briggs
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G. Bankoff
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STEAM EXPLOSION IN PERSPECTIVE

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ABSTRACT

The objective of this paper is to assess the risk that a hypothetical steam explosion may pose to the integrity of the containment of a commercial LWR. In order to achieve this objective, we make use of the results of recent comprehensive studies and published scientific literature. With the improved, but incomplete, understanding of steam explosions gained over the last decade, we reach the conclusion that the probability of containment breach by steam explosion is very much less (a few orders of magnitude) than 10^{-6} per reactor year. With such a low probability, the steam explosion becomes a negligible contributor to the overall nuclear reactor risk to the public safety and is completely overshadowed by other contributors. Accordingly, we suggest that future research concentrate on small scale phenomenological studies to gain a complete understanding of the phenomenon.

INTRODUCTION

The impact of the risk associated with steam explosion on the general safety of commercial light water reactors (LWRs) was first estimated in the Reactor Safety Study (WASH-1400). Due to inadequate understanding of the basics of steam explosions, WASH-1400 had to use conservative assumptions regarding the probability and consequences of steam explosions. Consequently, the conditional probability that a steam explosion would occur and would form a missile (perhaps the vessel head) which would breach the containment was estimated to be 10 .(1) Recent estimates put the probability of a severe core damage accident at 10^{-4} or less per reactor year. Coupling these estimates puts the probability of containment failure due to a steam explosion at 10^{-6} or less per reactor year. The Sandia National Laboratory has recently (3) estimated the conditional probability of a steam explosion to be not 10^{-4} but less than 10^{-4}, for an explosion of the relatively high efficiency of 3%.

This two orders of magnitude reduction in the probability of containment failure due to steam explosion would change the steam explosion from being a dominant risk factor to being one of many contributing factors.

The main objective of this paper is to put the steam explosion issue in a proper perspective, taking into account the findings of many workers since 1974. In order to achieve this objective, we survey briefly the main findings of several recent comprehensive studies on steam explosions. These include the Swedish Government study on steam explosions, the NSAC/EPRI assessment of steam explosions, the Sandia recent support for the lower probability of containment failure by steam explosions, and the NRC critique of existing steam explosion studies. Further, we have critically reviewed some of the existing physical models which have been used to analyze steam explosion experiments and suggest a refinement of the Cho et. al. mixing model (18) which has been used in the evaluation of energetic considerations associated with fine fragmentation of melt-water interaction. Finally, we indicate some of the uncertainties associated with the modeling of steam explosions and suggest additional studies which may resolve these uncertainties.

Our suggestions for future research are influenced by the ultimate contribution of steam explosions to the overall risk to LWRs.

CONDITIONS FOR CONTAINMENT FAILURE BY STEAM-EXPLOSION

In this section we make use of published information on steam explosions, and outline the events and conditions which have to be fulfilled in ord for the containment of a commercial LWR to fail b a steam-explosion.

There are remaining ambiguities, due to inconclusive research results, which will be pointed out as we describe the events. Because of these uncertainties, we attach conditional probabilities to the various events, even though new information may rule out the continuity of the events. The published information which we have used in this section encompasses work done in the U.S.A., U.K., Germany and Italy (ISPRA).

Termination of Core Melt

With reference to Figure 1, the first necessary condition for the occurence of a steam explosion is a severe accident. We assign no numerical value to this probability since it is our intention to focus on the change in perceived risk brought about by better understanding of steam explosions and not to evaluate overall risk. For general reference, it may be noted that the NRC safety goal (2) includes a secondary goal of 10^{-4} per reactor year for severe accidents. We use the letter "p" to indicate this probability

With proper operator action, a severe accident could be terminated. A substantial effort has been invested by the utilities, the industry and the NRC since the TMI incident to understand the causes for severe accidents. This better understanding and operator intervention reduces, in o opinion, the probability for the generation of molten material to 10^{-1} per severe accident.

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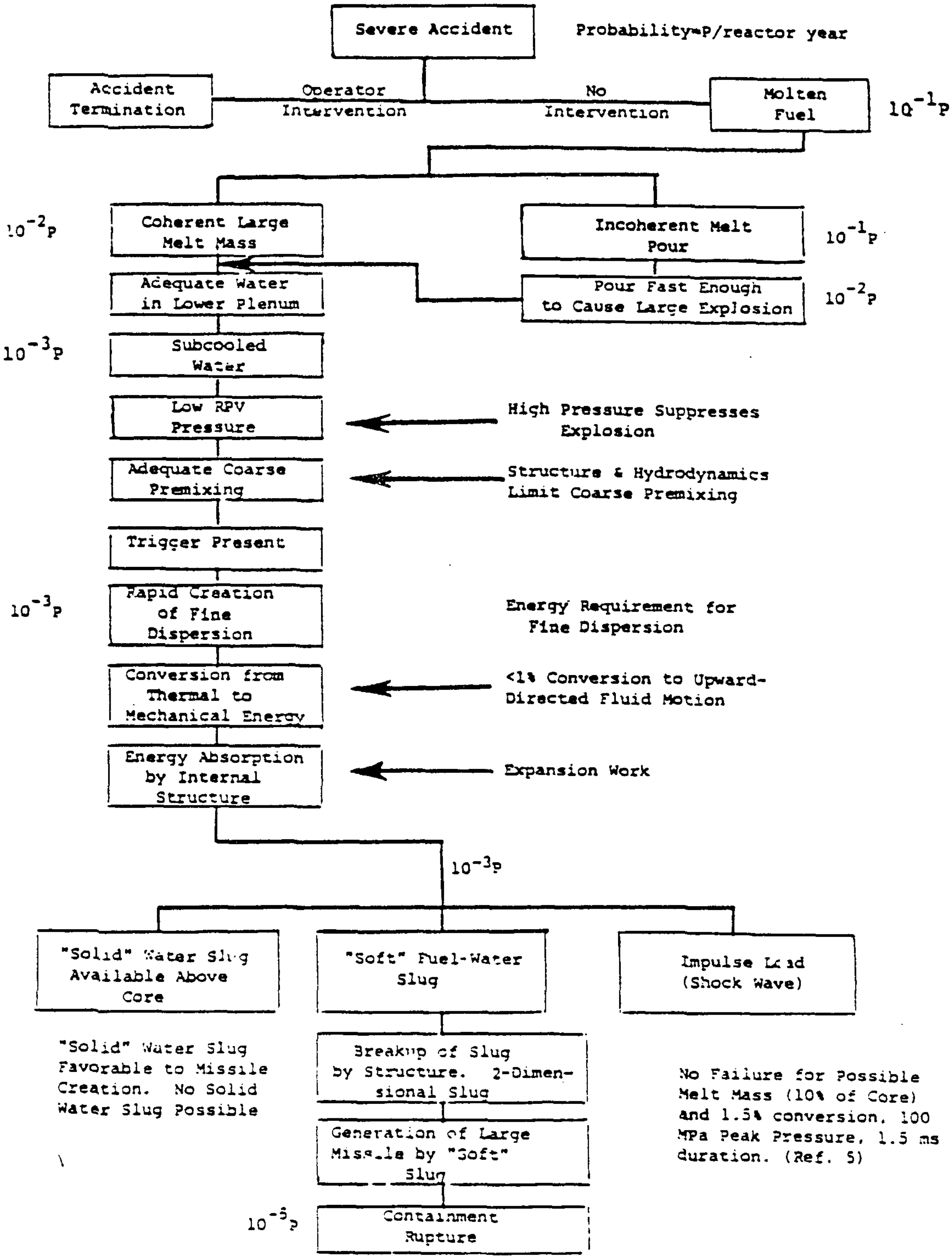
Fig. 1 - Conditions for Containment Failure by In-Vessel Steam Explosion



Assuming P=Probability of a severe accident = 10^{-4}/reactor year according to the NRC Safety Goal[2], overall containment rupture probability = 10^{-9}/reactor year.

Melt Progression

Once the core starts melting, a gradual core slumping will ensue. An incoherent core slumping is predicted by the ANCHAR code (NSAC/EPRI) and by the CORMLT code (EPRI). On the other hand, a scenario has been postulated (4) in which portions of the molten core solidify near the bottom of the core and allow the molten fraction of the core to grow. Others have postulated similar scenarios (5), however, the radiative heat transfer from the molten core will vaporize the water in the RPV lower plenum if the melt is allowed to grow without slumping, thus precluding the possibility of an in-vessel steam explosion. In the MARCH code (6) it was originally assumed that core slumping begins only after a substantial core fraction has become molten, however, a more recent version of the code (7) includes an incoherent core slumping model.

Using the above-mentioned information and our judgment, we believe that the conditional probability of generating a large coherent melt mass, given the presence of a molten fuel, is 10^{-1}. The determination of what mass of molten fuel participates in the interaction is of importance to the present discussion since the energy potentially available for release by a steam explosion is directly proportional to this mass (see Appendix B).

Coolant Conditions

The next mandatory requirement for a steam explosion is the availability of adequate water in the lower head. The melt/water mass ratio significantly affects the yield of the explosion and is determined by the mass of water available at the time of core slumping. It is generally agreed that the coolant has to be subcooled in order for a steam explosion to occur spontaneously (8), although the effect of the liquid subcooling is not clearly understood. (The theoretical model of Hall and Board (10) actually predicts that high subcooling would reduce (or eliminate) the efficiency of the explosion, and some experiments (21) support this condition.) The model suggested by Henry and Fauske (8) did predict the occurence of suppression of the steam explosion data of Long (9) (aluminum and water) based on water subcooling.

In a steam explosion study (11) performed by Sandia (National Laboratory) using 18.7 kg of iron-alumina melt, which was judged to be a good reactor simulant material, the explosion was suppressed in saturated water. An on-going study under EPRI sponsorship (12) in which 2 to 3 kg of corium was dropped or injected into saturated water has not produced steam explosions either. Since it is quite unlikely that subcooled water would be present in the lower head of the RPV, we reduce the conditional probability to 10^{-3} p.

Effect of Pressure

The next parameter in Figure 1 is the ambient pressure, the effect of which is not clearly understood. The model suggested by Henry and Fauske, (13) based on droplet capture and bubble growth arguments, predicts that high ambient pressure will suppress vapor explosions. Their predictions (13) agree with the experimental evidence of vapor explosion in Freon-22 and mineral oil where an explosion was observed at 1 atmosphere but was suppressed at 2.2 atmospheres. Board and Caldarola, (14) on the other hand, believe that the pressure may inhibit only the trigger and tion some of the underlying assumptions of He and Fauske model (e.g. that vapor bubble ca grow until acoustic relief takes place, etc.) Based on the single drop experiments of Nelson with iron oxide and water, a relationship was shown (15) to exist between the trigger strength and the ambient pressure. At pressures higher than ~0.9 MPa, an explosion could be triggered by increasing the trigger strength. A theoretical explanation was given (15) in terms of the effect of pressure on the stability of the vapor film.

Assuming that the Henry and Fauske (13) predictio of the pressure effect is valid, and without identification of a large trigger source, it is plausible that steam explosions in LWRs could be suppressed at pressures higher than ~1.0 MPa. This conclusion is an important one since the probability of a severe accident in which the RPV pressure remains high (TMLB' and small break LOCA is larger than that where the RPV pressure remain low (large break LOCA). Nevertheless, because of the lack of a precise definition of the effect of pressure we did not reduce the probability of steam explosion due to high RPV pressure.

Fuel/Coolant Coarse Mixing

There is general agreement among researchers (see for example references (4), (5), (14), (16), (17) that energetic steam explosions progress in three stages: (1) coarse intermixing or premixing, (2) triggering and (3) coherent propagation or fine dispersion.

Although there is disagreement among researcher regarding the dominant mixing and fragmentation mechanism (i.e., vapor collapse, violent boiling or hydrodynamic breakup) we observe that the film boiling fragmentation model of Henry and Fauske (8) provides a fair assessment of the average size of the fragments produced in the breakdown of the initial coherent mass into a coarsely fragmented mixture of water and molten debris.

Triggering

Before the propagation process can start, a trigg must be available. The interaction can either be triggered spontaneously when entering the water or when hitting the base, or it may require an introduced trigger. There is agreement among researchers that the trigger must be able to produce local contact between the hot and cold liqui by collapsing the vapor blanket, however, the exa mechanism by which this can be accomplished is no clearly understood and typical experiments are randomly triggered either near a water surface or near a structure. Based on the experiments at Sandia (5) with corium and water, which suggest that the variations in corium composition (corium A or [illegible] suppress spontaneous triggering, we could reduce the conditional probability of steam expl[illegible] in LWR by 10^{-1}, however, du to remaining ambiguities, we did not. This is an example of how uncertain it may be to extrapo experimental results with one type of materia actual LWR conditions. It should be pointed ou that in spite of the difficulty of spontaneously triggering an explosion with corium, later information from Sandia (19) using corium A+R with a larger mass (~8 kg) was spontaneously base triggered, however, this does not assure spontaneous triggering under more "prototypic" reactor conditions (i.e., melt composition and mode of delivery).

A steam explosion may also be triggered by an external trigger the size of which must be increased with an increase in the ambient pressure (15). The question is what can constitute an inherent trigger source under a "prototypic" LWR conditions. Some researchers have suggested that a trigger source may be present in the form of a falling object which could impact the lower RPV head and thereby generate a pressure pulse. The magnitude of such a pressure pulse could be estimated at ~10 MPa. It remains, however, to be proven experimentally that a falling object is a viable trigger source at high ambient pressure. We take no credit for the possibility that there may not be an external trigger present.

Propagation and Fine Dispersion

The next phase in the process is a coherent propagation of the explosion. There are several theoretical models (10, 14, 15, 16, 19) which can predict the propagation phase of the explosion, although the fuel fragmentation mechanism behind the propagating shock front (if one exists) is still a matter of debate between researchers. The manner in which heat is transferred behind a shock front (if one exists) directly affects the efficiency of the explosion. From a reactor safety point of view, it is interesting to note that not all steam explosions propagate throughout the mixture. Evidence of propagating explosions does exist (20), and there is also evidence of multiple explosions, (19) however, there is no clear evidence that multiple explosions are more efficient than a single explosion. In order to assess the impact of steam explosion on reactor safety, it is necessary to determine the mass of the fuel/water mixture that participates in the propagation and fine fragmentation. It should also be realized that the propagation takes place in a mixture containing a high vapor fraction, and as such is considerably slower (~100 m/second) than the propagation in a single-phase fluid. The fuel/coolant mass ratio and geometrical constraints also play a role in determining the propagation speed. Obviously, energetic considerations must also be made as to whether a propagation can be sustained.

Assuming that a sufficient trigger is present, the third and final phase of the explosion is that of coherent propagation or fine dispersion. Since this step must occur in a very short time (order of milliseconds), and the particles must be very small (order of a millimeter or less) Henry and Fauske (8) have proposed that the mechanical work required to produce the dispersion may be larger than that available from the explosion itself. They have estimated the fine dispersion work based on the model of Cho, Fauske and Grolmes (18) and showed that it depends critically on the size of the particles existing at the end of the coarse fragmentation step and on the mixing time. When these two models (the film boiling fragmentation model (8) and the fine dispersion work model (18)) were applied to a postulated severe accident in LWR (8), it was calculated that the energy required for rapid mixing far exceeds the available thermal energy within the melt. Corradini and Evans (19) in their analysis of Sandia's steam explosion experiments have checked this energy requirement and found it to be satisfied for experimental explosions involving up to 18.7 kg of iron-alumina melt and water. However, the conditions of these experiments were quite different from those of a hypothetical reactor accident so that the achievement of these experimental explosions does not invalidate the above-mentioned theories (8, 18). On the other hand, Cho, Fauske and Grolmes (18) had to make certain assumptions regarding the mechanism of the fine dispersion step, which have not been substantiated. We have made corresponding calculations assuming somewhat different mechanisms which, in some cases, lead to lower estimates of the dispersive work requirement. (See Appendix A). But we have found no case in which a steam explosion has been achieved that the Cho, Fauske and Grolmes model would have predicted it to be energetically impossible, although there are remaining uncertainties as to what experimental values for mixing time and particle size are to be substituted in the model.

In essence, the Henry and Fauske model (8) predicts that the coarse dispersion resulting from dropping a large (tons) coherent mass of molten debris into water in the lower plenum of the reactor would be so coarse that the energy required to create the fine dispersion necessary for an explosion, according to Cho, Fauske and Grolmes model (18), would exceed that available from the melt. Hence, no explosion would occur, or if one did occur, it would involve only a small fraction of the original coherent mass. On the other hand, explosions involving a larger fraction of an initially smaller coherent mass would be energetically possible, as found experimentally.

A direct conclusion of the above arguments is that only a limited melt mass, possibly in a form of an incoherent melt pour, can participate in the melt/water interaction.

Conversion Ratio

The next important question is: once all the previously described conditions and processes are fulfilled, what fraction of the energy contained within the melt has been expended in producing the explosion, and what fraction remains to do work on the reactor pressure vessel? This is a controversial issue since it is difficult to measure directly the efficiency of the explosion. The experiments at Sandia with 18.7 kg iron/alumina (claimed to be representative of corium) has yielded a kinetic energy conversion efficiency of approximately 1.3% for a water/melt mass ratio larger than 3, (11) whereas three recent experiments with ~8.0 kg of corium and water/melt mass ratios of between 13 and 31 have yielded a kinetic energy conversion efficiency as high as 2.6%. Similar results, scattered between 0 and ~3%, were obtained for UO_2/water by other investigators (21, 22) who used less than a kilogram of UO_2. The Swedish Government committee on steam explosions (17) has presented evidence that steam explosion efficiency is reduced with increasing size of the melt, reaching a maximum of 1.5% at a melt mass of ~10 kg.

The above experimental evidence suggests that only a small fraction of the ideal thermodynamic efficiency calculated by Hicks and Menzies (23) is actually available for doing work on the system. Three other comments on the efficiency are pertinent: (1) the kinetic energy efficiency evaluated by Sandia from their experiments (19) is calculated from the kinetic energy imparted to the water in all directions (lateral and vertical). In one particular example (page 122 of reference 19), out of a kinetic energy efficiency of 1.25%, 0.21% was in the vertical direction and 1.04% was in the lateral direction. Since the postulated damage by steam explosion to the RPV is by means of a "slug" of water moving vertically upward, only the kinetic energy in the <u>vertical</u> direction should be considered for calculating the potential for the creation of a large "missile" (i.e., RPV upper head). This, in turn, implies that based on the relevant experimental evidence to-date, the maximum thermal to vertical mechanism conversion efficiency is less than 1% and probably less than 0.5%, (2) the efficiency was shown (11) to depend on water/melt mass ratio, and was substantially reduced at a mass ratio smaller than 3.0. In a typical postulated reactor accident, this ratio is about 2. *Hence*, it is *implied* that in-vessel steam explosions, if they occur, will be of low efficiency, (3) Sandia has proposed (11, 19) that two "efficiencies" be considered, one due to kinetic energy and a second due to the pressurization of the chamber air. The two efficiencies were combined to yield a total "mechanical utilization" energy. However, the authors (11, 19) were careful in pointing out that primarily the kinetic energy is available to do work on the RPV. In fact, the pressurization of the RPV is absorbed by the RPV walls, and as long as the volume of the pressurized gas cannot expand (i.e., an intact vessel), no work can be done by the gas. Consequently, the "stored energy" efficiency has no impact on the conversion of thermal to kinetic energy and only the kinetic energy efficiency should be considered as a potential for producing a "missile". Finally, we translate this efficiency into an expected available energy when 4700 kg of melt (estimated amount of melt that can mix in 3.0m of water in the lower head (19)) pours into water (see Appendix B).

The thermal energy contained within the melt is approximately 6600 MJ, of which 1% is 66 MJ. The theoretical <u>maximum</u> thermodynamic efficiency for a coolant expansion to the reactor volume (23, 5) yields approximately 830 MJ for equal masses of fuel and coolant and approximately 300 MJ for a coolant/fuel mass ratio of 2.0. Only under the assumption that the melt/coolant mixture expands isentropically and the melt and coolant are in thermal equilibrium, or that the coolant expands to atmospheric pressure, does the theoretical thermodynamic efficiency increase approximately three fold. However, all experimental evidence to-date indicates that the process is quite inefficient because heat transfer is not completed behind the propagating shock front (if one exists at all) and efficiencies of the order of 1% of the thermal energy do result. The low efficiency is expected as a result of the low propagation velocity (due to high vapor fraction in the mixture), multidimensional effects and other causes.

<u>Expansion Work, Effect of Internal Structure and RPV Loading</u>

The explosion work estimated above may cause damage to the RPV either by the pressure pulse generated by the explosion or by accelerating a liquid slug against the RPV upper head as suggested in WASH-1400. However, before this work is transmitted to the RPV, part of it is absorbed by the remaining internal structure (plastic deformation). An estimate of the absorbed energy requires a sophisticated analysis. Both types of loadings (pressure pulse and liquid slug loading) will be dampened by the deforming structure which would reduce the expansion work. An estimate of the energy absorbed by the structure would depend on how much of the structure remains. However, an absorption of 75% of the expansion work is not unreasonable (3).

The remaining (~25%) expansion work would exert an impulse load on the RPV and may accelerate a slug. The potential for an impulse load to fail the lower plenum was estimated by Corradini and Swenson (5) based on an impulse peak pressure and duration (calculated by the CSQ computer code). For a "best estimate" load they assumed that 10% of the core interacts with 10 tons of water at a conversion efficiency of 1.5% resulting in 300 MJ of work and a pressure impulse of 100 MPa and 1.5 ms duration (see Appendix B). No failure of the lower plenum was calculated for this case by quite a substantial margin. A conservative explosion work of 3000 MJ resulting in a pulse of 400 MPa and duration of 3 ms did result in a lower plenum failure. However, such work was the result of mixing 40% of the core (~40 tons) with 20 tons of water at a theoretical efficiency of ~16% (see Appendix B). Since our previous arguments precluded the participation of a large melt mass, we should not expect more than about 10 tons of melt to interact with water. Therefore, we conclude that the impulse load probably would not affect the RPV (unless it could fail the instrumentation tube at the lower head of the RPV, which is unlikely). Furthermore, since the lower head of the RPV is expected to fail locally (by melting) shortly after the melt accumulates in the lower head, the failure of the lower head by impulse loading will have little effect on the safety of LWR (failure by impulse loading may relieve part of the expansion work which would otherwise be available for accelerating a slug).

<u>Slug Characteristics and "Missile" Generation</u>

Acceleration of a "solid" water slug is the next step in Figure 1 for transmitting the expansion work to the upper head. There is little doubt that when molten fuel drops into a water pool, only a voided water (or water/fuel) "slug" could exist above the core.

A "soft", i.e. voided, water/fuel slug may conservatively be assumed on account of the water/fuel mixture which remains after the explosion. However, if the explosion is triggered before the melt reaches the lower head of the RPV (e.g. at the water surface) very little mass of water would be available for a slug. Furthermore, even if the explosion is triggered at [illegible] (i.e. at the lower head) the fuel/water mixture is expected to be highly voided ($\alpha \simeq 0.70-0.90$)(24). If we also consider the fact that the structure which remains within the RPV after the explosion would break any slug (voided or unvoided), we conclude that only a "soft", voided, incoherent slug could exist at the point of impact on the vessel upper head.

In order to estimate whether such a hypothetical slug could cause a large "missile" as suggested by the WASH-1400 study (1) we refer to the study of Swenson and Corradini (3) and note that the potential to generate a large "missile" is most sensitive to the void fraction within the slug. For the conditions evaluated in reference 3, no large missile was produced in a PWR for a void fraction of 0.25 to 0.50 whereas for a triangular void fraction distribution of 0.0-0.25-0.50, 8 (out of 10,000 trials) large "missiles" were calculated to be produced under the same conditions. Consequently, when realistic void fraction, slug breakup condition, and trigger location are considered, no large missile should be generated under considerably higher energies than those considered in reference 3 (for a PWR 750 MJ mean explosion energy resulting in a 200 MJ mean impact energy yielded no large missile in 10,000 trials, whereas for a BWR, mean explosion energy of 1450 MJ resulting in 350 MJ mean impact energy resulted in 12 large missiles out of 10,000 trials). As support for this conclusion, we notice that the LANL calculation of the Zion/Indian Point study predicts no failure for a two-dimensional slug generated by 1200 MJ (25).

To further amplify the (energy) absorption capability of the RPV, we note that a considerably higher energy absorption capability was estimated by others (16). "Expected" values of 830 MJ and 1400 MJ for PWR and BWR respectively were calculated based on the strain distribution within the RPV structure. Similar conclusions were reached by the Swedish Government Committee on Steam Explosion (17). We, therefore, believe that the generation of a "large missile" by a few hundreds MJ as calculated by Sandia (3,5) is overly conservative. Small missiles (i.e. control rod) may be generated more readily, however, their energy would be absorbed by the missile shield without causing any damage (5). Finally, it should be realized that internal structure within the containment will absorb part of the energy of a large "missile" before containment failure.

Probability of Containment Failure

We now attempt to estimate the probability of containment failure based on the "event tree" of Figure 1. "Lumping" all the above-mentioned factors, namely: conversion ratio of 1% or smaller as a result of thermodynamically inefficient process, an expansion work to a constant high pressure volume, the energy absorption capability of the internal structure, the inconsequential impulse load, the unavailability of a "solid" water slug above the core, the very low bulk modulus of a voided slug, the high void fraction of the fuel/water slug, if any, the break-up of a slug by the remaining structure, the dispersive nature of a two-dimensional slug, the energy absorption capability of the RPV, of internal structure within the containment, and of the containment itself, we reach the conclusion that if a containment failure by a large "missile" were to occur at all, its probability should be reduced by at least two additional orders of magnitude. That is, given a severe accident, we estimate that the conditional probability of containment rupture by an in-vessel steam explosion is about 10^{-5}. This value compares with an estimated conditional probability of 10^{-2} in WASH-1400 (1) and estimated conditional probabilities of 10^{-4} for a PWR and $\sim 10^{-3}$ for a BWR by Sandia (3).

The possibility of containment rupture due to an ex-vessel steam explosion has also been considered. The conditional probability of such an event is believed to be insignificant due in part to the same arguments applied above to the in-vessel steam explosion. Where those arguments do not apply, there are others which have the same impact. Specifically, the debris must be released from the vessel in a non-dispersive manner and it must be coherent (not like a stream or series of drops), an adequate quantity of water must be present under the vessel, coarse mixing, triggering and fine dispersion must occur, the resultant pressure pulse must find its way through some tortuous paths to the containment itself and must be of sufficient strength to rupture it, or a large "missile" must be energetic enough to break the containment. The details of this process depend on containment design, which varies from one type of plant to another, but the conclusion is the same, namely that so many conditions and occurences must coincide that the conditional probability of a containment damaging ex-vessel steam explosion is insignificant. Similar conclusions have been drawn by others (5). Our judgement, similar to those of others (5, 16, 17), is that the probability of containment failure by an ex-vessel steam explosion is small, of the order of 10^{-5} per severe accident.

CONCLUSIONS

Based on a methodical evaluation of events and conditions which may lead to a steam explosion in an LWR, and using results from the published scientific literature, we conclude that the conditional probability of containment breach by steam explosion is of the order of 10^{-5} per severe accident, or 10^{-9} per reactor year, assuming a safety goal probability of 10^{-4} per reactor year for a severe accident. This compares with a probability of 10^{-2} per severe accident assumed in WASH-1400 (1) and with a probability of 10^{-3} to 10^{-4} per severe accident calculated by Sandia (3). With such a low probability, the steam explosion becomes a negligible contributor to the overall risk to an LWR and is completely overshadowed by other contributors.

Suggestions for Future Work

A long list of suggested future work on steam explosions was presented by Board and Caldarola (14) in 1977. Many of the studies they have suggested were since carried out by Sandia and by others (see ASME, HTD-Vol. 19, 1981, on Fuel-Coolant Interaction (9,15,21,22)). In spite of the additional research performed prior to 1982, a long list of recommended research topics was put forward in an Expert Review Meeting on Steam Explosion held in May, 1982 at the NRC.

We believe that the following small scale phenomenological studies are justified as confirmation research, giving additional assurance to the conclusions stated above:

(1) Acceleration and "coupling efficiency" (as a vehicle for energy transfer) of a voided two-

dimensional "slug" accelerated through internal structure.

(2) Energy considerations in mixing hot and cold liquids to verify and quantify existing mixing models such as those developed by Cho, Fauske and Grolmes (18), or a modification thereof (see Appendix A).

(3) Triggering of steam explosions at high pressure, in saturated water, using "realistic" trigger sources such as falling objects.

(4) Effect of confinement on steam explosion efficiency.

(5) Verification of computer code predictions of an impulsive load.

We do not believe that large scale (i.e. hundreds of kilograms of melt) steam explosion experiments are justified at this time because of: (a) the very low contribution of steam explosion to the public risk, (b) high cost, and (c) the small probability of gaining final resolution of the issues by such experiments. Similar conclusions were drawn by three out of four experts (26) who considered this issue.

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APPENDIX A - Mixing Energy

Cho, Fauske and Grolmes (18) have calculated the energy necessary to break up a fuel volume into small particles and mix them with equal volume of coolant. It is important to know this energy, for if the result yields a substantial fraction of the thermal energy within the fuel, the interaction and therefore steam explosion would not take place. We checked the sensitivity of the above model to the assumptions made with regard to the geometry of the mixed volumes. We consider a sphere of one liquid of radius R_1 and volume V_1 surrounded by a spherical shell of another liquid of radius R_2 and volume V_2. We assume that a droplet of the inner liquid moves radially outward from its initial position at r_1 to its final position at r_2 such that a uniformly mixed sphere V_2 results. The drag work required to move the particles a distance r_2-r_1 in time t is:

$$dW_D=N\cdot\tfrac{1}{2}\rho C_D Au^2(r_2-r_1) \qquad \text{(A-1)}$$

where W_D is drag work, ρ is fluid density, C_D is the particle drag coefficient, u is the particle velocity, (r_2-r_1) is the distance traveled by particles, A is the particle area, and N is the number of particles initially in shell of radius r_1. We substitute now $N=4\pi r_1^2 n dr_1$ (n=number of particles per unit volume in the liquid), $A=\pi r_p^2$, $u=(r_2-r_1)/t$, $(r_1/R_1)^3=(r_2/R_2)^3$ and obtain,

$$W_D=2\pi^2 r_p^2 n\rho C_D/t^2 (R_2/R_1-1)^3\int_0^{R_1} r_1^5 dr \qquad \text{(A-2)}$$

Integrating and substituting $R_1^3=3V_1/4\pi$ and $1/n=4/3\cdot\pi r_p^3$ yields,

$$W_D=\frac{9\ C_D\rho V_1^2}{64\ \pi r_p t^2}\left(\frac{R_2}{R_1}-1\right)^3 \qquad \text{(A-3)}$$

We now consider the case of equal volume mixing i.e., $V_2=2V_1$, and $R_2/R_1=2^{1/3}=1.260$, and compare this result with that of Cho, Fauske and Grolmes (CFG) (18). For <u>one step mixing</u> the ratio of our estimate to that of CFG is:

$$\frac{W_D(\text{this work})}{W(\text{CFG})}=\frac{9}{64}\ \frac{C_D\rho V_1^2\cdot 0.26^3}{\pi r_p t^2}$$

$$\frac{8t^2 r_p}{3C_D\rho_f V_1^2}=0.0021 \qquad \text{(A-4)}$$

For the CFG <u>progressive</u> mixing model where W_p is given by (18)

$$W_p(\text{CFG})=1.81C_D\rho V_1\left(\frac{V_1^{2/3}}{t^2}\right)\left(1-\frac{r_p^2}{V_1^{2/3}}\right)\ln\left(\frac{V_1^{1/3}}{r_p}\right) \qquad \text{(A-5)}$$

and assuming $r_p^2/V_1^{2/3}<<1$ there results,

$$\frac{W_D(\text{this work})}{W_p(\text{CFG})}=4.35\times10^{-4}\ X/\ln X \qquad \text{(A-6)}$$

where $X=V_1^{1/3}/r_p$. Substituting some numerical values for X, equation (A-6) yields 1.89×10^{-3}, 9.44×10^{-3}, 6.29×10^{-2}, 0.472 and 1.0 for $X=10, 100, 1000, 10^4$ and 2.3×10^4 respectively. For X=1000 we obtain $R_1/r_p=620$ and for $X=10^4$, $R_1/r_p=6200$. Consequently, this model yields a lower energy estimate than reference (18) as long as $X=V_1^{1/3}/r_p<2.3\times10^4$ or $R_1/r_1<14267$. It is apparent that it is important to know the true values of C_D and particularly t. These are not well known quantities.

The energy required to overcome the surface energy when creating smaller particles by subdividing a large particle is

$$W_s=3V_1\sigma/r_p \qquad \text{(A-7)}$$

where σ is the surface tension. The ratio of the drag work calculated above by equation (A-3) to the surface energy work W_s, in an equal volume mixing $(R_2/R_1=1.26)$ is

$$\frac{W_D}{W_s}=2.62\times10^{-4}\ \frac{C_D\rho V_1}{t^2\sigma} \qquad \text{(A-8)}$$

Substituting into equation (A-8) $C_D=1$, $\rho=1\text{gm/cm}^3$, $\sigma=0.5\text{N/m}$, $V_1=1\text{cm}^3$, t=0.01 sec yields, $W_D/W_s=5.25\times10^{-3}$ whereas for $V_1=3\times10^4\text{cm}^3$ and t=0.001 sec, $W_D/W_s=1.5\times10^4$. It appears therefore that under typical LWR conditions, drag work will dominate as in the CFG model.

Kinetic energy consideration yields,

$$W_{KE}=\int_0^{R_1}\frac{4\pi r_1^2\rho_p}{2}\left(\frac{r_2-r_1}{t}\right)^2 dr_1=$$

$$\frac{2\pi\rho_p}{5t^2}\left(\frac{R_2}{R_1}-1\right)^2 R_1^5 \qquad \text{(A-9)}$$

The ratio between drag work (equation A-3) and kinetic energy work (equation A-9) for equal volume mixing is

$$\frac{W_D}{W_{KE}}=1.46\ \frac{\rho_f}{\rho_p}\ C_D\ \frac{R_1}{r_p} \qquad \text{(A-10)}$$

For $\rho_f/\rho_p=1/8$ and $C_D=1$, equation (A-10) yields $W_D/W_{KE}=0.183\ R_1/r_p$ which again implies that the drag work will dominate kinetic energy requirements as in the CFG model.

APPENDIX B - Explosion Energy of the Melt

The total energy contained within the melt is estimated between (5)

$$Q=600J/kg\text{-}°k(2700-300)°k=1.44MJ/kg$$

and, (16)

$$Q=500J/kg\text{-}°k(3000-300)°k=1.35MJ/kg.$$

Consequently, for 10% of a 100 ton (PWR core $Q=1.4\times10^4$ MJ, whereas for 10% of a 200 ton core (BWR) $Q=2.8\times10^4$ MJ. A conversion ratio of 1% yields 140MJ and 280MJ for PWR and BWR respectively. (For the somewhat larger core used in reference 3, the 1% conversion ratio yields 183MJ and 350MJ for PWR and BWR respectively). These energy levels are considerably lower than the mean peak explosion energies of 750MJ and 1450MJ used in reference 3 to derive the containment failure probability of 10^{-4} for PWR and 1.2×10^{-3} for BWR.

Furthermore, the conservative expansion work of 3000MJ used in reference 5 for PWR implies an efficiency of 16.5% in a 130 ton core. However, this 3000MJ is claimed to result from mixing 40% of the core with 20 tons of water, for which the theoretical thermodynamic expansion to the reactor vessel volume is approximately 8%.(5)

We suggest that more realistic values for the explosion energies be used in future probabilistic studies which yield the "bottom line" for containment failure.

NS-RAT-DS-84-006

TO: Steam Explosion Committee Members

FROM: D. Squarer

SUBJECT: COMMENTS ON SANDIA'S PROPOSED EXPERIMENTS

In Dr. Ross' letter of August 24, 1984 two questions related to SNL suggested program were asked:

(a) Whether these experiments or other experiments and/or analysis will contribute to resolution of residual issues and further reduction of uncertainty.

(b) Is pre-experiment analysis or experimental procedures, which if followed will maximize the usefulness and effectiveness of the tests, necessary?

I reviewed briefly the suggested SNL program as documented by Mr. Berman et al (October 1984) and offer the following comments based on these documents and on the discussions held at the New Orleans review meeting on June 4 and 5, 1984.

1. The documents furnished by SNL summarize a substantial effort in an attempt to resolve a difficult question and the authors are to be commended.

2. The main objective is to address the alpha failure mode. Yet only the SEALS 3 addresses this objective directly with minimal uncertainty, and is therefore worth pursuing. The uncertainty involved in SEALS 1 and SEALS 2 measurements would be too large to yield a definite determination of the alpha failure mode.

3. Since the work potential in SEALS 1 and 2 would be estimated as in the EXO-FITS tests, and the main diagnostic tools would be photography, i.e., looking at the surface of the propelled slug, high certainty with respect to the slug mass and void fractions of the fuel and coolant would persist. This would lead to high uncertainty in the work yield and conversion ratio which should be defined apriori. If the expected uncertainty is too high, then SEALS 1 and 2 should be skipped. One way to reduce this uncertainty is to leave the crucible in place, close the melt release doors explosively, and use the crucible assembly to measure directly the work delivered by the slug. However, I believe that SEALS 1 and 2 should be skipped in favor of SEALS 3.

4. It does not appear from the SEALS portion of the SNL research plan that the coarse mixing issue would be resolved unless of course no fragmentation takes place.

5. In the general description of the experiments it was suggested that actual reactor geometry and internal structure would be employed, yet only SEALS 3 <u>approaches</u> the prototypic conditions although as proposed would include only simulation of the lower core support plate. There is much more structure (including a downcomer) than that in the prototype.

6. It is not clear to me that the SEALS facility has to be designed for 2000 kg tests. The anticipated SEALS results may be obtained with 1000 kg tests

at a lesser cost and complication, allowing for more tests with a wider statistical base. Furthermore, the chance of success with 1000 kg thermite is considerably higher than 2000 kg thermite and would involve less developmental work.

7. I have expressed my reservation on early occasions as to the merit of using iron-alumina thermite to simulate corium. I reiterate my reservation and suggest that either corium thermite be used or convincing evidence be given that the behavior of the two materials is similar when interacting with water. On the same issue (relates to question (b) above) characterization of the resulting melt after the thermite reaction is necessary in particular with respect to the amount of dissolved gases at different holding times, powder preheating effect, and molten jet characteristics with respect to prefragmentation.

8. If we are to learn the effect of scale on FCI identical experiments at different scales should be performed. This was skillfully performed at Winfrith with the 0.5 kg and 24 kg tests although both the contact and mixing modes were a prototypic. It is not clear from the SNL research program that this is indeed intended rather, the suggested program would address the effect of various parameters (water depth, water temperature, mass, structure, etc.) on a small statistical base. What I am suggesting is that prototypical conditions be selected and adhered to, and repeated tests be performed under an exact scaled up conditions. In the same context the 20 kg FITS and EXO-FITS tests should be repeated under the same exact selected conditions. If a good common base for scale-up is not preserved the wrong conclusions on the effect of scale will be drawn.

9. A few comments with respect to the experimental details and the proposed parameters to be tested: The trigger time in those tests where trigger is required should be determined very carefully, for if a trigger is used early when the material is premixed the results would not relate to prototypic conditions. Based on corium/water experiments at ANL I believe that a selection of one water depth is adequate. On the other hand, I believe that the jet diameter should vary in the experiments and the rational for selecting 85 cm jet diameter be justified. The suggested fragmentation and dropping of the crucible lower "doors" is disturbing and a prototypic. It would change the mixing characteristics and would introduce another random variable.

   SNL test RC-2 has shown that the conversion ratio cannot be determined with reasonable accuracy. It is not clear how SEALS 2 could do that.

10. The SHIP (high pressure single drop facility) test objective is reasonable and so is the ELVIS and may yield more meaningful results than SEALS and FITSX.

11. The program should receive low visibility until a complete analysis of the work potential is performed and the uncertainty and the implications of the results are determined. Otherwise, the program may yield potentially damaging outcome.

12. Pre-experimental analysis is useful for the design of the experiments (as was done by Berman et al in Appendix 1 to SEALS, October 1984). However,

since the FCI research program is a learning process and the proposed instrumentations for the SEALS facility are inadequate to validate some of the present models, it is not mandatory that pre-experiment analysis be submitted. Of course if such predictions are made they may serve as a guide for the experimental design, however this has already been done.

Post experimental analysis would of course yield the main results.

Pre-experimental procedure may be very useful and some of them are already in progress according to the SNL program plan. This includes the development of instrumentations and the measuring of the thermal energy of the melt.

I believe that the development of a method to measure directly the conversion ratio should be pursued for SEALS 1 and 2 if these tests are performed and that the anticipated uncertainty in the conversion ratio be determined.

Melt and molten jet characteristics should be determined in a scoping "dry" study at increasing scales, not necessarily when interacting with water, so that x-ray and γ-densitometer an be employed to characterize the falling jet. Also, the preparation for SEALS 3 can proceed with "dry" thermite melt preparation without pursuing SEALS 1 and 2.

More attention should be given to a wider statistical base in order to determine the probability of containment failure by the alpha mode.

Conclusions

Large scale experiments may be useful if done in a version of the proposed SEALS 3 facility under prototypic geometry and conditions (i.e. with corium). Smaller melt mass may be adequate. SEALS 1 and 2 may be skipped. SHIP and ELVIS are generally reasonable. Low visibility for the program is suggested until a complete analysis is performed.

TO: D. Ross, Deputy Director RES

FROM: T. G. Theofanous, Consultant

RE: Probability of α-failure in LWRS

1. Overall Approach.

We will develop the position that the α-mode containment failure in LWRS postulated to suffer any core melt scenario is physically unreasonable. Quantitatively, this wording implies a probability range below 10^{-6}. However, with due regard to the extreme nature of the phenomena considered, we believe that on a scale commensurate with the quantification of the so-called front-end of a PRA, a probability range below 10^{-4} would be more appropriate. Furthermore, we expect that it will be possible, in the near future, to show that such failure can be ruled out on physical grounds.

Our approach is based on showing substantial margins between upper bounds of explosive energy release (principally based on physical limitations in quantities of interacting materials) and lower bounds of missile energetics with containment challenge potential. Additional margins due to slug energetics and in-vessel structural effects are also envisaged. All energetic considerations will be confined to a low pressure scenario. For the high pressure scenario large-scale steam explosions may be ruled out as demonstrated in the next section.

2. The High Pressure Scenario.

Up until now this scenario has been considered with regard to the suppressed explosivity of reactor materials at elevated pressures as predicted by some of the available theories. Supporting experimental evidence is also cited, however, critics claim that it is only a matter of trigger availability and indeed that conversion ratios can be expected to increase with pressure. We will show how the issue can be sidestepped revealing a much clearer outcome.

The attached paper shows that high pressure steam can provide a close thermal coupling of all primary system masses accessible to it. It is predicted that core heat-up at high pressure will proceed with peak temperature gradients of less than ~150°K. Given that all steel (both carbon as well as stainless) loses its strength at ~1,000 to 1150°K, a core temperature of ~1300°K implies widespread structural failures. That is, either (a) the primary system boundary fails and the sequence reverts to a low pressure one, or (b) the core barrel creep-fails dropping the whole assembly of the bottom support casting (sometimes also referred to as forging), flow mixer plate, and lower core plate together with the whole core into the lower plenum, expelling all water from this region, and thus precluding any future large scale explosions. Considering the relative positions of these structural failures to the source of core heat, and the respective heat capacities, one can in fact argue that the core barrel failure will precede primary system boundary failure. Furthermore, the small amount of water possibly retained between the flow mixer plate and the bottom support casting will boil off upon primary boundary failure (i.e., depressurization and flashing) as well as by radiative and conductive heating through the lower core plate and support columns.

Given that the high pressure sequences dominate in terms of initiator frequencies yielding core melt, their neutralization by excluding early containment failure, per above, is deemed particularly important. I believe, therefore, that it would be extremely worthwhile to further pursue all details of high pressure heat-up scenarios.

3. Quantities of Materials Involved in Explosion.

At low pressure the core heat-up is highly incoherent and one would expect a rather small accumulation of molten material at the time of lower core plate failure. Unfortunately, the available models are too simplistic, in relation to the complex physical processes that make up the meltdown/relocation pattern, for quantification of the degree of incoherence. However, in my opinion such quantification is not only within immediate reach, but also it has the promise to provide the final nail in burying the α-failure mode concern. For now we will limit our discussions to certain qualitative, yet extremely important, features of lower core plate failure and associated melt/water mixing behavior.

Let us envision a fuel/clad melting and relocation pattern that yields substantial unheated plugs within the assembly nozzle/lower core plate region. Such a situation is clearly possible and has to be involved as a necessary initial condition to an energetically significant steam explosion. It should also be clear that the formation of such unheated plugs will maximize the quantities of melt available at the time of lower core plate failure. Given the distributed support provided by the large number of core support columns, such failure will have to occur by melt through. Indeed, catastrophic failure of the plate as a whole must obviously be ruled out. The size of the pour, therefore, will be limited by the radial core melt coherence pattern which should closely correspond to the radial power distribution. The central 30% of the core's radial dimension provides a good measure of this, which is also consistent with current heat-up calculations. This corresponds to a pour diameter of ~1 m.

Certain other possibilities yielding unequivocally non-explosive behavior should be mentioned. They involve heat-up and failure of the core barrel and a scenario similar to that discussed in section 2 or ablation of the relatively thin lower core plate, which finds itself in direct contact with the heated core rubble, and discharge of a largely solid fuel debris upon the flow distributor plate. Similarly, the distributor plate would be expected to ablate allowing the release of a largely solid debris mass (rubble) upon the bottom support casting, and through the large openings in it, upon the lower head. In lieu of quantitative support of such scenarios at this time a melt pour of ~1 m in diameter will be considered.

A free-fall distance of $\sim$1 m is available before the jet encounters the bottom support casting. If an explosion is to occur it will upon this impact and it will, therefore, involve a maximum of $\sim$1 m^3 of melt. This is $\sim$7 tons and at 1.2 GJ/ton its thermal energy content is estimated at $\sim$8,400 MJ. Furthermore, the hydrodynamic breakup of such a jet would be essentially nil (see our NED paper, ref [8] of report) and employing the coupling length of 10 cm we estimate a participating (in the explosion) mass of only $\sim$0.3 m^3. That is, $\sim$2.1 tons and a thermal energy content of $\sim$2,520 MJ would represent mechanistically more reasonable limits. We need not be concerned about subsequent melt releases into the lower plenum since the initial explosion will drive all water out from the lower plenum area. It is interesting that mechanistically the mixing limitations due to steaming do not even arise. However, the concept is useful as still another margin to a hypothetical largely prefragmented melt.

4. Conversion Ratio.

As we explained previously (NED paper), conversion ratios significantly lower than ideal cannot be adequately justified at this time. The Sandia experiments seem to yield increasingly higher values. Considering the uncertainties in such estimates, and the scale effects that would yield longer contact times, and higher pressures, and the need to maintain a clearly conservative posture in this assessment, we will remain with our original suggestion of 15%. With the quantities determined in the previous section, the implied mechanical energy release is $\sim$380 MJ. For the extreme non-mechanistic case that all the melt in the jet is assumed to interact, a release of 1,260 MJ is obtained.

5. Structural Effects.

Following up with the explosion scenario of section ~~4~~ 3, we can now visualize the expansion/structural mechanics. With $\sim$1 m^3 of the core in the explosion region the ~ 16 m^3 remaining upon the lower core plate will be accelerated upwards. Due to the relatively high inertia of this mass downward venting through the flow passages in the bottom support casting and perhaps even shearing of this casting from the core barrel would be expected. A significant energy partition away from the slug is implied. The core slug ~1.3 to 2 m in depth (depending upon the fraction being in the molten state) will accelerate upwards through a distance of ~2 to 3 m till it impacts the upper core plate that constitutes the lower boundary of the UIS, which for a low pressure scenario should be reasonably intact. This structure will fail at a pressure load of 7,100 psi which is considerably lower than the pressure required to fail the head studs (~8,700 bar). Simplistic analysis indicates the potential to dissipate ~ 1,000 MJ in crashing. This, together with the 270 MJ required to fail the head studs, and to generate a 90 m/s missile, clearly establish the margins of containment failure.

6. Conclusions.

Mechanistic evaluation of the high pressure scenario indicates that the α-failure mode can be neglected. For low pressure meltdowns, explosion limits at 380 MJ and 1260 MJ are projected as best estimate and highly conservative values respectively. Based on simple structural failure consideration[5] as well as detailed previous evaluations of dynamic loading, dissipation mechanisms, and structural failure thresholds, such energy levels may be safely considered of no importance to challenging containment integrity. Moreover, the alternative scenarios of creep-failure of the core barrel prior to lower core plate melt-through and that of a lower core plate melt-through by a largely solid core rubble mass are identified here as potentially likely paths of evaluation. That is, detailed core heat-up analysis is identified as the most promising area to further establish the unlikeliness of the α-failure.

TO: D. Ross, Deputy Director RES

FROM: T. G. Theofanous, Consultant

RE: Review comments on SAND83-1483

I welcome the opportunity to comment on the above referenced report. It addresses a matter of major significance regarding our perception of risk from commercial nuclear power generation, yet I fail to discern the quality effort that would be expected of such a task. I strongly disagree with the methodology as well as with the conclusions of this report. Furthermore, I believe that subject report in no way advances the state of the art in dealing with either the mechanistic or the probabilistic (uncertainty) aspects of the problem. In the following I will attempt to zero-in to the key consideration that lead me to these criticisms.

Within the existing state of the art in dealing with slug impact dynamics and missile generation, the conclusions of this report are driven by the size range of the explosion considered to be within the realm of possibility. That is, if we agree with the authors that the melt pour mass that is submerged in water at the time of explosion will vary from 56 to 94 tons (i.e., from 45 to 75% of the total core mass), <u>and</u> that all this mass will participate, the conclusion of failure probability near 1 becomes a matter of simple arithmetic.

Energy requirement for a 90 m/s upper head missile:

Missile kinetic energy	121	MJ
Energy to fracture bolts	150	MJ
Energy dissipation in UIS	250	MJ
Total	521	MJ

Thermal energy potentially available:

56 to 94 tons x 1.2 GJ/ton	67 to 112 GJ

Mechanical energy potentially released:

for conversion ratio of 3.3%	2,210 to 3,700 MJ
for conversion ratio of 5%	3,350 to 5,600 MJ

Thus, even with the partitioning of a good portion of this energy into a failing lower head we are left with ample energy to assure containment failure. This is the whole essence of the report and the plethora of parameter probability distributions and their Monte Carlo samplings serve no useful purpose.

As a corollary of the above, a melt participation of 28 tons would be required to begin approaching containment failure energetics. This is equivalent to 4 m^3 of melt while 8 and 13 m^3 would correspond to the 56-94 ton range discussed above. Is the fine scale, coherent, mixing of such large quantities of melt with water in the lower plenum possible? The authors of the report, clearly, think so. Their procedure is to reject all aspects of all previous work that sought to address this topic, so that they can conclude that "at present, evidence does not exist to allow an upper limit to be imposed on the mass of melt mixed that is less than all the available melt." I find no merit in such a wholesale rejection and I certainly dispute the resulting conclusions. Let me explain.

Mixing limitations due to intense steaming and associated fluidization phenomena (Henry-Fauske, Corradini) are discarded because available models are one-dimensional and depict only a process at steady-state. Although I agree with these limitations, I believe that these models still capture the essence of the physical behavior, in ~~by~~ depicting severe limitations in water availability within the bulk of a prefragmented large scale melt assumed to "rain" through a large pool of water. Two-dimensional, transient calculations will be soon available (Purdue, Northwestern, etc.) to further substantiate these results.

Mixing limitations due to geometric constraints on hydrodynamic breakup (Theofanous-Saito) were not discussed in the report, although the appropriate reference is cited and repeatedly misquoted. For example on p. 5, "Similar arguments have been advanced . . .," the concept is confused with the steaming mechanism of Henry-Fauske. On

p. 17, "It has been suggested [7, 8] that large . . .," a similar confusion of concepts may be found. Indeed, I believe that it is hard to argue with such a simple and transparent concept and I would be curious to know of any reservations they may have on it.

In the NRC/IDCOR meetings where these matters were discussed at some length, it became clear that these two concepts are complementary and reinforce each other's conclusions, namely, that with reactor materials and geometries, the extent of premixing is severely limited. All reasonable estimates of mixing using these concepts agree that all plausible levels of energetics are far below the levels of concern for containment integrity.

Additional margins will likely be found in the aspects of the problem regarding slug acceleration and impact mechanics, however, just on the size of explosion alone, the margins are clear and comfortable.

P.S. It should also be pointed out that the statistics shown in Table IV could be off by as much as two orders of magnitude. For example, consider cases 11 and 14. With all aspects quite similar, i.e., slug energies, bolt failures, etc., the 90 m/s missile probabilities are off by two orders of magnitude. Similarly consider cases 15 and 19. Their mean explosion energies are identical, their slug energies are practically the same, they give the same rates of vessel bottom failure, yet the frequencies of bolt failures and of 50 m/s missile are off by a factor of x300! Other similar, but less pronounced discrepancies exist, but have not been discussed by the authors. I suppose this is indicative of widely different standard deviation associated with each mean, and the relative position of these to the various thresholds of structural behavior utilized in the model. It may be worthwhile to check that indeed this is the case and examine the implications of this behavior on the calculational scheme employed.

Sixth Information Exchange
Meeting on Debris Coolability,
November 7-9, 1984, UCLA.

NATURAL CIRCULATION PHENOMENA AND PRIMARY SYSTEM FAILURE IN STATION BLACKOUT ACCIDENTS

H.P. Nourbakhsh, Chien-Hsiung Lee and T.G. Theofanous*

INTRODUCTION

The potency of high pressure steam natural circulation phenomena to redistribute decay heat within the primary system components during the core post-dryout/pre-melt period has not been fully appreciated in the past. The original suggestions of their possible significance were made by Denny and Sehgal [1] and by Winters [2]. Denny and Sehgal built an idealized natural circulation flow regime, coupling the core with the upper internals, into the code CORMLT. From calculations for a Station Blackout accident that are emphasized to be preliminary, they draw significant implications concerning: delays in core degradation, reductions in thermal energy released to containment, and higher hydrogen generation rates. Winters extended Station Blackout calculations with RELAP5 into the post-dryout regime. These calculations produced a natural circulation loop between the reactor vessel and the steam generators and an associated energy redistribution of such magnitude that the possibility of primary system boundary failure prior to fuel cladding failure became evident.

Theofanous and Lee [3] also considered this mechanism in an assessment of the likelihood of the so-called "high pressure scenario," i.e., vessel failure and core release to containment from a high primary system pressure. This simplified analysis will be summarized first as a way of developing a feel for the order of magnitude of various effects. Some simple experiments of natural circulation within a partially volumetrically heated porous medium will be presented to more concretely demonstrate the efficacy of natural circulation cells to penetrate deeply into a heated porous bed. This paper will then conclude with a brief description of a numerical model including applications to this demonstration experiment as well as the reactor conditions of interest.

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ORDER OF MAGNITUDE ANALYSIS

We consider a simple 3-mass lumped parameter model as illustrated in Figure 1. Each mass is characterized by the same temperature and the fluid leaving is supposed to be in thermal equilibrium with it. This is a reasonable assumption for this approximate analysis given the highly distributed (high interfacial areas) core, upper internals, and steam generator masses. The coolant flow through each mass $\dot{m}_c$, is controlled by a loss coefficient (permeability) and the available head due to the temperature and associated density differences.

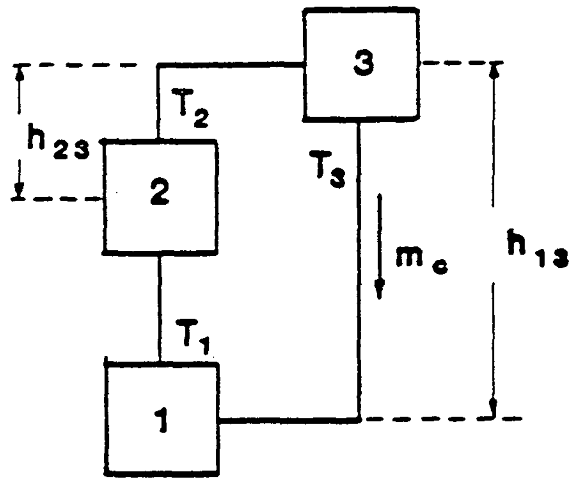

Figure 1. Schematic illustration of the 3-mass lumped parameter model.

The mathematical model may be expressed as:

$$R_1 \frac{dT_1}{dt} = Q' + T_3 - T_1 \tag{1}$$

$$R_2 \frac{dT_2}{dt} = T_1 - T_2 \tag{2}$$

$$R_3 \frac{dT_3}{dt} = T_2 - T_3 \tag{3}$$

$$n \frac{\mu}{k} \frac{\dot{m}_c}{\bar{\rho} A} = g(\rho_3 - \rho_1) + g\delta(\rho_3 - \rho_2) \tag{4}$$

where

$$R_i = \frac{m_i C_{ps}}{\dot{m}_c C_{pc}} \quad , \quad \text{and} \quad Q' = \frac{Q}{\dot{m}_c C_{pc}} \tag{5}$$

The solution for this system is:

$$T_3 = C_1 e^{z_1 t} \cos z_2 t + C_2 e^{z_1 t} \sin z_2 t + C_3 t + C_4 \tag{6}$$

$$T_2 = \left\{C_1 + R_3(C_1 z_1 + C_2 z_2)\right\} e^{z_1 t} \cos z_2 t$$
$$+ \left\{C_2 + R_3(C_2 z_1 - C_1 z_2)\right\} e^{z_1 t} \sin z_2 t + C_3 t + R_3 C_3 + C_4 \tag{7}$$

$$T_1 = \frac{Q't}{R_1} + \frac{C_0}{R_1} - \frac{R_2}{R_1} T_2 - \frac{R_3}{R_1} T_3 \tag{8}$$

where

$$C_0 = T_0(R_1+R_2+R_3) \quad , \quad C_1 = T_0 - C_4 \quad , \quad C_2 = -C_1 z_1/z_2 - C_3/z_2$$

$$C_3 = Q'/cR \quad , \quad C_4 = C_0/c\,R_1 - bQ'/c^2 R_1$$

$$a = R_2 R_3 \; , \quad b = R_2 + R_3 + R_2 R_3/R_1 \quad , \quad c = R_3/R_1 + R_2/R_1 + 1$$

$$z_1 = -\,b/2a \qquad\qquad z_2 = (4ac - b^2)^{1/2}/2a$$

For the computations Eq. (4) was coupled to Eq. (6) through the fluid equation of state and solved by successive approximations. Several numerical examples were considered to determine the effect of the assumed natural circulation flow pattern and of the core permeability (which would be related to the degree of degradation assumed). Geometric and mass data used are typical of a Westinghouse 4-loop plant. In all cases primary steam was at 2300 psia and decay power was set at 42 MW. The results are summarized in Figures 2 and 3.

For the "loop circulation" case it is assumed that the loop seals at the pump and lower plenum locations are broken and a steam flow path is open all around the primary system. The masses m_1, m_2, and m_3, are identified with the core, upper internals including upper head, and steam generator masses respectively. No heat losses to the outside of this three mass system are considered. Frictional losses in the core region dominate and they were represented by a permeability value of 1.5×10^{-8} m^2 which corresponds to a rubbled core with a characteristic particle dimension of 8 mm and includes a factor of 1/2x for deviations from spherical shape. A parametric using a higher permeability by a factor of 5x was also considered to represent a less disrupted core geometry. The calculations yield a nearly steady natural circulation flow of 25 kg/s per loop. This flow is sufficient to redistribute the decay heat such that the three mass system heats up after an initial transient of ~1 hr., essentially with a uniform rate of $\sim 1.7 \times 10^{-2}$ °C/s (Figure 2). The upper internals follow closely the core temperature while the steam generator lags by ~150, 70°C for the two cases considered.

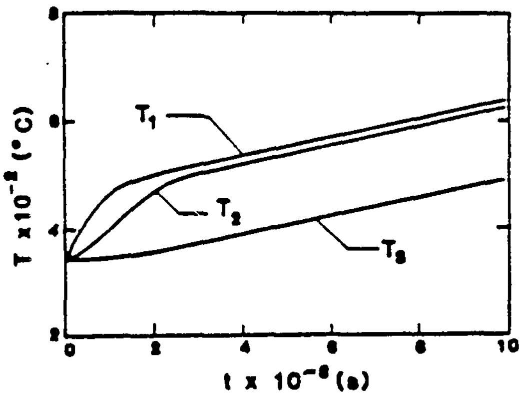

Figure 2. Predicted temperature transients for the "loop circulation" case.

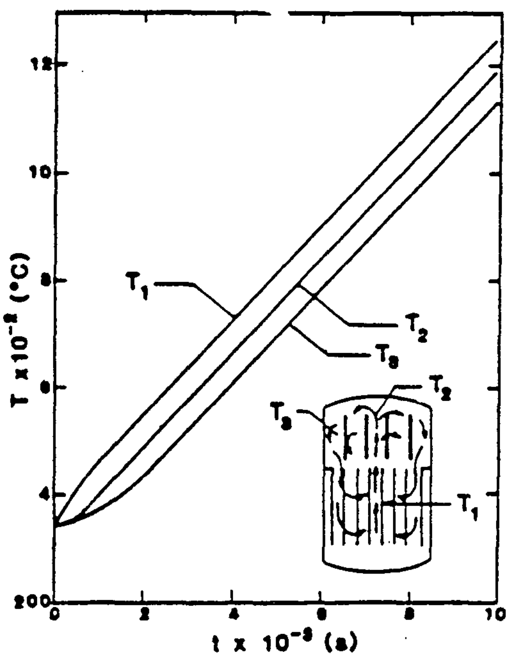

Figure 3. Predicted temperature transients for the "in-vessel recirculation" case.

Although some code calculations [2] do yield opening of the loop seals it is generally recognized that significant uncertainties in predicted behavior exist in this area. To complement the behavior depicted in the previous paragraph in this regard the "in-vessel recirculation" case was also considered. The flow pattern envisioned is shown as an insert to Figure 3. Now the three masses of our model were identified as the core, one-half of the upper internals (central region), and the other one-half of the upper internals (outer region). The same two cases of core permeability as in the loop circulation case were considered. For the nominal case a gradual increase in natural circulation flow to a steady value of 45 kg/s over a period of 2000 s was calculated. Again, these flows provide substantial thermal coupling between the core and upper internals masses. However, as seen in Figure 3, this coupling is not as efficient as in the loop circulation case. That is the rate of heatup is different for the three masses and at 10,000 s the outer portion of the upper internals lags by 120 and 60°C behind the core temperature for the two cases considered. Furthermore, as illustrated in Figure 3 these high temperatures would propagate into the hot leg, towards the steam generator, and pressurizer lines, by net flow caused by relief valve cycling as well as by counter-current steam flow in the hot leg [4].

These scoping studies indicate that primary system temperatures would follow the core temperatures during heatup within "a few hundred degrees Celsius" such that primary system boundary failure prior to core melting and slumping would be expected. Such failure would be due to loss of strength of structural material expected at around 650°C, i.e., would produce leak areas sufficient to depressurize the system. Furthermore if conditions for the loop circulation case were to be present, concerns about steam generator tube failures (by the same mechanisms) at high pressure should be addressed. On the other hand, conditions leading to clad ballooning to such an extent that blocking of the flow paths in the core occurs could also significantly alter these predictions. In such event phenomena of clad oxidation, melting, and relocation (which will reopen the paths), and fission product relocation within the primary system (and of associated thermal loading) must also be considered together with the natural circulation flows discussed here.

AN EXPERIMENTAL ILLUSTRATION

Purdue's Large Scale Debris Bed Coolability Simulation Facility [5,6] was adopted to illustrate the energy redistribution mechanisms for the "in-vessel recirculation" case. The present bed measures 21 cm in diameter and 100 cm in height and is packed by alternating layers of 1.25 cm aluminum spheres and a layer of similar thickness of 0.8 cm irregularly shaped stone fragments (gravel). It has a porosity of 3.86 and a turbulence permeability of 1.4×10^{-8} m^2 which is close to the permeability of a bed ~~made up~~ only with gravel and about one-half that of a bed of equal size spheres and the same porosity. The power to each layer of aluminum spheres is individually controlled such that any vertical portion of the bed can be volumetrically heated at will. For the experiment described herein a total of 5.7 kw were applied over a bed height of 30 cm extending from an elevation of 23 cm (from the bottom of the bed) to the 53 cm elevation. The bed was flooded with 20°C water and the experiment lasted until the bed reached boiling. The temperature transients were measured by means of 250 thermocouples appropriately distributed throughout the bed. These thermocouples were scanned every 5 s with the help of a PDP-11 minicomputer.

The results are shown in Figures 4 and 5. From Figure 4 we can clearly observe the conduction regime and a rapid transition to convection which is of sufficient strength to quickly homogenize (thermally) the whole bed. The prediction of the simple model already discussed for which convection was taken to initiate at a critical Rayleigh number of 30 is also shown for comparison. Observed spatial temperature distributions at selected times are shown in Figure 5. This figure contains also predictions based on the numerical model described in the next section.

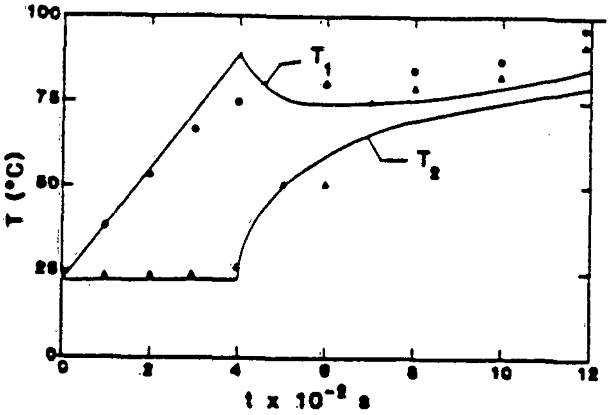

Figure 4. Comparison of 3-mass lumped parameter model predictions with experimental results.

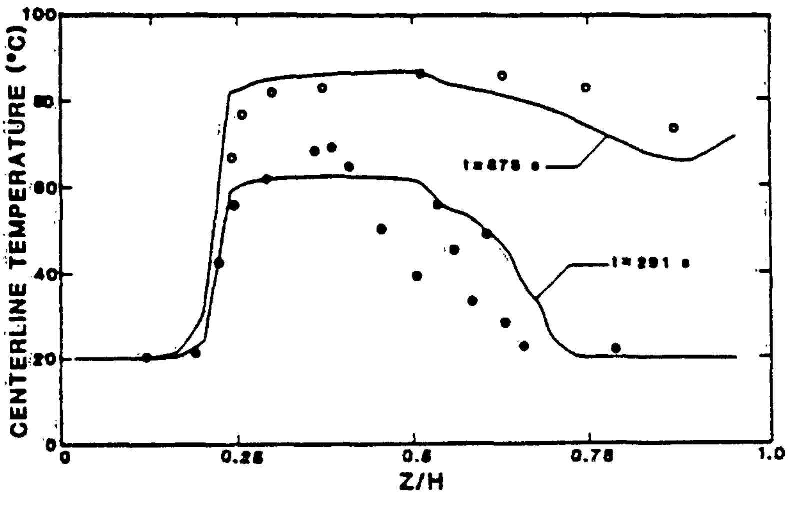

Figure 5. Comparison of numerical model predictions with experimental results.

A NUMERICAL MODEL

The "in-vessel recirculation" case was further examined by means of a distributed parameter model as follows. The whole vessel is viewed as occupied by a porous medium, the region corresponding to the core being heated while that corresponding to the upper internals being inert. The pore space is occupied by steam which is free to convect as buoyancy forces develop from heating. The vessel, as a whole is adiabatic. Friction is governed by Darcy's law, and density variations are handled in the Bussinesque approximation. This model with the exception that it includes thermal nonequilibrium between the fluid and the particles is commonly used for natural convection in porous media and may be expressed as:

$$\frac{\partial u}{\partial r} + \frac{\partial v}{\partial y} + \frac{u}{r} = 0 \qquad (9)$$

$$\frac{\partial p}{\partial r} + \frac{\mu}{\kappa} u = 0 \qquad (10)$$

$$\frac{\partial p}{\partial y} + \rho_{\ell} g - \beta\,(T_{f} - T_{\ell})\, g\, \rho_{\ell} + \frac{\mu v}{\kappa} = 0 \qquad (11)$$

$$(1-\varepsilon)\, \rho_p c_p \frac{\partial T_p}{\partial t} = \dot{Q} - h\, S(T_p - T_f) + k_p(1-\varepsilon)\left\{\frac{\partial^2 T_p}{\partial r^2} + \frac{\partial^2 T_p}{\partial y^2} + \frac{1}{r}\frac{\partial T_p}{\partial r}\right\} \qquad (12)$$

$$\varepsilon \rho_f c_f \frac{\partial T_f}{\partial t} + u \rho_f c_f \frac{\partial T_f}{\partial r} + v \rho_f c_f \frac{\partial T_f}{\partial y} = hS(T_p - T_f) + K_f \varepsilon \left\{\frac{\partial^2 T_f}{\partial r^2} + \frac{\partial^2 T_f}{\partial y^2} + \frac{1}{r}\frac{\partial T_f}{\partial r}\right\} \qquad (13)$$

The boundary conditions are obtained for impermeable adiabatic walls. This model was solved in nondimensional form by the finite difference method using upwind differencing and successive over-relaxation. At each time step the energy equations were solved explicitly (with implicit coupling of the exchange terms), while momentum and continuity were solved simultaneously using a marker and cell technique. For all calculations reported here a number of nodalization, time step, and convergence criteria, studies were performed to assure the reliability of computations. Clearly, appropriate porosity, permeability, and power distributions may be used in detailed evaluation. Two cases are considered here.

Calculations for the demonstration experiment discussed in the previous section were carried out using the measured bed porosity of 0.386 and a permeability of 1.5×10^{-8} m². From the comparison of Figure 5 we deduce that all essential features, including the conduction heatup regime, the time for the onset of convection, and the rapid transition to uniform heating following the onset of convection, are adequately predicted. In addition, for a short period following the onset of convection the model predicts a turnover similar to that observed in thermals with sufficient cold fluid penetrating the heated region to temporarily invert the vertical temperature gradient. Such behavior was qualitatively observed also in the experiments. However, due to non-axisymmetric behavior in the experiments precise interpretation of this aspect must await further investigations.

Reactor calculations were carried out with a uniform porosity of 0.5, a core power of 42 MW and a radial power distribution given by:

$$\frac{Q}{Q_{av}} = 2.32 \quad J_0 \left(4.81 \frac{r}{D}\right) \tag{14}$$

A low permeability value of 1.5×10^{-8} m² was chosen to maximize the thermal gradients calculated. An intact core geometry would be characterized by significantly higher permeabilities. Selected results of one such calculation are shown in Figures 6 and 7. In figure 6 we observe the unicellular flow pattern envisioned in our simple model presented earlier. In fact the predicted flow rate is ~50 kg/s which is in good agreement with the recirculation rate of 45 kg/s produced by the 3-mass model. The time-wise variation of the spatial temperature distribution may be visualized as in Figure 7. The isotherms are nearly vertical and seem to be continuously (in time) displaced outwards as new higher temperature isotherms are generated near the centerline. However, the maximum temperature difference at 10,000 s is only 240°C, and most of the mass is within 150°C which is in good agreement with the 120°C obtained in the 3-mass model.

CONCLUSIONS

High pressure steam natural circulation phenomena can be responsible for redistributing the core decay power to all primary systems components that are accessible to form circulating loops with the core region. As a result the primary system boundary is expected to fail prior to core melting, and the so-called "high pressure scenario" would appear unlikely. The possible role of clad ballooning, and oxidation remains to be assessed before these conclusions can be applied to all possible variations of a Station Blackout (or other similar) accident.

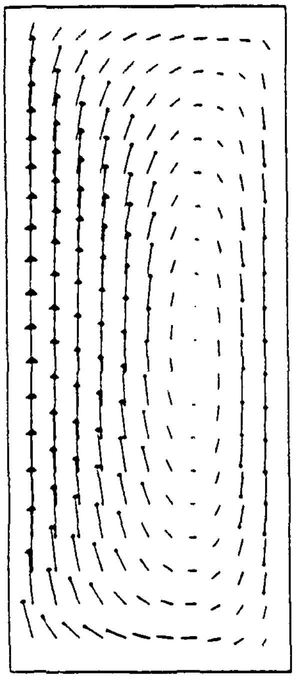

Fig. 6

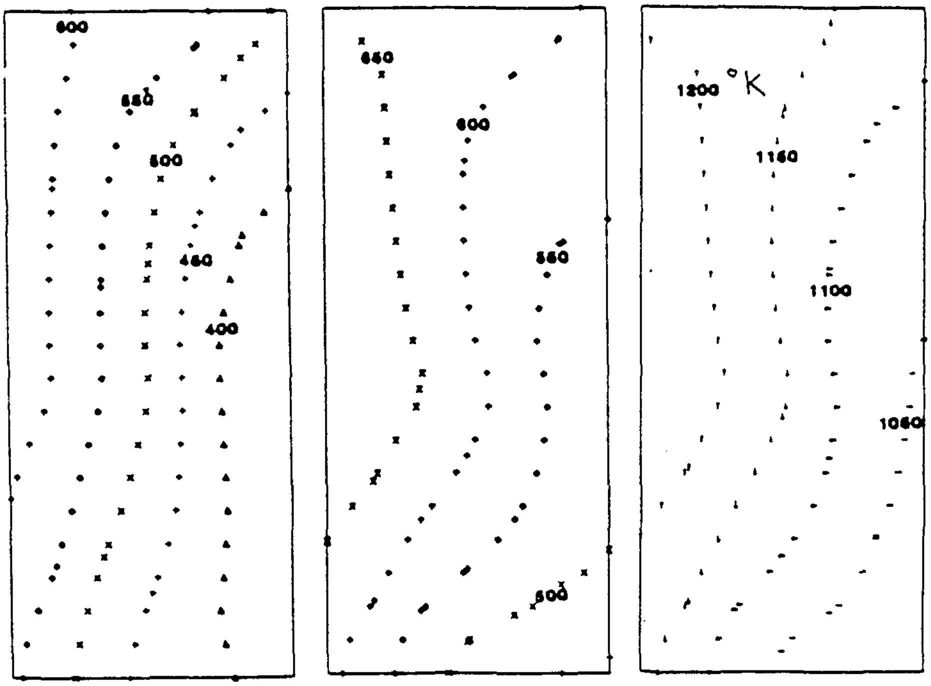

Fig. 7

Figure 6. Velocity vector plot at time = 1416 s.

Figure 7. Isoterms at 1416, 2832 and 9912 s.

ACKNOWLEDGMENTS

The financial support by the U.S. Nuclear Regulatory Commission under Contract No. NRC-03-83-093 is gratefully acknowledged.

NOMENCLATURE

A	= flow area
C_{pc}, C_{ps}	= specific heats of coolant and of structure
D	= vessel diameter
g	= acceleration of gravity
h	= heat transfer coefficient between fluid and particles
k_f, k_p	= thermal conductivity of fluid and effective thermal conductivity of porous medium
$\dot{m}_c$	= coolant flow rate
m_i	= mass of i^{th} compartment
p	= pressure
$\dot{Q}$	= volumetric heat generation rate
r	= radial coordinate
S	= the surface area between the fluid and porous medium per unit volume
T_i	= temperature of i^{th} compartment
t	= time
u	= velocity component in r-direction
v	= velocity component in y-direction
y	= coordinate in vertical direction

Greek Symbols

β	= thermal expansion coefficient
$\bar{\rho}$	= average density of fluid
μ	= viscosity
η	= frictional length/elevation length
δ	= h_{23}/h_{13}, where h_{23} and h_{13} are shown in Figure 1
κ	= permeability of the porous medium

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To: D. Ross, Deputy Director RES
From: T. G. Theofanous, Consultant
Re: Comments on Sandia's Steam Explosion Research Proposal

1. Introduction

Fuel-coolant interaction experiments, at two scales, ELVIS-400 kg and SEALS-2,000 kg, representing order of magnitude increases, in interacting melt mass, from those presently available, are proposed. According to the proposals this work will address a long list of severe accident safety issues from ''Accident Termination and Safe Shutdown'' to ''Indirect Failure of Containment.'' It is a pleasure to see the enthusiasm of the authors exceed any expectations; however, for purpose of review one must take one thing at a time. Since this whole effort seems to have been precipitated by energetic aspects of FCIs (α-mode failure of containments) and since it is this aspect that is emphasized in the proposal I will focus my attention accordingly.

The principal motivation of the proposed work is provided by Sandia's dispute of previous work which, on the basis of physical limitations in fuel/coolant mixing, concludes that the α-mode containment failure makes a negligible contribution to risk. The authors seek to examine experimentally whether such limitations exist and to provide data such that appropriate models can be developed and validated allowing more confidence in predicting the energetic consequences at full scale. This certainly is a worthwhile goal. In deciding whether the proposed research deserves support we must examine in addition to the prospects for comprehensive resolution of the stated objectives, also time and resource commitments relative to other potential approaches.

2. General Comments

First of all it should be clear (see my other two letter reports) that my comments are based on the perspective that the probability of missile induced containment failure due to in-vessel steam explosions is vanishingly small. Hence, any additional research in this area should be viewed as confirmatory. That perspective implies that we are not looking at an urgent, serious problem, requiring commitments along all possible approaches. Instead a rather deliberate, well thought out, and cost effective approach is required at this ''final stretch' in what has proved a long road to resolution of this issue. An important part of this deliberate approach would be to better delineate the range of conditions expected during lower core plate failure and associated fuel relocation to the lower plenum. Core heatup analysis has only begun to scratch the surface in elucidating these conditions. Specifically, relative timing of core barrel failure, lower core plate failure, vaporization of lower plenum water, and core melting must be better understood. Work is in progress in a number of places, including our own. I expect that this work may provide new perspectives as to the need of pursuing large-scale molten corium-water interactions, and will certainly provide the insights as to other, possibly more beneficial, areas for experimental work.

Without the insights described in the previous paragraph one is put in a position to consider the problems in a rather detached, ad hoc, basis. This has been the approach followed until now. Basically, then, one has to consider molten core quantities amounting to a significant fraction of the core held upon the lower core plate and release areas yielding almost catastrophic releases into the lower plenum. Even so, hydrodynamic and steaming limitations have been brought forth to limit the extent of coherent explosions under such conditions. Clearly the Sandia group is doubtful of such limitations. They propose experiments to resolve these doubts. I do not believe that at this time this is an appropriate approach. More specifically, large scale experiments on steam explosion energetics are premature at this time, and may be even irrelevant. Following the IDCOR meeting, we all agreed that hydrodynamic breakup together with steaming must be examined by considering transient and two-dimensional effects. A number of research groups are involved in this work right now. The need, relevance, and even the scope of any large scale experiments cannot adequately be assessed without an adequate theoretical scoping of the problem. I believe that between the results of such analyses and those of the heatup behavior spoken of previously the need for such large scale tests will be eclipsed. Furthermore, I believe that such results will be available within a year or two.

3. Specific Comments

As originally pointed out in our 1981 paper premixing limits are a scale effect. I believe that the ELVIS scale is clearly too small to address such questions. Hence this proposal will not be discussed further here. All comments below refer to the SEALS experiments.

1. I agree that a 2-ton melt may be sufficient to exhibit mixing limitations. I disagree that such quantities are in the range of interest for LWR containment failure. I believe that at least an order-of-magnitude larger quantity would be necessary for such results. I agree that it would eventually be possible to build and operate such a facility, however, the costs reported in the proposal are grossly underestimated.

2. I am concerned with the author's attitude concerning understanding and interpretation of previous work. I mentioned in my comments on the Sandia report their misconception of the hydrodynamic breakup limitations concept. In the SEALS proposal I see that they have also misunderstood the Henry-Fauske steaming limitations. They propose that the criterion for going on to phases 2 and 3 will be whether the melt falls into the water like ''molasses.'' Since they have already managed to destroy a number of facilities with much smaller quantities they should know better.

3. The gases present in the thermite melt are highly undesirable. No discussion was offered as to how to adequately remove these gases from the 2-ton melt.

4. Without manpower needs reported we really do not have any idea on the cost of these experiments.

5. The manifestation of mechanical energy release and its measurement depend upon the inertia constraint present and the coupling, at impact, between the slug and head/honeycomb system. The lack of a prototypical inertia constraint in the experiment is not discussed. The difficulty of slug/head coupling was recognized although it was dismissed on the grounds that the ''relative'' behavior is of interest (i.e., from one experiment to the next). I find the proposed experiment very weak on both counts. The point is that not only the elastic properties of the slug will vary from experiment to experiment, but also the slug-to-head mass ratio. If we also consider the significant losses due to condensation in the water I conclude that the proposed system will suffer from grave uncertainties concerning the most significant aspect of its results.

4. Conclusions

I recommend that large scale experiments in this area are premature at this time. Given the state of knowledge in steam explosions we should seek to scope out analytically the heatup behavior and premixing behavior. Based on results of such studies the need of, and conditions for possible future experiments should be addressed. In addition I cannot recommend the proposed experiments because of large uncertainties in measurement of conversion ratio and lack of adequate inertia constraints.

APPENDIX D

SUMMARY OF COMMENTS ON MAJOR STEAM EXPLOSION PHENOMENA

On the second day of the Harpers Ferry meeting the Steam Explosion Review Group (SERG) discussed some of the fundamental mechanisms involved in the explosion and reviewed the proposed SNL research program. The conclusions of our discussions follow.

Our discussions on the fundamental processes were divided into four general categories: (1) initial conditions for the explosion, (2) fuel-coolant mixing, (3) single and multiple explosions, and (4) slug dynamics. In each area the SERG came to some agreements and identified some areas of differences of opinion. We summarize each category below:

Initial Conditions

a) High pressure core-melt sequences may be avoided due to local heating of structures in the upper regions of the reactor pressure vessel (RPV) which may lead to leak before rupture (this same phenomena may weaken the upper head structure and this possibility although deemed remote should be checked);

b) The amount of the core that is molten at the time of contact between the fuel and coolant is still uncertain - some consider that the fuel will melt incoherently and drip into the lower plenum in small masses while others conceive of the meltdown process to be more coherent and result in a large molten pool (10% molten) before fuel pouring into the lower plenum (see Figure 1);

c) The temperature of the fuel and its composition at the time of fuel-coolant contact is also uncertain although linked to the possible scenarios in b) above - if the meltdown is incoherent then the probable melt temperature will be between the solidus and liquidus of the melt (U-Zr-O), while if the meltdown is coherent the probable melt temperature will be above the liquidus of the melt (UO_2-ZrO_2-Zr);

d) The size and location of the melt pour stream into the remaining water in the lower plenum of the RPV is also in doubt (see Figure 2) -the pour stream may be centered in the lower grid plate or off to the side and may be aided in breakup by the lower internal structure (the lower internal structure may also aid in early triggering of the explosion; neither option has been systematically tested to date);

e) The SERG agreed that core meltdown calculations on melt progression at the present time should be viewed with some critical skepticism, and each calculation should be internally consistent and compared to other consistent sets of calculations.

Fuel Cooling Mixing

a) In order to aid in understanding of this concept a pre-mixing length scale was defined to be that physical dimension which is determined by the hydrodynamics before the explosion and in turn determines the available exposed surface area of the melt available for interaction with the coolant (see Figure 3);

b) The 'coupling length scale' was defined as that physical dimension which is determined by the explosion shockwave propagation itself and specifies the depth of melt at the exposed surface area that is rapidly fragmented and quenched - i.e the actual mass of melt that 'participates' in the explosion (the SERG noted that the subsequent 'blast wave' expansion of the explosion could also cause subsequent fragmentation of the fuel but some members felt that the rate of this fragmentation was too slow and the size of fragments too large to contribute to the explosion yield);

c) These concepts were discussed at length and no consensus was reached on how these quantities could be calculated a priori at this time, although some members of the group felt that they could quantitatively bound their values.

Single and Multiple Explosions

a) Flooding and/or fluidization limits on fuel-coolant mixing and the subsequent explosion seemed to be valid concepts in principle although some members of the SERG had some difficulties with specific models and/or predictions that have been advanced;

b) Transient jet breakup due to entry hydrodynamics also seemed to be a relevant concept when considering mixing and the explosion, although not all the details of current models have been experimentally examined;

c) Multiple explosions can occur as evidenced by the FITSB experiments at SNL, however, there does not seem to be a concensus on the ramifications of such phenomena in the full scale accident situation - some members consider multiple explosions to be a concern because one explosion may cause rapid mixing of nearby 'unmixed' melt and thereby increase the melt involved, while others consider one explosion to be the trigger of a second explosion of a nearby melt mass and the propagation of the second explosion to be fast enough to 'decouple' its fragmentation from the blast wave of the first event (see Figure 4).

Slug Dynamics

a) The formation of a slug that transmits the explosion energy to the upper head in the form of slug kinetic energy may take on different compositions - e.g., a water slug may be formed in the downcomer region of the RPV while a fuel slug may be formed in the core region (see Figure 5 and consider that these possibilities not to be mutually exclusive depending on the time of the explosion trigger);

b) Taylor instabilities would be formed at the boundary between the slug and the explosion zone, and this phenomena would affect the expansion in two ways - first, the slug would begin to breakup and its final kinetic energy would be limited by this process, and second, the liquid from the slug breakup would be entrained into the explosion zone causing energy transfer with it (a cooldown of the explosion zone would occur if coolant is entrained; conversely a heatup of the explosion zone would occur of melt were entrained).

c) Slug dynamics would be affected by the possible failure of the lower plenum wall of the RPV due to the locally high explosion pressures -this would reduce the upward slug kinetic energy and has been included in NUREG/CR-3369 analyses;

d) Slug impact with the upper internal structure and the upper support plate in the RPV would absorb some of the kinetic energy of the slug - SNL and recent LANL estimates of this energy absorption capability both seem to agree on the upper bound values and this also has been included in the NUREG/CR-3369 analyses.

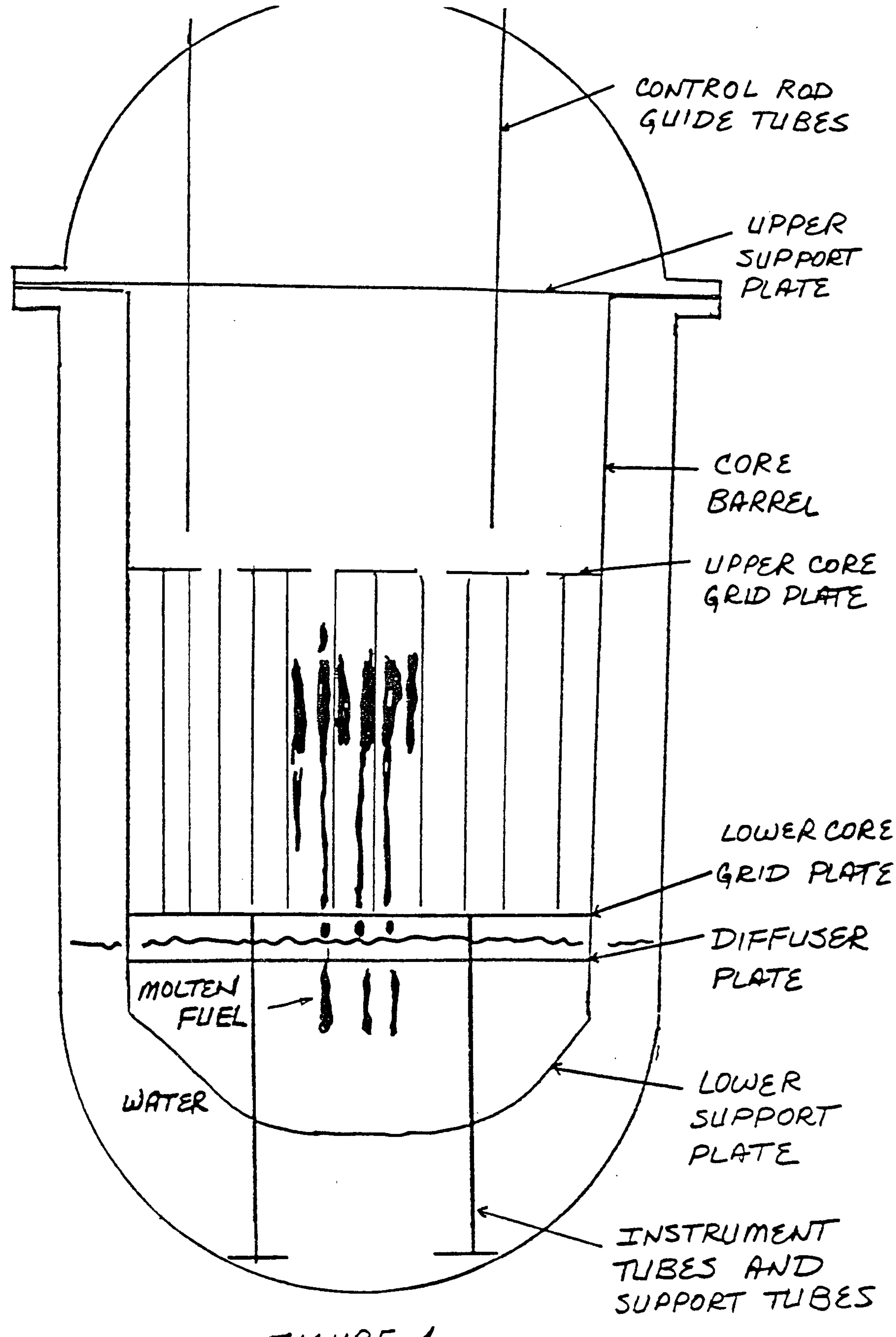

FIGURE 1a

INCOHERENT FUEL MELTING AND QUENCH

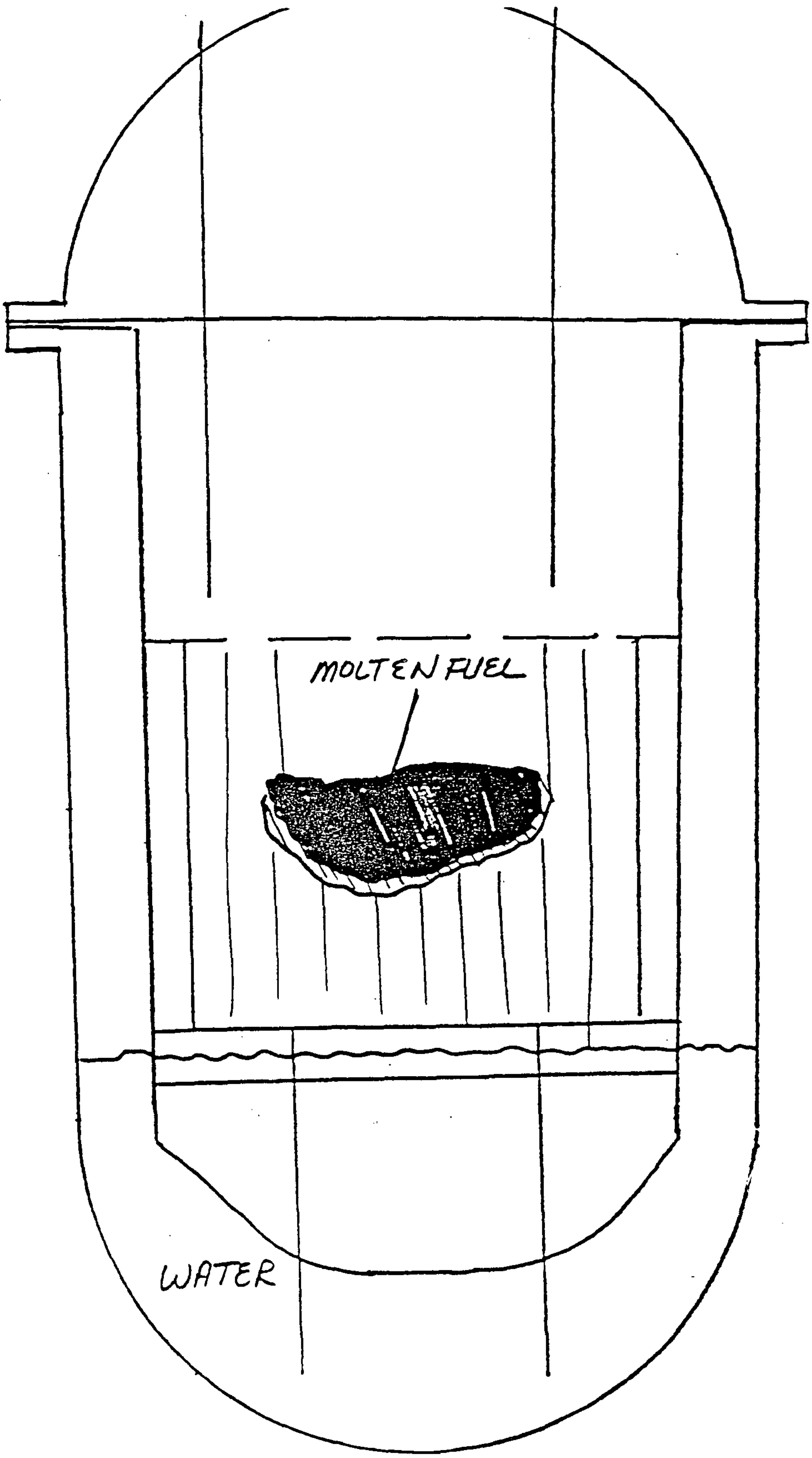

FIGURE 1b

COHERENT FUEL MELTING

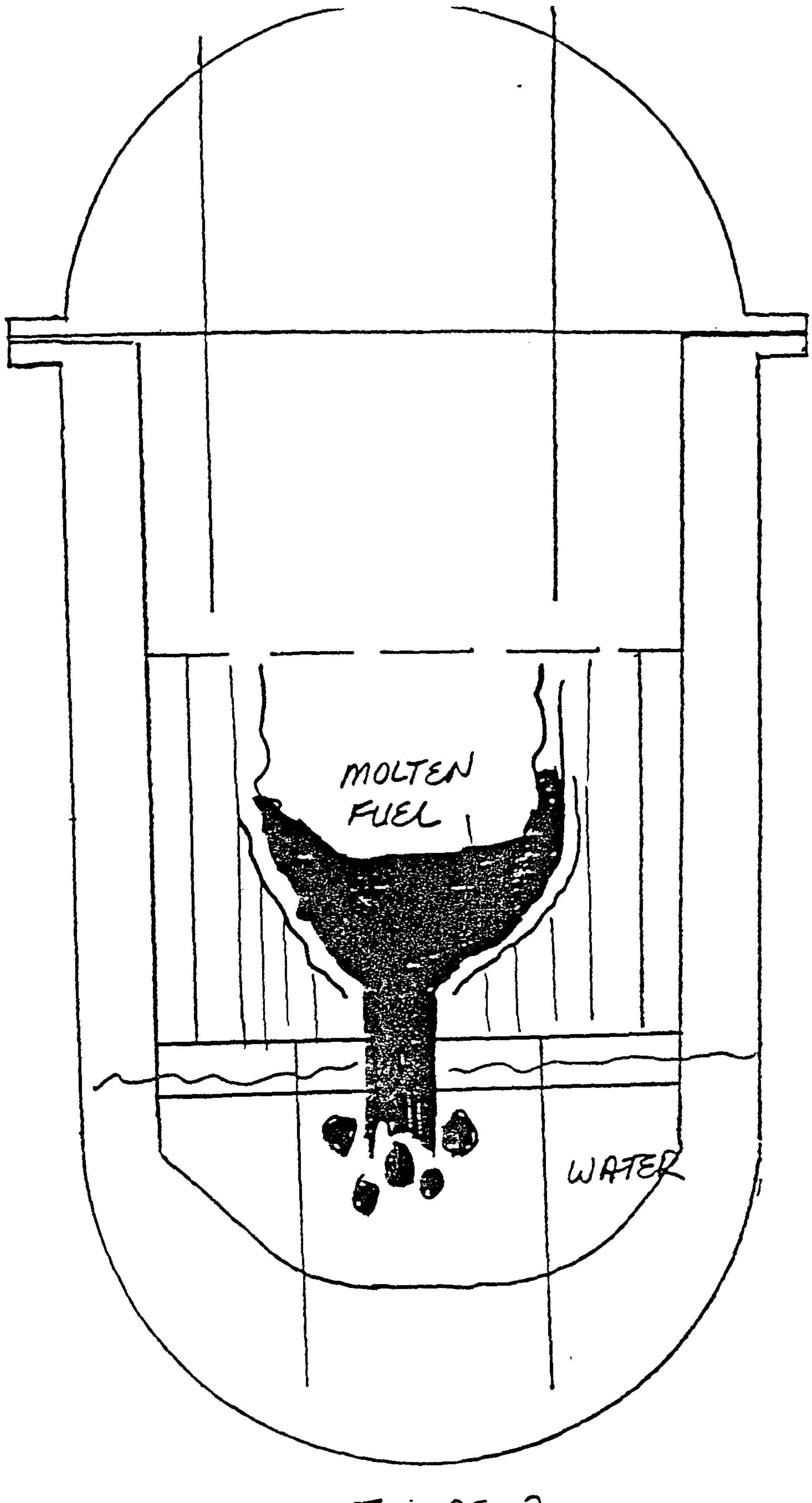

FIGURE 2a
FUEL POURING NEAR CENTER OF CORE

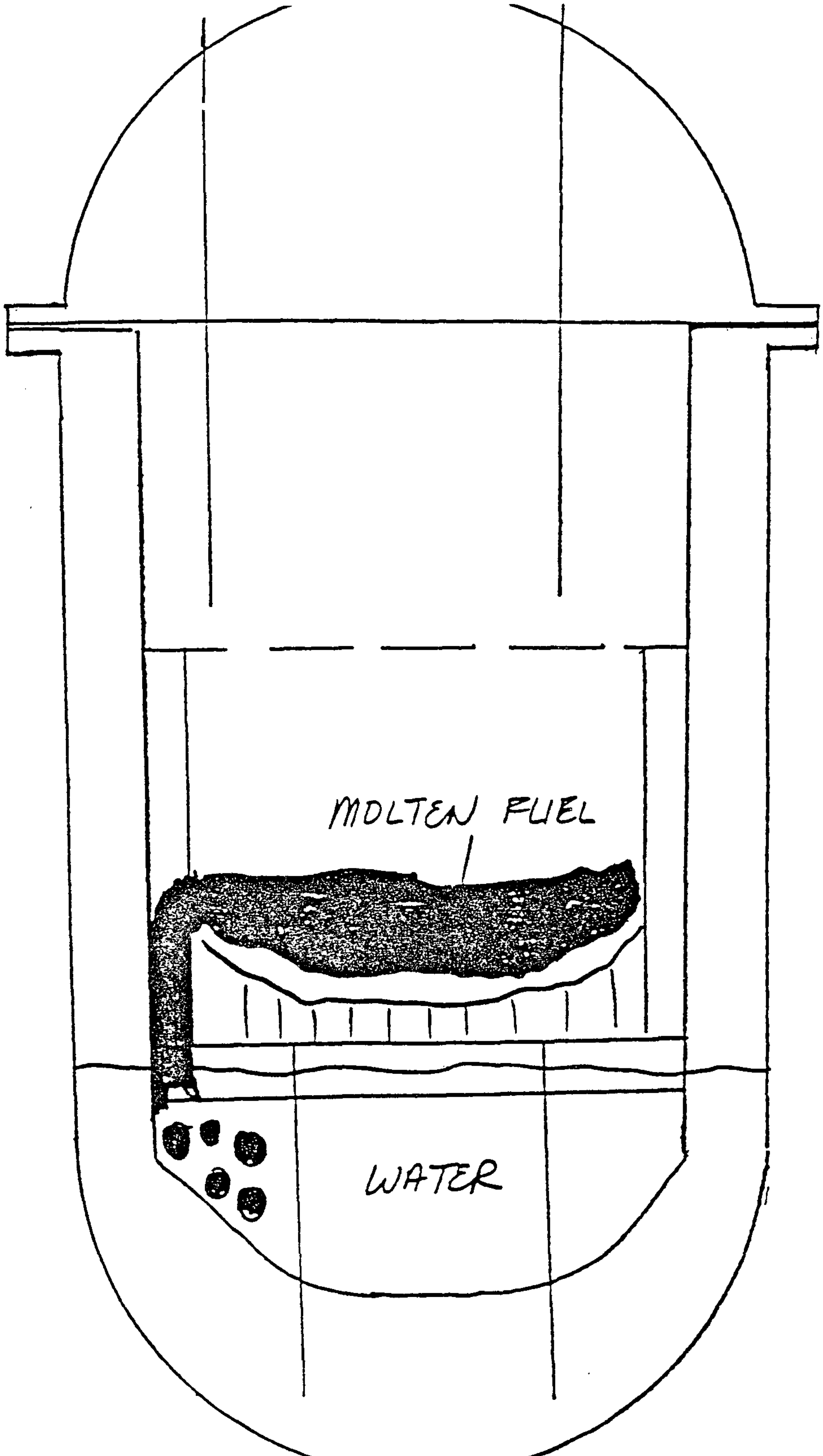

FIGURE 2b
FUEL POURING NEAR EDGE OF CORE

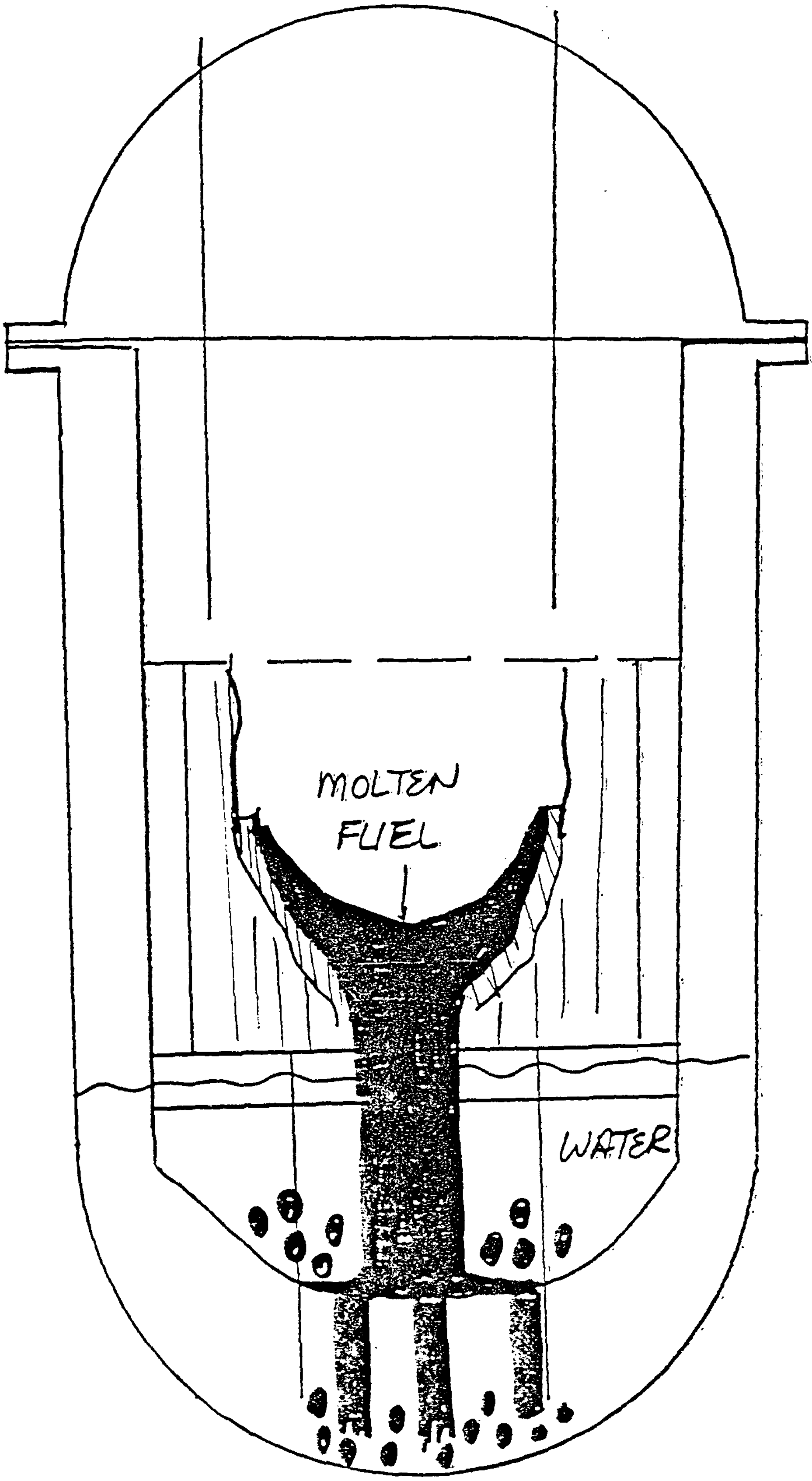

FIGURE 2C
FUEL POURING THROUGH STRUCTURE

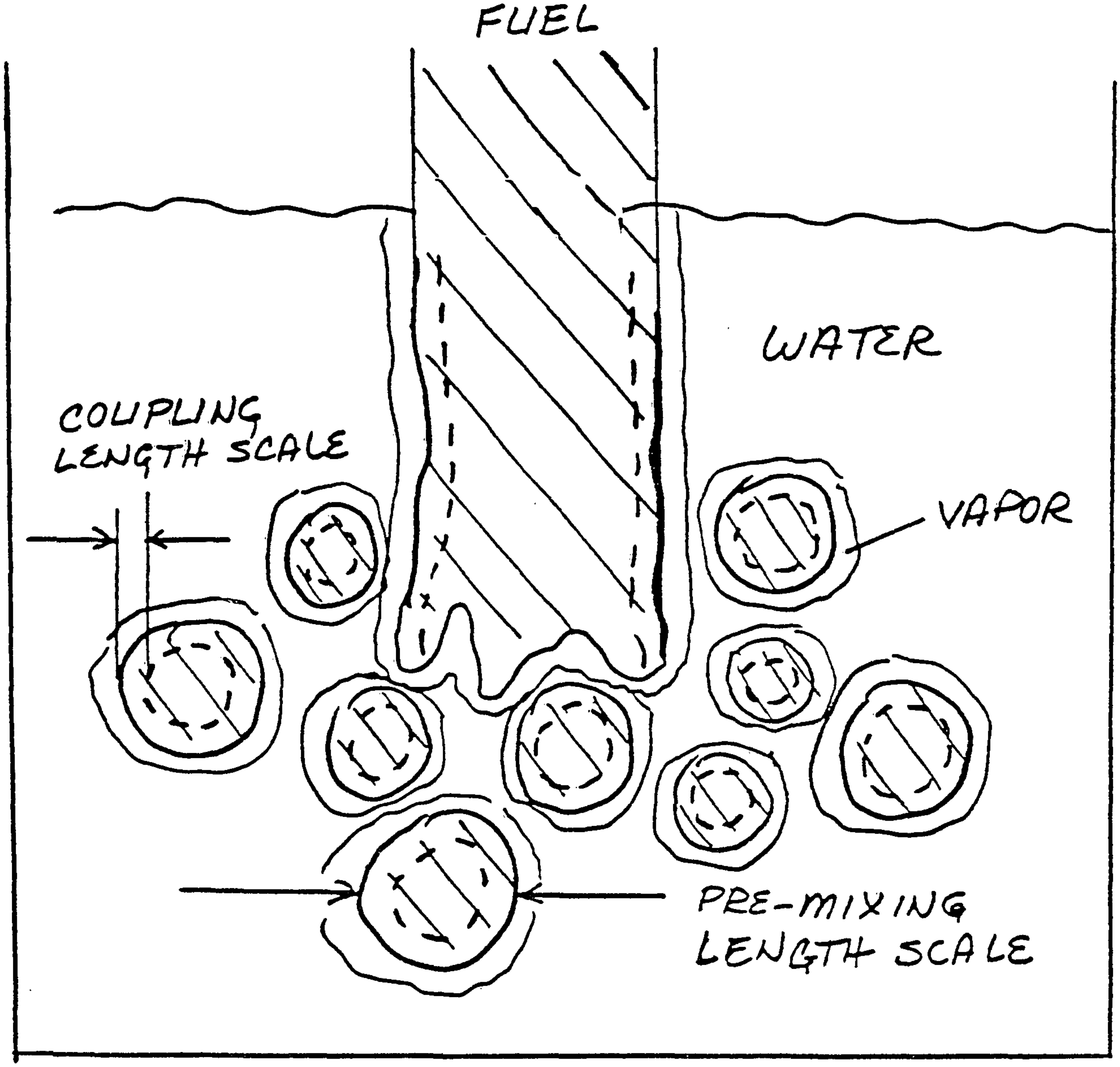

FIGURE 3
CONCEPTUAL PICTURE OF FUEL-COOLANT MIXING

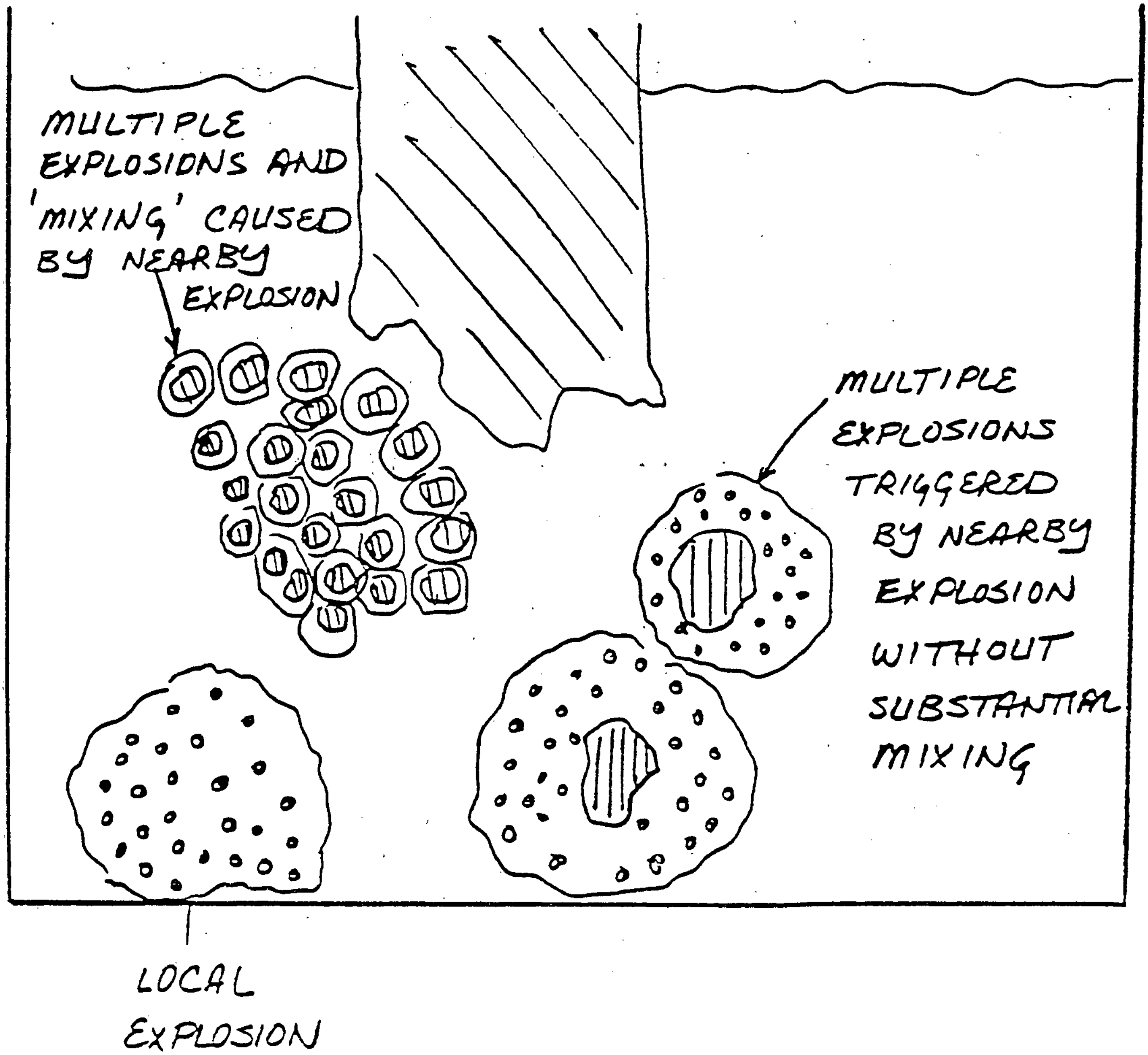

FIGURE 4

CONCEPTUAL PICTURE OF MULTIPLE EXPLOSIONS

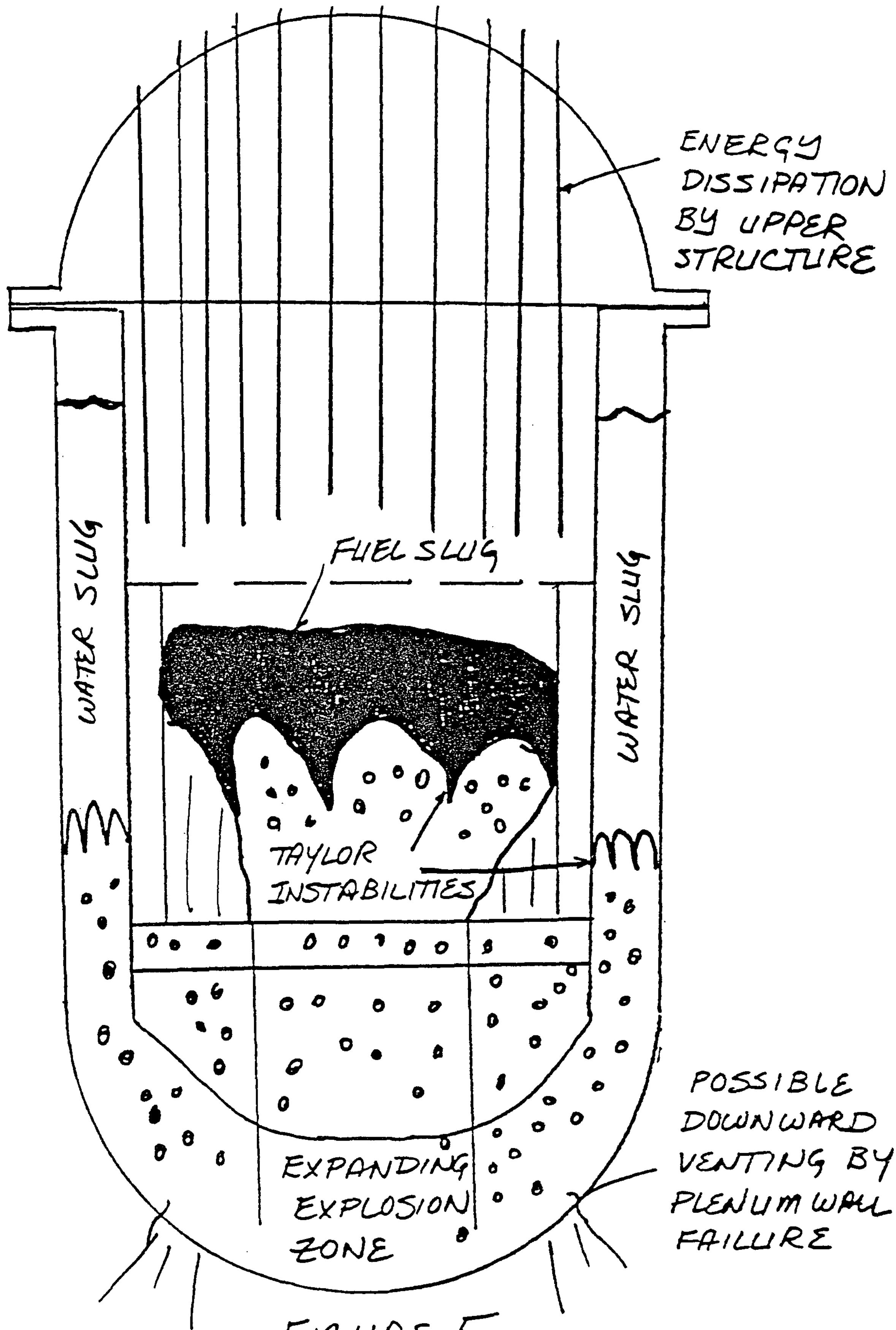

FIGURE 5

SLUG DYNAMICS PHYSICAL PROCESSES

Sandia National Laboratories
Albuquerque, New Mexico

date: January 16, 1985

to: C. Allen, NRC, T. Ginsberg, BNL, M. L. Corradini, UW

Marshall Berman

from: Marshall Berman, SNL

subject: Comments on Draft Summary of the Steam Explosion Review

I have read the preliminary draft of the SERG Committee summary which you sent me. Although I recognize that you have attempted to honestly represent the views of the committee members, I nevertheless disagree with many of the statements and conclusions which you have drawn. I would like this memo to accompany the distribution of your summary as a dissenting minority opinion.

GENERAL COMMENTS

The opinions of almost all the SERG members concerning alpha-mode failure were a matter of public record prior to the deliberations of the SERG committee. No knowledgeable individual expected that their opinions would change as a result of the group's work. Hence, a summary which is simply a poll of the members' beliefs could have been written without the expense and work of the committee. A valuable and accurate summary would require the authors to investigate the underlying reasons for particular opinions and the associated uncertainties in those opinions.

There is no doubt that some of the SERG committee members supported one or more of the statements referred to in the summary report. However, I believe that references to "broad agreement" do not do justice to some of the disagreements that were obvious both in the written material, and in the meeting at Harper's Ferry. Many of the committee members openly and frankly admitted that their statements were conjectural. However, all but D. Cho and P. Cybulskis felt obliged to provide probability estimates, even when they recognized the complete lack of any statistical basis for their estimates. (I. Catton refers to his own "probability" estimates with the following descriptive sentence: "Calling the estimates made in the above arguments probabilities is an insult to those who deal in statistics.") It is clearly legitimate for any individual to provide his subjective probability estimate or guess concerning alpha-mode failure. However, to be useful for decision making, the foundations and uncertainties associated with these guesses must be addressed and evaluated.

The fact that the probability estimates were all low is not surprising. Almost all individuals will assume that events that have not already happened are unlikely. (Consider a hypothetical poll of experts concerning the probability of the Union Carbide Bhopal accident before it happened.) It is essential that the summary discuss both the reasons for holding a particular view and the uncertainties associated with those reasons. The summary stresses the former and totally ignores the latter.

Many statements in the summary refer to ideas about mixing and conversion ratio (and other phenomena associated with alpha-mode failure) which are hypothetical and have not been factually demonstrated. The summary does not clearly distinguish between fact and supposition. Hence, the summary also completely misinterprets the purpose and objectives of the Uncertainty Report (NUREG/CR-3369). This misunderstanding may have been prevalent among some of the SERG members, but others clearly understood the intent of the report. (E.g., see the comments by A. Briggs.)

Finally, the language used in the summary is imprecise and frequently misleading. For example, the reference to "current thinking" in the executive summary clearly discriminates against a large body of current thinking, held by some of the SERG members and a large external group of highly competent and experienced scientists and engineers in the United States (e.g., at Sandia, Argonne, and the NRC), England, France and Germany.

The summary seems to describe a uniformity of opinion on the part of the SERG that was not apparent to me in either the written material or in the meetings. A more accurate portrayal of the group's opinions would stress the diversity, and even polarization, of views that prevailed among its members. To the extent that the summary failed to adequately portray the group's disagreements, it seems biased toward a particular viewpoint. The following material will document in detail the basis for my views.

SPECIFIC COMMENTS

EXECUTIVE SUMMARY

Paragraph 1:

The executive summary contains the indicative sentence: "The major argument supported by most members of the group is that the combination of a limited melt mass participating in the explosion together with a low thermal to mechanical energy conversion limits the explosive yield to values which are below estimates of values required for containment failure." The arguments, however, are clearly conditional and speculative. The major objective of the research program outlined by us is to prove the above statements. It is possible that large-scale experiments will demonstrate that the models are indeed valid, and even conservative. It is also possible that the models will be demonstrated to be incomplete or wrong. To imply that they have already been proven is misleading. Many SERG members recognized the speculative nature of assumptions concerning mixing and conversion ratio. Furthermore, lumping conversion ratio and limited melt mass together does not represent the viewpoint of some SERG members that conversion ratio may be quite high, and that limited mixing is the primary reason for expecting a low failure probability. The following excerpts clearly demonstrate some of the uncertainties that were recognized by the SERG members:

1. T. Ginsberg: "The available data base of experiments involving masses of up to 25 kg are of too small a scale to allow us to reach relevant conclusions regarding the behavior of large melt streams... All mixing-limited explosion concepts will remain as 'plausibility arguments' until data are available to substantiate any one of them. We will not, with any honesty, be able to put the problem to rest until we have demonstrated the validity of a premixing model from a suitably designed set of experiments..."

2. A. Briggs: "[The Sandia program] can only be justified if there is an important uncertainty in the assessment of steam explosion effects on the public risk from severe accidents. The current lack of agreement between different estimates of alpha-mode failure probability is strongly indicative that this is indeed the case...

   The origin of the current uncertainty is that detailed physical models of steam explosion phenomena have not been developed to the stage at which they can be used with confidence for quantitative prediction. In particular both the initial mixing phase, and the fine fragmentation/heat transfer mechanisms are not well-understood in detail. This makes the assessment of the importance of mass scale difficult and uncertain (etc.)."

3. I. Catton: "I agree with the arguments against previous analysis that have lead [sic] to limited mixing. A great deal more understanding of the mixing processes is needed before one can limit the mixing process. Here, I think we must accept that what gets in could mix."

4. T. Theofanous: "...Conversion ratios significantly lower than ideal cannot be adequately justified at this time. The Sandia experiments seem to yield increasingly higher values."

Even some members who felt that the mixing hypotheses would eventually be demonstrated to be valid still recognized the extremely weak foundation of current models. E.g., M. Corradini labels the estimates of fuel depth reacted as 'guesses': "The depth of fuel which rapidly fragments and quenches at this interface is not known although a number of investigators (i.e. myself, Theofanous and Fauske) have guessed that it may lie in the range of 1-10 cm."

In addition to the comments of these SERG members, many other highly competent and experienced scientists and engineers have reviewed the current mixing models and found them to be unsubstantiated and unverified. (With respect to hypotheses concerning limits to mixing, a prestigious group of British FCI experts concluded in the "Gittus" report: ".. the available evidence does not conclusively imply the physical impossibility of mixing tonne quantities of molten debris and water over the range of time-scales and particle sizes that are associated with a steam explosion.") Current models of mixing are frequently based on small-scale, isothermal, liquid-liquid mixing correlations, or flat-plate critical heat flux correlations, neither of which have been shown to be applicable to in-vessel steam explosions. Critiques of these models have been published and presented in open meetings. These critiques have not been formally answered nor have the technical questions been resolved.

Paragraph 2:

This paragraph states: "...In choosing probability density functions for use in their analysis, the authors do not make use of physical models which are representative of current thinking within the technical community." In light of the reservations stated above by some SERG members, and the many reservations of other highly competent people who were not SERG members, this statement seems very biased and misleading. Whose "current thinking" is <u>necessarily</u> superior to the "thinking" of many people who have spent years studying these phenomena? Consider the <u>stated objective</u> of the study: To evaluate uncertainties in failure probabilities and the associated parametric sensitivities. Are the SERG members and the authors of the summary asking us to unequivocally accept the validity of the current mixing models without experimental verification? If mixing is as limited as current models propose, the study shows that the failure probability <u>is</u> essentially zero. But a <u>major</u> point of the study was that these models are highly uncertain. These uncertainties <u>were</u> systematically investigated with resulting calculated probabilities covering the full range from 0 to 1, depending on the other initial conditions. In particular, 19 out of 49 calculations were devoted to investigating the influence of various mixing assumptions. The summary authors or SERG members might like to suggest a <u>different method</u> for investigating uncertainties in mixing models; but to imply that there are no uncertainties, and that one or more of the models should have been mechanistically included in the Monte Carlo model, is to completely misunderstand both the existing uncertainties and methods of investigating and evaluating them. To imply that these uncertainties were not investigated is clearly not true.

Section 1.1 Introduction and Background:

A major problem in analyzing the steam explosion issue has been the tendency of some authors to state a series of conditions at the outset of a report. These conditions are given the weight of geometric axioms - essentially self-evident. Unfortunately, as we have frequently pointed out, these axiomatic conditions are frequently either hypothetical or wrong. In addition, the axioms themselves frequently bias the results, or imply the existence of self-evident conclusions which may be neither self-evident nor true. This section also begins with a definition of "steam explosion" and axiomatic conditions that are demonstrably wrong. A steam explosion can be simply defined as a boiling process rapid enough to generate shock waves; (in some contexts, the boiling may only need to be rapid enough to damage structures.) Nothing else is needed. The remainder of this "definition" is superfluous and could strongly bias an uninformed reader. A steam explosion does not require "finely divided molten fuel.. intimately mixed with liquid coolant such that the thermal energy in the fuel is rapidly and efficiently transferred to the coolant thus generating a large amount of steam which can produce high pressures." The implication of the plethora of modifiers is that steam explosions are difficult to produce; the history of FCI experiments and industrial accidents, however, imply that steam explosions are often difficult to prevent. What are the meanings of the words "finely divided," "intimately mixed," "rapidly and efficiently," "large amount of steam," "high pressures?" Steam explosions have been observed for finely and coarsely divided fuel, for intimate mixing and no mixing at all; rapid and efficient heat transfer has meaning only in the quantitative context of the particular safety question, as does the amount of steam; and the experimentally observed pressures have been small, medium and large.

The next sentence clearly conveys a particular viewpoint: "To result in a significant safety concern the interaction must be very rapid (millisecond time scale) and must involve a large fraction of the core mass (greater than 10%)." This is simply not true. In fact, the time scale of the explosion is completely irrelevant to vessel or containment failure. The "explosion" can occur in 1 ms or 100 ms. The only factor important for failure is the loading on the vessel. This loading can occur due to high pressure generated by explosive and non-explosive boiling and heat transfer processes, by shock wave loading, by fuel/water/debris impact, by the transmission of loads through structures, or by combinations of any or all of these methods for converting thermal to mechanical energy. Furthermore, even if we (falsely) assume that the only failure mechanism is a single, coherent, steam explosion of 'sufficient' energy release, we can still show that much less than 10% of the core is required. If we assume a heat content of 1.6 MJ/kg, a conversion ratio of 30%

(higher values of this are possible from Hicks-Menzies calculations), and a vessel or containment failure energy of 830 - 3000 MJ, then only 1700 - 6000 kg of molten fuel would be required; this is 1.3 - 4.5% of a typical PWR molten core mass of about 130 tonnes. In a paper used by Briggs, the amount of material required was stated to be greater than 6000 kg, or less than 5% of a typical PWR core melt. Our calculations indicate that pour masses of this magnitude are <u>very</u> likely. The glib use of pseudo-axioms has plagued this field for years. Qualitative thinking and errors of factors of two or more could be found in the material presented by the SERG members. Order-of-magnitude arguments are used to make sweeping order-of-magnitude conclusions which we believe are not justified in many cases.

Section 2.1 <u>Probability of Containment Failure</u>:

2.1.2: The last paragraph states that the members "argue that a combination of small melt mass participating in the explosion together with a low conversion of thermal into mechanical energy, limits the explosion yield to values which are below the estimates of containment failure energetics." This is not true. Theofanous argues that small melt mass <u>alone</u> (not in combination) results in low failure probabilities, even if high conversion ratios cannot be ruled out. In addition, the SERG members and summary authors again treat adjectives (small, low) in a loose and unscientific manner. Note that T. Ginsberg's <u>best estimate</u> conversion ratio was 5%, equal to the absolute upper limit of the range of this parameter in the basic set of uncertainty calculations in NUREG/CR-3369!

2.1.3: The last paragraph accurately states that the SERG members believe that the probability of containment failure is small. However, the same looseness of language persists in the statement concerning "the difficulty in intimately mixing large masses of core melt with adequate water." What are the meanings of the words 'difficult,' 'intimately,' 'large,' and "adequate." As stated before, only a few tonnes of melt might be required. "Intimate" mixing may be "no mixing at all" for a stratified explosion. An "adequate" supply of water seems highly likely if not a near certainty. Based on today's knowledge, I believe the above sentence could be reworded with an entirely different emphasis: "There is great uncertainty concerning the ease with which sufficient masses of melt might mix with the available water." Unless we're careful, the particular style of writing and choice of words can encourage the reader to draw a conclusion which is not justified by the facts.

Table 3: In this table, and all other tables, P. Cybulskis' contributions were omitted. In the Corradini submission, probabilities of essentially one were assigned to the columns labelled Initial Conditions, and Slug Dynamics. Why were these numbers not included in the table? The probabilities in each of those columns vary from 0 to 1 for the various SERG members. Hence, the total uncertainty range in the table is from 0 to 0.1;

the only reason that the upper limit is less than one is the belief that mixing and conversion ratio will be insufficient. Given the uncertainties stated in 1. through 4. of this memo, we believe that the SERG upper limit of 0.1 (rather than 1.0) is based more on faith than on hard data and analysis. During the Harper's Ferry meeting, some of the underlying probability estimates were revised up or down by order-of-magnitude increments. (Interestingly, the final probabilities were unchanged.) If uncertainties in underlying probabilities are of the order of a factor of 10, it seems difficult for us not to treat the value of 0.1 as uncertain by a factor of 10. In any case, would it not be fair, reasonable and representative of the committee's views to include the following in the Executive Summary: "The SERG committee concluded that the uncertainty in the failure probability ranged from 0 to 0.1. This range is consistent both with WASH 1400 and the Gittus study in England." How can one exclude this information which addresses the most important question raised by D. Ross when he created this committee?

Section 2.2 Critique of Uncertainty Study (NUREG/CR-3369):

This discussion, in our opinion, is inaccurate in its portrayal of the methodology and assumptions in the report. The authors of the study are accused of not making use of "current thinking within the technical community" and exaggerating the uncertainties and values of the parameters. These statements are simply not true, despite the fact that the calculated uncertainty range of 0 to 1 is recognized, even by the study's authors, to be very unpalatable and undesirable. The authors, in fact, represent a major element of "current thinking within the technical community," and hence most certainly did not ignore their own thinking. We have presented our views on the FCI issues in many forums, and have provided strong technical support for our arguments. Stated overtly or not, the majority of the SERG committee members recognize the uncertainties in much of the prevailing "current thinking." I believe that the summary authors (and many SERG members) adopted a very parochial and narrow view of the state of "current thinking."

The history of steam explosion analyses is replete with examples of "current thinking" which have later been shown to be false. Some SERG members and other researchers have taken strong stands on certain hypotheses and models which either have subsequently been shown to be questionable or uncertain, or have already been conclusively demonstrated to be false. For example: 1. Large-scale explosions cannot occur without a premixture of particles one cm in diameter or less; 2. Large-scale explosions cannot occur in stratified geometries of water over melt; 3. Crushing of the upper internal structure could absorb a very large fraction of the slug energy; 4. Corium does not explode spontaneously; 5. Spontaneous explosions do not occur in saturated water; 6. Conversion ratio decreases with increasing ambient pressure; 7. Supercritical pressures cannot be achieved during steam explosions; 8. Large-scale steam explosions cannot occur for LNG-water systems; 9. The energy required to coarsely fragment the melt precludes large-scale steam explosions; 10. A liquid-continuous slug is a necessary condition for upper-head failure. This list is by no means comprehensive. What does it portend for the reliability and uncertainty contained in "current thinking?"

"Current thinking" did not anticipate that a steam explosion involving less than 20 kg of melt would destroy the EXO-FITS experimental facility at Sandia. Another steam explosion involving 24 kg of melt vented and stretched the bolts securing the upper head of the massive steel test chamber at Winfrith in the United Kingdom. Again, such extensive damage to the vessel had not been anticipated by 'then' current thinking. These events imply that "current thinking" requires verification and confirmation to demonstrate that such energetic events cannot occur at a scale large enough to jeopardize the reactor vessel or containment. In the absence of experimental confirmation, we believe that the current mixing models are unquestionably speculative and uncertain.

Some "current thinking" presupposes that a "coarse premixture" is a necessary condition for an "efficient steam explosion." Corradini and Theofanous have hypothesized that the hydrodynamic processes which cause mixing between isothermal liquids are also dominant in the case of unequal temperatures. It is conceivable that such an analogy will prove to be accurate for explosions in reactors. It is also quite possible that the analogy may be either non-conservative or wrong. In fact, under some circumstances (especially for high degrees of confinement) it is quite conceivable that the majority of the "mixing" takes place during the explosion itself, and because of the relative velocities induced by the explosion. Hence, the entire concept of a coarse premixture might be incomplete or even inapplicable to reactor geometries. Furthermore, the experimental observation of precursor explosions (which enhance mixing) must be addressed by current mixing models if they are to have credibility in accident analyses.

The summary report implies that the large variability in the SERG members' opinions provides additional support for the low failure probability estimates. We feel that it is also true that the variability indicates high levels of uncertainty and potential errors in "current thinking." H. Fauske states that the absence of a liquid-continuous slug is an important factor in his conclusion of low failure probability. Corradini et al. argue that it may not be a limiting factor at all. W. Bohl's SIMMER calculation shows that kilobar pressures can be achieved that last for many ms; supercritical pressures are calculated throughout the vessel which last for tens of ms. CSQ calculations have also shown that the absence of a liquid-continuous slug does not prevent large pressures from developing at the upper head. At the Harper's Ferry meeting, R. Anderson reported on experiments in which supercritical pressures were experimentally measured.

H. Fauske et al. in the IDCOR report placed great emphasis on limits to steam explosions due to fragmentation energy requirements. He did not employ these arguments at the SERG meeting in Harper's Ferry, nor did anyone else even mention such limitations. Are these arguments, which played such an important role in the IDCOR conclusions, no longer valid? Why didn't any of the SERG members refer to these arguments? What will be the status of "current thinking" after large-scale experiments are performed?

The statement that the ranges in the parameters were exaggerated is false. We challenge anyone to technically defend smaller uncertainty ranges than the ones we chose. Furthermore, the range in conversion ratio is now considered to be possibly much too low, when compared to several estimates provided by the SERG members themselves. The summary states that the most commonly cited example of excessive range is the amount of core melt; the upper limit was selected as 94,000 kg or 75% of the assumed core melt, the same limit used in the earlier Swenson-Corradini study (NUREG/CR-2307). Recent MELPROG calculations clearly support the possibility of this number as an upper limit. Furthermore, a reading of the report shows that this was not an especially important parameter. The conclusions of the report would not have changed at all if this number was reduced to 56,000 kg (46% of the core, and less than Bankoff's "more realistic upper bound" of 50%), and the conversion ratio range was selected to cover the range suggested by T. Theofanous. (Indeed, non-zero failure probabilities can be calculated for only a few thousand kg of melt, as discussed on pp. 5 and 6.) The ranges were selected after careful deliberation and many calculations; they were not selected in a whimsical and speculative fashion.

The range of pour diameters in Table 1 is from zero to the full diameter. Various SERG members speculated on smaller values. The speculations were based predominantly on qualititative thinking concerning melt-through processes; multiple explosions leading to massive fracturing of a supporting crust was not discussed. To quote I. Catton, "To just speculate, then give it meaning is not science; it is science fiction." MELPROG calculations show that large pour diameters are possible or likely. Although these calculations are preliminary, they certainly must be taken as seriously as the speculations resulting from qualitative thinking and simple hand calculations.

Squarer felt that the lower limit on slug void-fraction could "not be justified." Bohl and Butler, on the contrary, said that the slug void-fraction "range appears reasonable based on SIMMER-II calculations with postulated nonuniform interfaces." Finally, we feel that a range in pour depth from zero (no explosion) to the full lower plenum depth underestimates the actual uncertainties since it excludes the participation of stratified melt in the explosion.

Hence, _every_ range employed in the study is supported by one or more SERG members and/or by independent calculations. Should an uncertainty study use ranges that do not reflect the true uncertainties? The preceding discussion clearly indicates the uncertainties in the underlying ranges. The statement that the parameter ranges were exaggerated is not only false, but the reverse is the case: At least two of these ranges, conversion ratio and pour depth, might have been seriously underestimated. The restriction of conversion ratios to 0 - 5% and the restriction that an explosion must occur at or before bottom contact can clearly be interpreted as understatements of the true range of possibilities.

P. Cybulskis said that he was "troubled by the use of triangular probability distributions in the earlier Sandia studies...," but that he is "equally, if not more so, troubled by the use of rectangular probability distributions in the current study." Bankoff suggested an "M-shaped distribution for the pour diameter." (Imagine the _practical_ problem of achieving agreement from the SERG on the many parameters required to generate such a distribution!) Bohl and Butler said: "With these realizations concerning the nature of random behavior in the steam explosion phenomenon, the use of flat distributions is reasonable... we believe it is very difficult to _justify_ [original emphasis] other distribution shapes for performing additional calculations." Bankoff said that "[Uniform probability distributions and ranges which are close to the maximum conceivable for each parameter] are assigned without regard to physical calculations to check their reasonableness. Consequently, highly-biased results are obtained." On the contrary, we believe that the results are not biased, that they represent realistic uncertainties and sensitivities, that uniform distribution functions are appropriate for the objectives, that the use of any other type of distribution function could incorrectly imply more knowlege than is currently demonstrable, and that the ranges were determined by physical calculations and were checked for reasonableness by many experts in all cases; furthermore, recent experimental data and the views of some SERG members indicate that the ranges may have been underestimated in the case of conversion ratio and pour length.

Section 2.2.2:

The summary statement again favors a particular viewpoint. The conclusion that is accurate is that the SERG members determined a range from 0 - 0.1, compared to a range of 0 - 1 in NUREG/CR-3369. The reference to an "inappropriate choice of the probability density functions" requires that the group itself generate and agree upon an _appropriate_ choice of technically defensible probability density functions to determine the failure uncertainty. An examination of the summary tables shows that this new range would lead to the _same_ conclusions as determined in the report. It is also important to note that the earlier Monte Carlo study by Swenson and Corradini allowed up to 75% of the core to be molten, and up to 100% of the molten core to mix;

the "best estimate" was that 50% would be molten, compared to Bankoff's estimate of 50% as an "upper limit." That study would yield the same uncertainty range as NUREG/CR-3369 if several corrections were made; these include the very limited beneficial effect of crushing upper internal structure, and the use of more realistic conversion ratios.

CONCLUSIONS

The summary report tends to overemphasize the comments of some of the SERG members. It does not accurately report on the depth of disagreements or the ranges of uncertain phenomena. The wording and style of the summary report could be interpreted to contain a bias. We do not believe that the summary does justice to the contributions of some of the group or to the variety of opinions expressed at the Harper's Ferry meeting. To quote again from I. Catton, but in a context different from his intentions: "To just speculate, then give it meaning is not science; it is science fiction."

The SERG group <u>unanimously</u> supported the continuation of intermediate-scale experiments to address the effects of rigid confinement, prototypical structures, and explosions in stratified, <u>unmixed</u> geometries with separated fuel and coolant. (Interestingly, all these phenomena were either ignored or only casually or qualitatively mentioned in the analyses submitted by the SERG members. No quantitative models were presented; and only one member even <u>considered</u> stratified explosions, but only in a speculative and potentially non-conservative manner.) The majority of members either strongly favored large-scale experiments now, or felt that planning and design of the tests were premature. Furthermore, according to the summary, only two members (out of thirteen) "felt confident enough about the low probability of containment failure based on their analyses that the [large-scale] SEALS tests were not considered warranted." The Executive Summary stated that continuing (intermediate-scale) research was needed "to reduce uncertainties in certain aspects of steam explosion phenomenology and in estimates of alpha-mode failure probabilities." However, the summary, in my opinion, did not adequately discuss these uncertainties so that the need for research could be clearly seen and appreciated.

The key questions to decide are whether or not alpha-mode failure poses a risk to the public and what are the uncertainties associated with the current estimates of this risk. In fact, these are the only questions that should be posed for any aspect of reactor safety research. The committee is obliged to <u>distinguish</u> between personal opinions and scientific facts. This summary report appears to make no such distinctions.

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Dr C Allen
US Nuclear Regulatory Commission
Washington DC 20555
USA

22 January 1985

Dear Dr Allen

Comments on Draft Steam Explosion Review Group (SERG) Report

I have now had an opportunity to examine the draft report of the meeting at Harpers Ferry on 27-28 November 1984 further, with particular reference to its treatment of Sandia National Laboratories' evaluation of the uncertainty in the probability of containment failure due to steam explosions [1, 2]. My offers, as one of the authors of the NUREG/CR-3369 [1], to attend the meeting to assist in its discussion were refused, so I must assume that the meeting record is accurate [2]. However it appears to me that the conclusions drawn about NUREG/CR-3369 do not represent a balanced appraisal of that report, as suggested in my letter of 23 November [3], or follow from the supporting arguments. I will here explain why. Naturally this letter only necessarily represents my own views.

The second paragraph of the executive summary of your draft report summarises the SERG conclusions about NUREG/CR-3369 [1, 2]. It says two things; the first is:

> "The members of the review group believe the methodology used in NUREG/CR-3369 provides a potentially useful approach for estimating the probability of containment failure".

This statement is not strictly relevant to a review of NUREG/CR-3369 because our paper did not attempt to estimate the probability of containment failure. What it did was to estimate the uncertainty in this probability. Your statement above is not supported by tho discussion in section 2.2 of your report (the review of NUREG/CR-3369), nor by your section 2.1 which reports the SERG's attempts to estimate the probability.

The more important conclusion about NUREG/CR-3369 reported in your executive summary is that:

(5 9 1)

> "However, with broad agreement, the review group did not accept the conclusions of the study. The major factor in this conclusion is that in choosing probability density functions for use in their analysis, the authors do not make use of physical models which are representative of current thinking within the technical community."

This justification is self-evidently false. Because Sandia is one of the two laboratories, to my knowledge, currently conducting major experimental steam explosion research programmes (the other being Winfrith), the physical models used in the Sandia study necessarily are an important part of current thinking within the technical community. For further discussion of modelling choices, I will refer to Section 2.2 of the draft SERG report.

Section 2.2.1 of your draft reports that on the one hand:

> "With broad concensus the SERG members agreed that the methodology described in NUREG/CR-3369 was inadequate for the purpose of establishing the uncertainty in the probability of containment failure by a steam explosion."

and on the other:

> "There was considerable support among SERG members for the use of the Monte-Carlo method as a method of combining uncertainties, provided that the probability density function are chosen to adequately represent the physical phenomena."

It is thus not immediately clear whether the SERG are objecting to our method of combining uncertainty in parameters, to obtain uncertainty in containment failure probability, or to our selection of probability distributions to represent the parameter uncertainties. However I will assume that the objection is to the latter and not to the former because:

- there is no discussion in Section 2.2.1 of [2] of the method used to combine parameter uncertainties to obtain the probability uncertainty in [1], but there is discussion of the choice of probability distributions and

- the SERG do not offer an alternative method for estimating the uncertainty in the containment failure probability.

Section 2.2.1 of your draft report repeats the claim that the authors of [1]:

> "did not make use of physical models of aspects of steam explosion phenomenology which are representative of current thinking within the technical community."

Although "aspects" appears in the plural, only one of these is discussed, namely limits to mixing. Failure of the report to disclose what other aspects, if any, are intended, weakens support for the claim quoted above, makes it inscrutable and prevents a full reply to it.

Mixing models predicting smaller limits to mixing than the one used in [1] were reviewed in the Sandia study, in my two letters to the SERG, and elsewhere [1, 3-5]. These reviews showed, for each model, that it did not accommodate fully our uncertainty in one or more of:

- multi-dimensional effects
- time dependent effects
- possible enhancement of mixing due to steam production or
- the effects of multiple explosions.

Thus the conclusion in [1] that:

> "At present, evidence does not exist to allow an upper limit to be imposed on the mass of melt mixed that is less than all the available melt".

still stands. The SERG's objection to this conclusion is also seriously undermined by the Group's failure to agree on any smaller upper limit. This disagreement is partially displayed in Table 1 of the draft [2]. (I have not checked all the entries in this Table, but from my reading of Ginsberg's submission [6], his conversion ratio should be 0.05 and melt mass 1250 - 15,000 kg). The arguments used to support various limits to mixing proposed by Members of the SERG were sufficiently unconvincing that, as reported in Section 1.1.3 of the draft report, after the meeting,

> "No modifications to earlier positions were received."

The SERG's inability to agree on mixing models or limits meant that they were also unable to propose a range of probability distributions, for parameters relevant to mixing, to replace those in NUREG/CR-3369. They did not do this for any other parameters either, or suggest how it should be done. Thus their criticism of the form of the distributions used in NUREG/CR-3369;

> "It is felt that we do indeed know more about many of the various processes and parameters than is implied in the report by the choice of the rectangular distributions."

is unsubstantiated.

Although the SERG do not recommend how to estimate the uncertainty in the containment failure probability, their responses to the question requesting a "best estimate" of this probability display a wide variation. Section 2.1 of [2] summarises the replies but does not state the range of estimates. Various ranges can be abstracted from the replies listed in Section 2.1.4 of [2] and elsewhere. I tabulate three here.

BASIS OF RANGE	RANGE	REMARKS
"Best Estimates"	0.0-0.0038	Arguments that these single estimates are useless were submitted to the SERG [Section 10 of 3] but are not referenced or discussed in the SERG draft report [2].
Explicit Uncertainty Range	0.0-0.1	Upper limit from Bohl and Butler obtained by stylised argument; evidence inconsistent with higher values than 0.1 not cited [7; see Section 10 of 3]
Implicit Uncertainty Range	0.0-1.0	Briggs [Section 4.1 of 8; see Section 10 of 3]. Agrees with NUREG/CR-3369

These values are discussed further in Section 10 of my 23 November letter to the SERG [3]. That discussion shows that only the 0.0 to 1.0 range is useful in the sense that it provides a technical basis for decision making. That range can very simply be seen to follow from the evidence before the SERG, since the Group did not rule out the possibility of α-mode containment failure (however subjectively unlikely they found it), and evidence was not put before the Group

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inconsistent with values, outside a range narrower than 0 to 1, of the fraction of core-melt accidents leading to such failure. (The non-zero probability numbers given were strictly subjective probabilities.)

The 0 to 1 range is also supported by a similar argument in Sandia's recent review of severe accident uncertainties [9].

I have tried, in this letter, to point out where the conclusions reached in the draft SERG report do not follow from the evidence before the Group, with particular emphasis on the Group's review of NUREG/CR-3369. The discussion set out here is, in my view, a necessary adjunct to the SERG report, in its present form, if a balanced view of NUREG/CR-3369 is to be presented. I therefore request that, unless the SERG report is substantially revised along the lines of the discussions herein, a copy of this letter be included in an appendix to your final report to Dr Ross. This is in addition to my request (Fax of 17 January) that my letters of 9 November and 23 November be also included.

Yours sincerely

Tony Wickett

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M Berman, Sandia
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1. M Berman, D V Swenson and A J Wickett, An Uncertainty Study of PWR Steam Explosions, NUREG/CR-3369, SAND83-1438 (Albuquerque, NM, May 1984).

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To Members of the USNRC Steam
Explosion Review Group

9 November 1984

Gentlemen

Response to Comments by Corradini and by Briggs on NUREG/CR-3369

Following discussions with Cardis Allen and Marshall Berman, I have agreed to provide responses to the various comments made by members of the Review Group on NUREG/CR-3369, of which I was a co-author. This letter contains my responses to Corradini's and Briggs' comments. Naturally it only necessarily represents my own views.

If anyone would like further clarification of any point, please do not hesitate to contact me. I hope this letter is useful to the Review Group.

1. RESPONSE TO CORRADINI'S COMMENTS

Corradini's comments on Reference 1 consist of an initial paragraph summarising his reading of the report, followed by three numbered points setting out his opinions [2]. This reply will be set out in the same order.

In his first paragraph and his point 1, Corradini describes the calculations reported in Reference 1 as a sensitivity study rather than an uncertainty study. He does not explain whether this is because (a) he agrees with the definition of uncertainty analysis in paragraph 1 of Section 2.1 of Reference 1, but thinks the study does not satisfy this definition or (b) thinks the definition is inappropriate. In case (a) I suggest that he explains where the calculation in [1] falls short of the definition. In case (b) I suggest that he provides his definition of an uncertainty study and shows that it yields a better expression of technical bases for decision-making than the definition in [1], and that the calculation in [1] does not comply with it.

In the same paragraph and elsewhere Corradini misinterprets the probability distributions in [1] as distributions of frequentist probability. As explained in Section 2.2 and Appendix A of [1] they are subjective probability distributions.

Paragraph 1 of [2] goes on to say that the distributions in [1] reflect the widest possible range of values without violating obvious physical limits. This is not wholly correct. The following upper limits could have been increased without violating obvious physical limits:

Fraction of core molten	75%
Conversion ratio	5%

Also within the ranges used the widest possible selection of distributions was certainly not used; no narrow or delta function distributions were selected.

The final point in Corradini's opening paragraph is that he interprets the 0 to 1 uncertainty range for the containment failure probability as a 100% confidence interval. I think this is a misleading interpretation. To say that a value U is a 90% upper confidence limit for a quantity is a measure of the consistency of evidence with the value U; it means that if the quantity were greater than U there would be a 10% or less chance of obtaining a mean value of the same number of measurements as were obtained, that was less than the mean actually obtained. Since no measurements have been obtained of the containment failure probability and no value of it is inconsistent with all current evidence, all upper confidence limits (50%, 90%, 100%) for the probability are the same and equal to 1, and all lower limits are 0.

In the second paragraph of [2], point 1 reiterates the comment on sensitivity/uncertainty dealt with above. Corradini does not think that [1] adds much to previous work using similar methodology, presumably a reference to [3]. I suggest that [1] offers the following advances over [3]:

- it provides a reasonable uncertainty range for the containment failure probability which [3] does not;

- it uses improved modelling and

- it does not offer a "best estimate" containment failure probability, because none can be technically justified, while [3] did offer one.

Point 2 in the second paragraph of [2] claims the modelling changes would not have changed the main results of [3]. I think this is wrong. The energy of steam explosions that might endanger the RPV top head is much more strongly dissipated by upper internal structure (UIS) in [3] than in [1]. A 2000 MJ upward moving slug loses up to 1800 MJ to the UIS in [3] (depending on how much of the UIS remains unmelted) but only 233 MJ in [1]. Point 2 then describes the probability distributions of [1] as less physical than those of [3]. This is completely unsupportable as we have no evidence as to the distribution of any random quantities in this problem. However point 2 concludes:

> "I am not all surprised by what was found to be important and the wide variability of the results. Careful engineering judgement would have produced the same conclusions by experimental observations and simple calculations".

This is a comment that one would expect in response to an unsurprising and technically correct calculation. The qualification careful engineering judgement is particularly important (in contrast to random or guessed judgement).

Point 3 of [2] criticises the interpretation that the calculations in [1] lead to a 0 to 1 uncertainty range for the probability of containment failure, and claims that these calculations do not validate this result. If this criticism were correct it would negate a major result of reference 1. I therefore find it remarkable that Corradini offers no arguments whatsoever to set against those in Sections 2, 5 and 6 of [1] and to justify this criticism, which must therefore be seen as completely unsupported. Corradini then writes "In a statistical sense this [ie the 0 to 1 range] is a precise interpretation of the calculations, however in an engineering sense this is a somewhat useless conclusion." (Original emphasis). This appears to contradict the previous criticism but to suggest that the result, even though correct, is useless. I am at a loss to understand this; the correct result may be unpalatable, but an incorrect result would surely be worse than useless.

The other two criticisms in Point 3 relate to the selections of parameter ranges and models in [1]. One is that specific estimates of mixing and conversion ratio were not made. This was not done because evidence does not exist to support any single estimates of these quantities - such estimates would be arbitrary. The other criticism is that models of Fauske, Theofanous and others were not incorporated to see how they would affect the results. These models were carefully reviewed in Section 3.5 of [1] which concluded that in their present form these models cannot be used to restrict the upper limit of melt mass mixed. However several of the cases calculated in [1] effectively do exactly what Corradini is asking for. Case 49 has an upper limit of 93 kg of melt mixed which is similar to the limit proposed by Henry and Fauske. Cases 2, 20, 25, 28, 31, 34, 37, 40, 43 and 46 have an upper limit of 7000 kg which somewhat exceeds Theofanous' limit. In all of these cases no failures were calculated. A deliberate choice was made in [1] not to concentrate on any particular mixing model but to vary the mixing parameters systematically thereby encompassing the various models.

To sum up this reply to Corradini's comments on [1], his comments are mainly adverse criticisms. These criticisms all either arise from misreading the report, or are unsubstantiated.

2. RESPONSE TO BRIGGS' COMMENTS

Briggs has carefully and accurately summarised the line of argument in Reference 1 and has made fair comments. I only wish to take issue with one aspect of his discussion. This is the question whether best estimates can be made of parameters relevant to the steam explosion problem. In Section 7 of Reference 4 Briggs advocates a best-estimate calculation of the mass of fuel involved in steam explosions. He offers two physical reasons why this may be much less than the upper limit identified iin [1]. These are that the pour diameter may be small (for reasons not stated but which presumably relate to non-uniformity of a melt pool) and that steam flow may prevent large mixtures. For balance it should be noted that both of these physical reasons may be vitiated if more than one explosion occurs, as the first explosion could open a large diameter pour and enhance melt-water mixing transiently. In Section 6 he discusses the possibility of a best estimate for the containment failure probability. I regard use of the word "best" to describe an estimate as making the claim that evidence exists which demonstrates in some sense that this particular estimate is better than any other that could have been made of the same quantity. I have not found evidence to support best estimates of either the mass of fuel or the probability of containment failure, either during work in preparation of Reference 1, or in Briggs' comments [4] or in his recent paper on steam explosion probabilities [5]. So I do not see the possibility of defining best estimates for these quantities at the present. Even if "best-estimates" could be identified, this would not in itself alter the uncertainty analysis of Reference 1.

Yours faithfully

Tony Wickett

A J WICKETT

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5. A J Briggs, The Probability of Containment Failure by Steam Explosion in a PWR, AEEW - R 1692 (Winfrith, UK, December 1983).

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To: Members of the USNRC Steam Explosion Review Group

23 November 1984

Gentlemen

Draft Response to Comments on NUREG/CR-3369

As reported in my letter of 9 November I have agreed to provide the Steam Explosion Review Group (SERG) with responses to the comments that Members have prepared on NUREG/CR-3369 [1]. On 20 November I received, in addition to the comments addressed in my previous letter, comments from Bohl and Butler, Catton, Cho, Cybulskis, Fauske et al, Ginsberg, Mayinger, Squarer and Rahe, and Theofanous. The extent of this material testifies to the importance of the subject matter and the industriousness of the Members of the Review Group. The range of approaches offered gives an indication of the uncertainties involved in the subject. Unfortunately the bulk of the documents means that I have not been able to reply to them as thoroughly as their content warrants, in time for the meeting on 27 and 28 November. So these responses do not cover all the topics that I would have liked to discuss. Because of the shortage of time all these responses must be regarded as draft, and I must apologise in advance for any errors, omissions or misunderstandings herein. Also, of course, this letter only necessarily represents my own views.

If the USNRC wishes to conduct a more balanced evaluation of NUREG/CR-3369 then sufficient time should be allowed for its authors to prepare a more considered response to the comments offered. Nevertheless I hope that this response will prove useful to the Review Group and will be happy to clarify any points if anyone wishes to contact me.

1. **Draft Response to Comments by Bohl and Butler [2]**

This is by far the most complete set of comments. It includes detailed discussion of many aspects of the problem and the authors' own work has yielded useful new insights. I will pick out a selection of the detailed points raised and discuss the conclusions drawn.

In Section A, on pages 33 and 34 of [2], Bohl and Butler suspect that the following assumptions will eventually not prove to be valid:

(a) all molten corium can mix with water and explode

(b) a significant fraction of the explosion energy reaches the top head

(c) the head fails at the bolts.

While this suspicion may well turn out to be right, NUREG/CR-3369 evaluates the uncertainty based on today's knowledge. Thus for each of these assumptions we ask "is it within today's reasonable uncertainty range?" Unless we can be reasonably certain it is inconsistent with evidence, the answer is "yes". Consider each in turn.

Assumption (a) seems at first sight implausible, but it must be recognised that although much effort has been expended, none of the several current models are yet ready to show it to be inconsistent with evidence. One mechanism that might short-circuit these models is the occurence of multiple explosions (as observed in several experiments). The interpretation that "we can infer that explosions strong enough to cause bolt failure will involve almost all the melt" of page 40 of [1] is, I think, wrong; it is not necessary for all the melt to mix: this depends on the conversion ratio as shown in Ginsberg's figure 11 [13].

Assumption (b) - transmission of energy to upper head - will of course be mitigated by lower head failure. I am sure Bohl and Butler would readily agree that the SIMMER-II calculation on pages 34-36 which predicts a head force somewhat less than the failure freshold is not bounding. For example, a sharper loading on the upper head might occur if less corium was in the explosion leaving some above it, if the remaining solid core were taken into account or if the conversion ratio were higher.

A careful review of Appendix B of [1] is the basis of the questioning of assumption (c). The conclusion of this review appears to be that it is uncertain whether the head or bolts fail first, and if the head fails whether several fragments come off or not. This is similar to the conclusions drawn in Section 5.2 and Appendix B of [1]. Again, the assumption of bolt failure is not made to the exclusion of alternatives in NUREG/CR-3369; it is said to be within the range of uncertainty.

Section A of [2] concludes (on page 33) that the uncertainty range for the conditional probability should not range up to 1 although it does not provide evidence inconsistent with 1. I do not agree that "the assignment of uncertainty ranges is a highly subjective process" necessarily although I think it is more subjective than I did when we embarked on NUREG/CR-3369. It can of course be subjective; the upper limit of 0.1 apparently derived by Bohl and Butler from a stylised argument that a particular SIMMER-II calculation "undoubtedly requires edge-of-spectrum parameters" (what does that mean?) and from arguments about triggering at high pressure provides an example.

Section B (pages 45-48) questions whether our methodology is useful by suggesting that it does not hold the promise for reducing the range (of probability calculated). This reduction could occur in two ways.

(1) if evidence narrowed some of the uncertainty ranges of physical quantities or

(2) if evidence about frequentist probability distributions of stochastic variables became available; subjective probabilities have to be consistent with such evidence.

So I do not agree that the methodology is insufficient for a risk assessment (page 48); or that a rigorous upper bound (on the probability) does not yet exist.

In Section C (pages 49-56) detailed remarks are given highlighting additional or alternative considerations. Some of these reflect caveats mentioned in our paper. Some of them should indeed be pursued further. I would very much like to discuss some of them but do not have time now.

At the end of Section C, Bohl and Butler "regard NUREG/CR-3369 more as a statement of the consequences of a particular subjective view of initial conditions and physics uncertainties, rather than a cause to claim that new concerns should exist". I do not think our view of the initial conditions or uncertainties is particularly subjective, certainly not to such an extent as to make the conclusions we draw subjective. (After all, they are described as "intuitively obvious" on page 45 of [2]). All our physical limits are supported by reasoning which, as is appropriate for uncertainty ranges, takes the form "values within this range are not inconsistent with evidence". Bohl and Butler rightly pay particular attention to mixing arguments and I would agree that these hold significant potential for reduction of upper limits in the future, but that this has not yet been achieved. Since some of the mixing arguments contain effects of scale, some larger experiments will be needed to test whether the arguments correctly model scaling effects.

I also do not think our paper claims that new concerns exist; after all, the α-mode has been in the literature for nine years and the probabilities associated with it have always been regarded as uncertain by careful reviewers.

The other area that Bohl and Butler concentrate on in the review is whether a "big steam explosion" can generate a large missile, the main topic of the Los Alamos steam explosion programme. This new work is to be welcomed as a new attack on one of the important areas of uncertainty found in NUREG/CR-3369 and elsewhere. It should be noted that while the SIMMER-II code may well be a suitable parametric "source term" of explosions for this study, it has features which make it unable to extrapolate from 20 kg explosions (used to calibrate parameters in the code) to thousands of kilograms without introducing uncertain ies. These include the treatments of particle size distributions, fragmentation in an explosion, heat transfer coefficients and slip between condensed phases. Thus it does not obviate the need for large scale experiments to test modelling of both mixing and explosion efficiency.

2. Draft Response to Comments by Squårer and Rahe [3]

I will here discuss two of the general comments and some of the specific ones in [3].

Squarer and Rahe say of the "0 to 1" range "of course ... it is useless". This would be so if it were wrong; otherwise a narrower (wrong) range would be worse than useless. Our aims should be to narrow it, recognising what it is at the outset. The physical arguments in WASH-1400 do not support a narrower range.

In their second general comment Squarer and Rahe identify case 1 of NUREG/CR-3369 as a "best-estimate". It may be Squarer's and Rahe's, but it is not ours, for the reasons set out in Section 6 of [1]. That argument is expressed in terms of case 3 but applies equally to case 1. No evidence exists (none is identified in References 1 or 3) to support preference for case 1 over any other or to support the equivalent operation of averaging over 3^5 possible cases using the 1/3 subranges. Anyway, the numerical result from case 1 depends on the modelling and parameterisation chosen (given the identified physical uncertainties) as explained in Section 5.5 of [1]. Thus the arguments in [3] do not justify a narrower range than 0 to 1.

I now turn to a selection of the specific comments in Section 2.2 of [3]. The following lettered paragraphs refer to similarly lettered ones in Section 2.2 of [3].

(b) Pour diameter 3.4 m too high.

We took considerable time in trying to justify a lower upper limit without success. Remember that in the case of second or subsequent explosions all assumptions about initial geometry of a melt pool or crust are uncertain. Reference 3 does not provide a lower figure or justify it.

(c) All melt in water participates.

This assumption calibrates the Sandia definition of "conversion ratio" used in [1]. Had the Winfrith definition been used to analyse Sandia experiments, I expect higher values would have been obtained for some tests. This would have affected the ranges we used, in our calculations, for conversion ratio. Also, if we had used the Winfrith definition, this would have entailed another unknown parameter, fraction of melt in water that participates, for which an uncertainty interval would have had to have been justified for large explosions. Taking these two changes together, I expect the overall conclusions of the Monte-Carlo study would have been unchanged.

(d) Water subcooling

First, we will list the thermite experiments in hot water cited by Squarer and Rahe in Section 1.1 of 3.

Test	Melt (Mass Delivered)	Water Subcooling	Result	Reference
13	Iron-alumina thermite (4.4 kg)	0°C	Two spontaneously triggered explosions	4
23	Iron-alumina thermite (6.9 kg)	0°C	Two spontaneously triggered explosions	4
CM-8	Iron-alumina thermite (18.6 kg)	2°C	Delayed surface interaction (about 20 cm penetration) later "moderate" explosion (about 10 cm shards)	5
CM-9	Iron-alumina thermite (18.6 kg)	2°C	Delayed surface interaction (15-20 cm). Later "moderate" explosion	5
OM-4	Iron Oxide (~10 kg)	4°C	Surface Interaction followed by strong bottom-triggered explosions	6
SUW07	Urania-Molybdenum Thermite (24 kg)	0°C	Three explosions	7

These experiments with three different thermite compositions and hot water all yielded multiple explosions.

Next we quote the assessment given in Reference 5 more fully than the selective quotation in Reference 3:

"If surface interactions occur in saturated water at ambient pressure in the reactor case, as they have in the CM tests, then it may be very difficult to have a large-scale steam explosion, under those conditions. It is also possible that such interactions in a confined geometry could enhance mixing, which could subsequently lead to a large-scale explosion. In either case, a possible process might involve multiple or continuous vigorous interactions. The rates of steam and hydrogen generation could be very rapid."

This is consistent with the experimental evidence cited: spontaneous triggering with hot water could suppress or encourage large steam explosions. This covers the same range of possibilities as was used in NUREG/CR-3369.

(e) Mixing limitations.

See my discussion of "assumption (a)" in the response to Bohl and Butler above.

(g) Conversion ratio.

What upper bound would Squarer and Rahe justify instead of 16%, and how would they justify it? Note that this figure represents the possibility of increasing the conversion ratio by a factor of ~5 from current measurements while the mass increases by a factor of ~1000 - a change with scale that seems not implausible a priori. Of course the conversion ratio may not increase with scale - most of the calculations in NUREG/CR-3369 used 5% as an upper limit. But in an uncertainty study the effect of the higher figure needs to be considered as well.

(h) Slug Composition.

Rapid acceleration and deceleration of the slug might collapse voids. The range of void fractions used in our paper was 0-75%. Bohl and Butler [2] found it reasonable.

(i) Sampling Scheme.

The choice of sampling scheme is essentially arbitrary, so this means that:

(a) special significance should not be attributed to any single calculation (for example as Squarer and Rahe do for case 1) and

(b) care must be taken to ensure that the overall conclusions are not artifacts of this choice. This is assured in Sections 5.5 and 6 of [1].

(j) Results of Main Calculations

It would be interesting to see reasons to support the characterisations here of various quantities as "reasonable", "unreasonable" and "conservative". Which meaning of "conservative" is being used here?

(k) Other Areas of Uncertainty.

All the items listed here are accounted for in Sections 5 and 6 of [1]. A careful reading of Sections 5.5 and 6 will reveal that the range of uncertainty in containment failure probability and the other conclusions of the study are not in fact due to the choices of modelling, parameterisation and probability distributions made, but reflect the uncertainties identified in physical processes.

(l) Effect of Scale

The Winfrith experiments have compared conversion ratios over the range from 0.5 to 24 kg. To narrow the range of uncertainty at larger scales, experiments at those larger scales are needed to distinguish between different possible dependencies on mass.

3. <u>Draft Response to Comments by Catton [8]</u>

In several instances Catton argues that we [1] should have done more, or more detailed, calculations to have produced different modelling for phenomena, different values for some quantities and possibly some narrower uncertainty intervals. It is not always obvious exactly how much detail to use when modelling a collection of processes, but when conducting an uncertainty study like NUREG/CR-3369 excessive detail is completely unhelpful. "Excessive" here means extra detail that does not narrow the uncertainty range. For example under "<u>Fraction of Core Molten</u>" Catton writes "Before one tries to argue for some fraction of the core being molten one ought to do some calculations. If one postulates that the molten pool is surrounded by a crust then calculational tools are available to calculate heat transfer to its boundaries." Details of severe fuel damage progression are well-known to be highly uncertain and the detailed structure and composition of a crust (if one forms) will be the result of the various preceding stages in its evolution. Its chemical composition and porosity will affect both its thermal conductivity and its mechanical properties and hence its subsequent evolution. Once the uncertainty in these factors is allowed for, the thickness of a crust at a particular stage in its evolution is likely to be very uncertain, so the vario cases in [1] probably cover the range of possibilities. An important narrowing of this range would be achieved if evidence were found that crust formation cannot occur; but that evidence does not, I think, exist today and is unlikely to emerge from purely code studies. In conducting an uncertainty study, too detailed modelling can be dangerous by confining the range of possibilities beyond that justified by evidence and hence producing spuriously narrow uncertainty ranges. I am not arguing that more detailed calculations should not be done - they should, and I hope Catton's enthusiasm will lead him to do some. But they do not necessarily help an uncertainty study.

It is interesting to compare what Catton says about our probabilistic analysis with what he says about his own. Of his own calculations he says "Calling the estimates made in the above arguments probabilities is an insult to those who deal in statistics. Rather, engineering judgement (mine) has been invoked to choose a set of numbers that will give the fraction of core melts that might result in a serious steam explosion". It is reasonable to suppose, from this, that he would agree that this fraction is not necessarily exactly equal to 5×10^{-5} at low pressure or 10^{-4} at high pressure. However, he gives us no indication of how to estimate the uncertainty in this fraction. To make these estimates useful this uncertainty has to be stated. His main complaint about the probabilistic method used in NUREG/CR-3369 is that conditional probabilities are not used. This is another example where, to attain the goal of an uncertainty study, the introduction of extra detail would have to be accompanied by extra uncertainty analysis of that detail. The conditional probabilities would have to be varied within their justified uncertainty ranges (the ranges of values not inconsistent with current evidence). These ranges would include the ones used in the present study (which are consistent with evidence). Thus the present study can be regarded as a subset of that larger study and so cannot yield uncertainty ranges wider than it. Additional discussion of this point will be found in Section 5.4 of [1].

These general points cover many of Catton's specific points but I will comment futher on a few of them.

Conversion Ratio - Calculations of the kind suggested, done by others, are used and referenced in Section 3.7 of [1].

Fraction of Water Above the Explosion - downcomer effects are, in fact, discussed in Section 5.3 of [1].

Slug Composition - The idea of a slug consisting of water alone is old-fashioned: it will contain corium and other debris from above the explosion as well, probably.

Slug Impact Model - Catton's discussion about the impact pressure is correct and that is why we did not use ρuc, but ρu^2. See Section 3.14 of [1].

4. Draft Response to Comments by Theofanous [9]

In his comments, Theofanous places reliance in a single argument concerning geometrical constraints on hydrodynamic breakup set out in Reference 10. Unfortunately in drafting NUREG/CR-3369 we misinterpreted this argument as being closer to the steaming arguments of Henry and Fauske than Theofanous and Saito intended. An apology is due to these authors for this misinterpretation.

Theofanous describes his model as a simple and transparent concept and asks whether we have any reservations about it. My own reservations are set out in a document reviewing Reference 10 which I gave to Professor Theofanous on 29 April 1982 [11] and which was sent to the USNRC on 3 May 1982 [12]. This review says:

"Theofanous and Saito rely upon mixing and triggering arguments to limit the quantity of melt efficiently participating in an explosion. They propose a reasonable upper limit, 0.1 m, for the length scale of regions of melt that can effectively take part in an explosion. In a flooding mode of contact they assume that this thickness of the melt's surface participates. In a pouring mode, they apply hydrodynamic stability criteria suitable for a jet of one fluid flowing through another to limit the extent of jet break up, and then estimate the quantity of melt within 0.1 m of the resulting interface with water.

The weakness of this argument, particularly in the pouring mode, is that it neglects the effect of the production of steam. Formation of bubbles will cause extra unordered velocities near to the interface, enhancing break-up. They will also promote stirring and hence reduce settling out from a mixed pool. Note that a sphere of melt 1m in diameter at 3000 K and having an emissivity of 1 radiates enough heat to produce 20 times its own volume of steam per second at atmospheric pressure. By neglecting this flow of steam their argument may substantially underestimate the rate and overall extent of effective mixing."

Other mechanisms which might lead to more mixing than predicted by Theofanous' model are breakup of a melt pour by structure in the lower plenum as discussed in Section 5.3 of [1], and multiple steam explosions as often observed with hot water (see Section 2 of this letter and Section 3.5 of [1]).

Theofanous also questions some of the numerical results in [1] in a postscript. The discrepancies noted arise where the mean explosion energies in two cases are similar but the calculated failure probabilities are different. This can, I think, be attributed to different widths of the distributions of the quantities relevant for failure; the failure probabilities calculated arise from the high-energy or high-stagnation pressure tails of these distributions. This is the same as the interpretation offered by Theofanous.

5. Draft Response to Comments by Ginsberg [13]

At the bottom of page 17 Ginsberg discusses the probability density function (PDF) for the vessel head kinetic energy as it hits the containment dome in one of the cases calculated in Reference 1. He writes "The usefulness of this computed function depends entirely on the validity of the input probability distributions of the individual variables". I would say "validity" where Ginsberg says "usefulness" but the main point about this statement is that, provided the bounding values of the PDF's for the various uncertain variables on which the head kinetic energy depends do not stray into regions inconsistent with evidence, there is no way of telling that one PDF is more or less valid than another. Thus a range of PDF's for each variable has to be used, as in NUREG/CR-3369. Later the idea of "best-estimate" PDF's is postulated. I do not think these exist since we do not have evidence that one PDF for the variables is better than all others. Based on the idea of a single set of PDF's for the variables, Ginsberg says we can obtain, by the Monte Carlo method:

(i) The probability that the kinetic energy exceeds a given failure criterion and

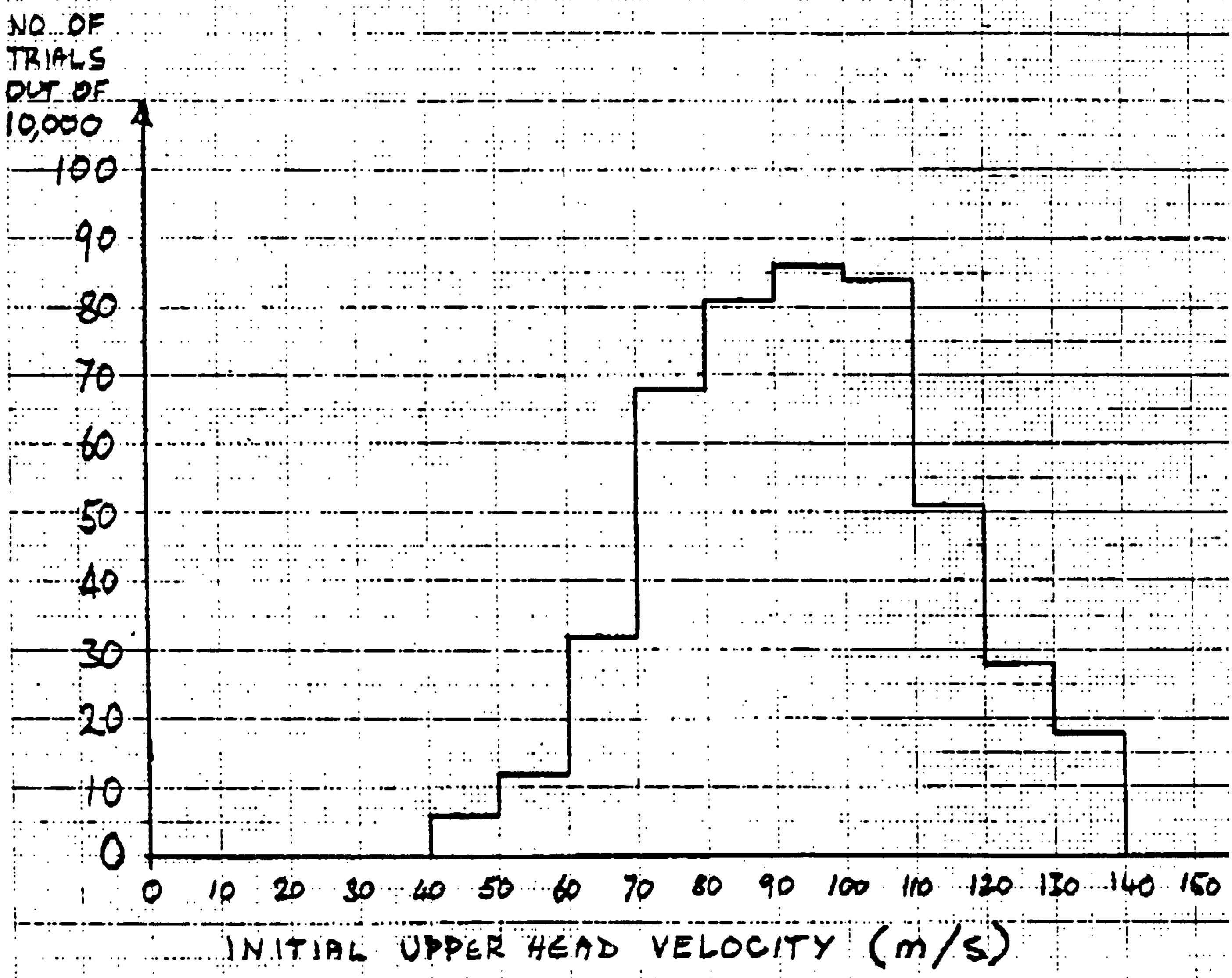
(ii) the uncertainty in this probability.

Item (i) is clear; it depends on the choice of PDF's for the variables. But I do not understand item (ii). Does this refer to the statistical variability introduced in a Monte Carlo sample of N trials, $\sim\sqrt{N}$? How can a single set of PDF's yield the uncertainty in a probability which is determined by integrating over these PDF's? This idea is called into question by the immediately following discussion of sensitivity analysis to be performed if we are not sure of the individual PDF's. I am attaching a plot of the distribution of initial head velocities in Case 1 of NUREG/CR-3369, to assist clarification of this question.

Ginsberg interprets case 1 of our paper, the "full width" distributions, as our "best estimate" or "best guess" set of distributions. We would not accept this interpretation for the reasons set out in Section 6 of our report. That reasoning is expressed in terms of case 3 but applies equally to case 1. We do not have evidence to show that one set of PDF's is better than any others, so we do not have a best estimate.

Subsequently Ginsberg characterises the variational calculation in our report in these terms: "It should come as no surprise that if one selects extreme values of the input variables for the calculations that one will compute extreme values of the containment failure probability". This is not what we did. He later describes the bounds of the individual distributions as "in general, reasonable". In the Monte-Carlo calculations no case corresponds to selecting extreme values because:

DISTRIBUTION OF INITIAL UPPER HEAD VELOCITIES IN CASE 1 OF NUREG/CR-3369



IN ADDITION TO THE 466 TRIALS PLOTTED HERE, THE REMAINING 9534 DID NOT HAVE BOLT FAILURE.

(a) distributions over 1/3 of each range were used, rather than concentrating more towards the extremes and

(b) in most calculations the upper limit on the conversion ratio was set at 5% in contrast with the justified valve of 16%. (Note that Ginsberg's best estimate for this parameter is 5%).

Ginsberg complains that judgement on the part of the authors, as to what the most likely distributions would be, is missing. Such judgement is out of place in an uncertainty study, which aims to find the range of possibilities consistent with evidence. All the distributions and the corresponding probabilities in our study are consistent with evidence, and evidence does not make any of them more likely than any other.

In discussing the parameter and probability distribution estimates of Reference 1, Ginsberg says: "The estimation of parameter ranges and probability distributions is to a large extent subjective". I would contrast the estimation of parameter ranges, which is based on reasoning from evidence and is somewhat subjective, with the assignment of probability distributions which is of course wholly subjective.

Ginsberg suggests that we should have biassed the melt mass parameters towards the lower end of the distributions as a "judgement call". Again this would be outside the scope of an uncertainty study aiming to cover the ranges consistent with evidence. To introduce such a systematic bias would be to exclude from the study the possibility that all the mixing arguments are wrong and that some mechanism (such as multiple explosions) acts to encourage large mixed masses. We calculated enough cases so that readers who wish to make their own "judgement calls" can do so by attaching their own different weights to them.

May I add a few words on Ginsberg's analysis of question 1? His treatment is refreshingly independent and offers some interesting new insights. It is clearly written and appropriate qualifications are expressed. The main new feature is the description of melt-water mixing influenced by the actual structure in the lower plenum. This description leads to relatively low limits of mixing - 1250 to 3700 kg "best guess" and 15,000 kg "the outside". As a point of clarification, does the liquid jet breakup calculation at the bottom of page 5 assume the steam is at rest relative to the water, or has a velocity equivalent to its steady state production rate? The illustrations in his Figure 6 strongly suggest the possibility of early triggering leading to multiple explosions particularly in hot water and at low pressure. In Section 2.4.1 Ginsberg assumes that, if there are more than one explosion, the first will be the most energetic. This assumption must be regarded as highly uncertain; for example if a 100 MJ explosion involving 1250 kg in accordance with Ginsberg's best-estimate occurs this will:

(a) leave most of the melt unconsumed and

(b) yield enough energy to mix the RPV contents up thoroughly.

6. Draft Response to Comments by Cybulskis [14]

Cybulskis makes a number of general and specific points. Many of these support, or add interesting extra discussion to, the analysis in Reference 1. Cybulskis' comments do not support his conclusion that the report [1] "does little to define uncertainties associated with in-vessel steam explosions".

7. Draft Response to Comments by Cho [15]

Cho has formed the impression that the authors of NUREG/CR-3369 assume that the low, middle and high thirds of the uncertainty range are equally probable. This is not quite correct; we make no assumption regarding their relative probability. This is because evidence does not exist to justify such an assumption.

In reply to Cho's final point, the improvements claimed over the previous study [16] are listed in Section 1.3 of [1]. These are that the uncertainty has been evaluated in a more complete and systematic manner, and that modelling improvements have been made. In my view the most significant of these modelling changes are the revised treatment of dissipation in upper internal structure and the slug-head impact model. These are described in Sections 3.13 and 3.14 of Reference 1.

8. Draft Response to Fauske et al [17]

These authors rely on two arguments to conclude the vessel damage by steam explosions can be ruled out.

The first argument is that insufficient premixing will occur. They define premixing as requiring fuel particle diameters in the range 1-10 mm. This is rather smaller than values used by other authors (see other submissions to the SERG) and is not stated to be a prerequisite by Gittus et al [18] whom Fauske et al cite. Unfortunately I only have the left hand 60% of their page 6 which gives quantitative details of their calculations, so I cannot comment on these details. However it would appear that this calculation does not allow for some possible phenomena identified in Reference 1, in other submissions to the SERG, and elsewhere in this letter. These include:

(i) possible enhancement of mixing due to steam production

(ii) the effect of structure in the lower plenum which might break up gross flows of melt and

(iii) multiple steam explosions.

The second argument of Fauske et al is that Taylor instability will break up a slug of melt after 2.6 m of travel. I have not seen the report cited that calculates this length, so cannot comment on this calculation. Substantial acceleration may still occur over the 2.6 m of travel as the driving pressure is greatest during this phase, however. The presence of internal structure, particularly core debris, might counteract breakup by adding extra material at the front of the slug. This argument will require more careful consideration.

9. Draft Response to Mayinger [19]

It is impossible to do justice to this interesting paper without also studying the original references cited. These report extensive work from Germany. Unfortunately I have not been able to do this.

Mayinger uses a model by Burger et al (his Reference 23) to predict that:

(a) Steam explosion detonation waves cannot develop at system pressures in excess of 2 MPa and

(b) The maximum pressure in such a detonation wave cannot exceed the vessel design pressure (about 20 MPa).

This model seems inconsistent with the following peak pressures observed in steam explosions at Sandia National Laboratories:

Test	Peak Pressure (MPa)	Pulse Width at Half Maximum (ms)	Reference
MD18	111	4	20
MD19	18	0.1	21
MDC2	100	15	22
FITS1B	29	0.2	20, 23
FITS4B	60	0.5	20
FITS8B	40	0.7	20
FITS9B	63	2.5	20
CM7	70	0.6	20, 24
RC2	70	1.3	20
SPIT17	200	0.15	25

If prediction (b) above were correct then it could be argued that any steam explosions could be contained by an RPV. These data contradict prediction (b) and so also remove support from prediction (a).

Mayinger quotes work by Körber who calculates the maximum mass of melt that can participate in a steam explosion as 2000kg. This appears to be due to limits on crust stability, flow area and pour length. Without more detail it would be unhelpful to try to comment further on any uncertainties in this calculation.

The other calculation quoted by Mayinger is by Wagler et al who use a code KODEX to describe large steam explosions. The prediction that the RPV will withstand an explosion involving 50,000 kg of melt is reported. It is not clear whether this is due to small conversion ratios or small peak pressures (as discussed above). Again, study of the original references would be necessary to permit comments on any uncertainties in this prediction.

10. General Remarks on Probability Estimates

Some of the submissions discussed in this letter accompany estimates of the probability of containment failure due to steam explosions. Berman has classified ways of forming such estimates; most of those submitted to the SERG are in (or are similar to) Berman's classes 1 and 2 [26]. Class 1 estimates are based on physical arguments that the probability is exactly zero. I have reviewed several such arguments in this letter and concluded that they are not yet justifiable, in agreement with several members of the Review Group. The class 2 estimates divide the containment failure process up into a number of steps and assign a subjective probability to each step. These probabilities are conditional on the appropriate outcomes of previous steps. They are combined by multiplication and, if the sequence of events has been described by a tree, addition. Berman characterises probabilities generated in this way as uncertain, arbitrary and useless [26].

Here we will consider features common to the estimates made by Bohl and Butler [2], Briggs [27], Catton [28], Ginsberg [13] and Squarer and Rahe [3]. All these authors describe their estimates as uncertain and subjective in differing but more or less direct terms. Two of the assessments are claimed to be biassed, in the pessimistic direction, those of Briggs and Catton. One assessment, by Bohl and Butler, presents a range intended to account for uncertainty.

It is generally accepted that quantitative advice for decision makers should include a statement of its tolerance or uncertainty. This protects both adviser and advised against decisions taken based on a single estimate that gets changed by subsequent new evidence to one which would have led to a different decision, provided that the uncertainty range covers all values consistent with evidence. (More precisely, the reasonable uncertainty range, that is the smallest possible range of values for which there is not reasonable certainty that they are inconsistent with all available evidence, is required. Note that absolute certainty is not required.) Single estimates for uncertain quantities or uncertainty ranges that do not cover all values consistent with evidence do not therefore constitute a full "technical basis for decision-making". However some of the above-mentioned authors have not offered uncertainty ranges. Also their single numerical estimates are not all the same. The evidence presented does not favour one estimate over the other. These estimates therefore open to Berman's description as "arbitrary". Because of the danger inherent in technical advice in the form of single estimates of uncertain quantities, they may also be described as "useless" as a technical basis for decision making.

The two estimates claiming to be biassed do not offer evidence to show that more pessimistic values are inconsistent with evidence. Thus the "biassed" estimates are not necessarily the same as the upper limit of the reasonable uncertainty range, so they do not give useful information in the sense described above.

Of the two estimates offering uncertainty ranges, that by Bohl and Butler uses a stylised scheme of argument. Their upper limit, 0.1, may be traced to two parallel arguments - (a) that at high pressure triggering is unlikely and (b) that at low pressure parameters in a certain SIMMER-II run are "edge-of-spectrum vaues" - which are used to place an upper limit on the probability at a step in the sequence. In neither case does the form of the argument assert that higher values are inconsistent with evidence and so the resulting upper limit does not give useful information in the sense discussed above.

The other uncertainty range may be found in Section 4.1 of Briggs' assessment [27]. He carefully discusses the nature of his probability assignments at each step and considers as an example:

"A case in which two different assumptions concerning the theoretical modelling of the mixing process lead to either failure or survival of the vessel. If an 'expert' considers the set of assumptions leading to survival to be reasonable, and the other set to be extreme and improbable (but not impossible), he could assign a high probability to survival, say 0.9 (or 0.95). This example illustrates a number of aspects of probability values obtained using engineering judgement".

One of these aspects is:

"The probability value is an estimate of the probable outcome of such an event, but it does not necessarily represent an estimate of the relative frequencies of events in a large trial. In the example the uncertainty could relate to the controlling phenomena in large systems, and it is possible that a large trial would establish that the system always behaved in one particular way, hence the expected result of a large trial could be 100% survival or 100% failure. However, a probability of 0.9 could still be assigned to success before such a trial were conducted."

This implies a reasonable uncertainty range that is based on the nature of the evidence available, is not arbitrary and is useful as a technical basis for decision-making in the sense discussed above.

11. Summary

This letter is a "quick look" response to submissions to the SERG and does not necessarily cover all points raised in the reviews of NUREG/CR-3369.

Some submissions argue that steam explosions strong enough to fail the RPV are impossible. I have argued that the evidence is not yet strong enough to support that conclusion. Several reviewers identify mixing arguments as limiting the size of steam explosions. It is agreed that these have the clear potential to do so, but at present none of them is sufficiently certain to allow it to place a limit on the amount of participating melt. In particular the effect of multiple explosions on these arguments needs to be considered.

Some reviewers said that we did not exercise enough judgement in NUERG/CR-3369. Although our choice of modelling and bounding values involved judgement, we did not use it to bias our probability calculations. Had we done so we would have run the risk of failing to meet our objective of estimating the reasonable range of uncertainty in the probability of containment failure, that is to say the range of values consistent with evidence. A narrower range, determined by our judgement, would be an arbitrary subset of the full, reasonable uncertainty range. No-one has shown how to avoid this.

Several potential reasons for drastically reducing this uncertainty, perhaps to zero, have been identified in NUREG/CR-3369 and by its reviewers. It is hoped that research with the potential to establish one or more of these reasons can be put in hand.

Yours faithfully

Tony Wickett

A J WICKETT

(1982

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8 January 1984

Dr Cardis Allen
Executive Secretary
Steam Explosion Review Group
US Nuclear Regulatory Commission
Washington DC 20555
USA

Dear Cardis,

I have just received your Preliminary Draft of Steam Explosion Review, dated December 26. While it generally does a good job, it does not state my position properly on p 15, nor, I believe, that of Theofanous. Our calculations indicate, more strongly than any other evidence, that the threat of containment failure due to an in-vessel explosion is essentially negligible. My position is that the SEALS program is certainly premature at this time, and for some time in the future, for the reasons outlined in Para 1 on p 15. I think Theofanous expressed exactly the same sentiments. Based on our calculations, I would add my name to those of Fauske and Mayinger in the last sentence.

The FITSX and ACM experiments (< 50kg) may be useful, but more should be said about improved experiment design compared to the work to date. In my opinion very little solid information has been obtained of a quantitative nature, due largely to inadequate instrumentation and non-prototypical geometries.

Sincerely yours,

S. George Bankoff

S George Bankoff.

Los Alamos

Los Alamos National Laboratory
Los Alamos, New Mexico 87545

DATE: January 14, 1985
IN REPLY REFER TO: Q-6-85-155 (R13K)
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Safety Assessment

Mr. Cardis Allen
Executive Secretary, SERG
Office of Nuclear Regulatory Research
US Nuclear Regulatory Commission
Washington, DC 20555

Dear Cardis:

These are my comments on the draft summary report of the SERG as requested by your letter of December 26, 1984. The first two paragraphs discuss general comments; the rest of the letter treats detailed comments.

Overall, the report does represent the view of the SERG as I understand it. I would have preferred the SERG to have produced more scientific analysis and less unsubstantiated opinion, but the result obtained is probably the only possibility given the time and funding constraints. Perhaps a response at this level of sophistication is what the NRC actually wants?

There is a contradiction in the SERG viewpoint in (a) rejecting the Sandia National Laboratories (SNL) report because of its exaggeration of uncertainties and stating positively that the conditional probability of containment failure from a steam explosion is low while at the same time (b) concluding that additional research is required and inserting a summary appendix that mentions many uncertainties. Perhaps the problem is that the question of what constitutes an acceptable level of uncertainty was not addressed by the SERG. I believe the paragraph that mentions the development of a detailed strategy for reducing uncertainties also should mention the necessity of defining what constitutes an acceptable point to terminate steam explosion research.

In the first paragraph of the Executive Summary, I believe that the major argument for steam explosions producing insufficient energetics should include the necessity of properly considering the dissipative mechanisms in the post-explosion expansion. The approximate 2000-MJ containment failure threshold depends on a upper-head loading produced by a two-phase spray.

In the second paragraph of the Executive Summary, I would prefer that the last sentence say ". . . the authors do not construct physical models for sampling which are representative of current thinking within the technical community." Unfortunately, although the thinking exists, models representing such thinking need to be constructed for the approach to be useful.

In the third paragraph of the Executive Summary, the priority and advisability of larger scale experiments are an issue. If additional steam explosion tests are to be performed, larger scale tests should have priority in my opinion. Only at large scale can it be claimed that the qualitative effects of fluidization and coupling length will be observed to limit the participating melt mass and the energy conversion, if indeed such limiting phenomena act to the extent currently predicted.

In the second paragraph of the introduction, the relevant time scale for heat transfer may be as long as that over which the pressure acts to accelerate the fluids and debris. Current calculations suggest that this time scale should be tens of milliseconds if a containment failure possibility exists. This is the time scale from initiation of a significant "explosion" until peak loading pressures are observed on the upper head.

In Sec. 2.1, it may not be meaningful to mention that the Indian Point-2 and Peach Bottom systems were chosen for analysis. In general, this instruction was ignored.

I do not believe the numbers discussed in Sec. 2.1.2 deserve the identification of "mechanistic calculations." I would call them phenomenological estimates and reserve the phrase "mechanistic calculations" for integrated code calculations from MELPROG or CORMLT when these codes eventually have the capability of performing a core meltdown calculation with some type of steam explosion.

Your tables do a reasonable job of summarizing the probabilities formulated by the SERG.

Sec. 2.2 does appear to represent the opinion of the SERG regarding the NUREG/CR-3369 document. However, I am concerned that the SERG recommendation for formulating more detailed probability density functions (pdfs) may not be possible if this task is to be done with objective criteria based on current technology. Engineering judgment will be required. Indeed, the amount of engineering judgment may be such as to predetermine the outcome of any Monte Carlo study, reducing its cost effectiveness. I would not like to be assigned the project of trying to justify revised pdfs and associated correlations. Could I suggest that the use of this Monte Carlo method may be premature given current knowledge?

In Sec. 2.3.1, the report should say why a strategy for reducing uncertainties is required. This returns to defining what are acceptable residual uncertainties.

In Sec. 2.3.2, I disagree with the unconditional recommendation given FITSX. As a consequence of the SERG meeting, I believe that the proper priorities for additional alpha-mode research (if any) should be (1) modeling of the meltdown process, (2) large-scale steam explosion experiments, and (3) smaller scale experiments to address specific mechanisms. The reasons for this judgment are as follows.

1. Meltdown understanding would add confidence to the decisions that have been made on more serious (higher probability) issues than steam explosions.
2. Both the state of any corium pool before contact with water in the lower plenum and the mode of contact depend on the meltdown sequence, which is uncertain.
3. The state of vessel structure internals as well as the temperature of the vessel head and head bolts depend on heat transfer during the core heatup (meltdown) phase of an accident.

4. Both SIMMER calculations and the theories of Corradini, Fauske, and Theofanous lead to the conclusion that large-scale explosions will possess characteristics that differ from the smaller scale FITS tests. These differences are sufficiently qualitative to be observable with current technology.

I would like to see the figures for Appendix D. I did not necessarily agree with the implications that could be drawn from the preliminary figures sketched at the SERG meeting.

Please feel free to call me if further explanation on any of these comments is required. Even as it stands, the summary report represents a reasonable effort at a very difficult job.

Sincerely,

W. R. Bohl

W. R. Bohl

WRB/jl

Cy: J. L. Telford, NRC
L. H. Sullivan/J. R. Ireland, Q-DO/RS, MS K552
T. A. Butler, Q-13, MS J576
R. A. Haarman/W. S. Gregory, Q-6, MS K557
C. R. Bell, Q-6, MS K557
CRM-4 (2), MS A150
Q-6 File

0305 63111

FROM 0305 63111 '85.01.16 13:02

TO: (Block Capitals) MR C ALLEN, USNRC, WASHINGTON DC

Date: 16

1150

REPEAT TO: (Block Capitals)

Number: 512

From (Block Capitals)			Signature	
Mr A J Briggs			AKFern	
Building	Room	Tel. Ext.	Date	Time
B30	036	2091	16.1.1985	8.25

PLEASE USE BLOCK CAPITALS FOR TEXT OF MESSAGE

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*delete as appropriate.

SUBJECT COMMENTS ON DRAFT SERG REVIEW

THE DRAFT GIVES A FAIR SUMMARY OF THE MEETING AND I HAVE ONLY MINOR COMMENTS.

1. PAGE 1, LINE 18. THE FRACTION COULD BE LESS THAN 10%, HENCE I WOULD PREFER EITHER "GREATER THAN 5%" OR "PROBABLY GREATER THAN 10%".

2. PAGE 13. SECTION 2.2.2. SHOULD WE ADD A COMMENT THAT THE CRITICISMS OF NUREG/CR 3369 MADE BY SERG WERE NOT IN GENERAL ACCEPTED BY THE AUTHORS OF THAT REPORT. WILL THE RESPONSE TO THE SERG COMMENTS MADE BY THE AUTHORS BE INCLUDED IN THE APPENDICES?

3. TABLE 3. SPLITTING MY PROBABILITY INTO THREE PARTS IS AMBIGUOUS, BUT I WOULD PREFER THE 0.02 TO BE DISTRIBUTED AS 0.05 FOR MIXING AND CONVERSION, AND 0.3 FOR SLUG DYNAMICS. THIS IS QUITE CONSISTENT WITH YOUR PRESENTATION IN TABLE 4. OTHERWISE I CONFIRM THE TABLES CORRECTLY REPRESENT MY POSITION.

BEST WISHES

TONY BRIGGS

W9941

LOS ANGELES: SCHOOL OF ENGINEERING AND APPLIED SCIENCE
MECHANICAL, AEROSPACE AND
NUCLEAR ENGINEERING DEPARTMENT

January 29, 1985

TO: Cartis Allen

FROM: Ivan Catton

SUBJECT: Preliminary Draft of Steam Explosion Review

After numerous re-readings of the report I keep coming to the conclusion that if you want a meaningful consensus of the experts, you must weight their estimates more where they are known to have done work and less where they are admittedly being speculative. For example, Theofanus and Bankoff have been trying to understand mixing and pre-mixing (maybe they do) whereas my own research efforts have dealt with core melt phenomena and acceleration of high density fluids by a low density fluid.

A way you might initiate such a weighting procedure is to ask each expert to give himself a grad (1 to 10) representing his knowledge of the various facets of the α-mode containment failure. Use the grade as a weighting factor to obtain a more meaningful consensus. I realize this approach sounds hokey but I don't know of any other way you can incorporate special knowledge and lack of into your bottom line. Another approach to the weighting process might be to have a separate review board made up of say Peter Griffith, Sol Levy and Virgil Schrock for the purpose of digesting and weighting the opinions you have in hand. Here, unfortunately you may find that the review board thinks we are all hackers. Of course, you could also do the weighting yourself.

I think we need to keep in mind that a probability of 10^{-2} or less is low enought to allow us to approach the question of steam explosions in a rational way. How low a probability the α, mode must have for it to be an outliser in risk space has been the subject of studies at BNL and I have a vague notion that it could be a bit higher before it becomes an important consideration. This does not mean we should not try to understand the processes leading to a steam explosion. Rather, we should lay out a research program that properly deals with observations already in hand. If we don't fully understand the 25kg drop lets not be foolish and recommend a 1000kg or 2000kg drop.

The SERG chairman, vice-chairman and executive secretary have the responsibility to produce a summarizing view of the group members. The summary need not be filled with cross referencing for self protection. If any of the SERG members disagree with the summary they should then be allowed to have a dissenting opinion included in an appendix.

As a final note, I did not say the SANDIA 2000kg tests should never be conducted. Rather, I said there are certain basic physics needing understanding before any more large scale testing is conducted. My position is only a "no" if this understanding is not to be sought first.

I would like you to forward my thoughts to all SERG members.

ARGONNE NATIONAL LABORATORY
9700 South Cass Avenue, Argonne, Illinois 60439

January 9, 1985

Mr. Cardis Allen
Executive Secretary
Steam Explosion Review Group
U.S. Nuclear Regulatory Commission
Washington, D. C. 20555

Dear Cardis:

In response to your request I have reviewed the preliminary draft of the steam explosion review. Overall, the report looks fine. I would like to commend you, Ted, and Mike for the job well done. I only have a few minor comments, which are summarized below.

- I would prefer slightly re-phrasing the last sentence of Executive Summary as follows. "The major factor in this conclusion is that the probability density functions used in their analysis are highly subjective and do not seem to reflect current thinking within the technical community in a comprehensive manner." Of course, you and Ted will have the final say.

- I am not sure whether Table 4 is necessary. Isn't Table 3 sufficient?

- I agree completely with the last paragraph of Section 2.3.1 appearing in page 14. My support of the SEALS program was, in fact, based on my own concept of a strategy for reducing uncertainties. I thought the SEALS program would contribute to an early resolution of the major uncertainties associated with the scale effects.

Sincerely,

D. H. Cho
Reactor Analysis and Safety Division

DHC:kj

cc: Dr. T. Ginsberg, Brookhaven Natl. Lab.

Battelle
Columbus Laboratories
505 King Avenue
Columbus, Ohio 43201-2693
Telephone (614) 424-6424
Telex 24-5454

January 18, 1985

Mr. Cardis Allen
U.S. Nuclear Regulatory Commission
Mail Stop 5003
Washington, D.C. 20555

Dear Cardis:

I have reviewed the "Preliminary Draft of Steam Explosion Review", dated December 26, 1984, and would like to offer the following observations for your consideration.

1) The statement in the executive summary, ". . . that the combination of a limited melt mass participating in the explosion together with a low thermal to mechanical energy conversion limits the explosive yield to values which are below estimates of values required for containment failure" does indeed represent the view of a number of the members of the review group. It should be further recognized that this view is a postulate rather than an established fact. In most fuel-coolant interraction experiments, the extent of fuel participation in the interaction is not know, and the conversion ratio is inferred assuming interaction of all the available fuel.

2) The statement on page 1, ". . . the interaction must be very rapid (millisecond time scale) . . ." overstates our understanding of steam explosions. The time scale available for the interaction between hot molten fuel and water is still not very well defined. While it is frequently assumed that such interactions must occur over very short time scales, this is not an established fact. A more important time scale in terms of damage potential may be the time scale over which the kinetic energy of the moving fluid (slug) is delivered to the reactor vessel.

3) The concluding paragraph in section 2.1.2 in page 8 presents the limits to mixing and low conversion ratios arguments as established facts rather than postulates.

4) The discussion at the top of page 9 addresses potential differences between PWR and BWR designs and how these differences may affect the likelihood of damaging steam explosions. While I recall the discussion of the in-vessel differences between the two designs, I do not recall any discussion at the Harper's Ferry meeting that compared the likelihood of containment failure, given a large missile, in the two types of designs.

It is in the latter respects that the large, dry PWR and the Mark I BWR designs may have the greatest differences insofar as their response to steam explosions is concerned.

5) Apparently, I did not adequately articulate my position with regard to the SEALS proposal. While I feel that larger scale experiments will probably be needed to resolve outstanding issues, I do not feel that the SEALS proposal as presented should be implemented at this time. I view such a program as being premature; conduct of large scale experiments should be conditioned upon the availability of better analytical models and improved experiment diagnostics.

In addition to the foregoing comments, I would call your attention to my revised responses to Dr. Ross' letter, provided to you on January 18, 1985.

Sincerely,

P. Cybulskis

Peter Cybulskis

PC:dem

cc: T. Ginsberg, BNL

FAUSKE & ASSOCIATES, INC.

January 11, 1985

Mr. Cardis Allen
U.S. Nuclear Regulatory Commission
Mail Stop AR5003
Washington, D.C. 20555

Dear Cardis:

SUBJECT: PRELIMINARY DRAFT OF STEAM EXPLOSION REVIEW

The following changes are suggested in the Executive Summary of the subject report:

Change the sentence starting with "The SERG concludes..., to read "The SERG concludes that the occurrence of a steam explosion of sufficient energetics which could lead to containment failure (α-mode) as having low probability." (The same change is recommended in the first sentence of Section 2.1.1 Approaches.)

On page 2 of the Executive Summary change the sentence starting with "A consensus was reached..., to read "A consensus was reached regarding the desirability for continuing research (using melt masses in the 50-100 kg range) to further confirm the low probability of α-mode failure."

In Section 2.3, Review of SNL Proposed Steam Explosion Research Program, the following changes are recommended:

In the sentence on page 14 starting with "With these perceptions..., add the word confirmatory to describe the research program.

Delete the last paragraph in Section 2.3.1. I don't recall much discussion on this topic.

In summary, I believe the report is well done and adequately reflects the discussions at the Harper's Ferry meeting. Please call if any questions should arise.

Sincerely yours,

Hans

Hans K. Fauske, President
Fauske & Associates, Inc.

HKF:jab

Corporate Office: 16W070 West 83rd Street • Burr Ridge, Illinois 60521 • (312) 323-8750

January 15, 1985

NS-RAT-DS-85-003

Mr. Cardis Allen
U.S. Nuclear Regulatory Commission
4550 Montgomery Avenue
5th Floor, Room 5003 (Air-Rights Bldg)
Bethesda, MD 20814

Dear Mr. Allen:

Enclosed please find my comments on your Preliminary Draft of the steam explosion review as requested in your letter of December 26, 1984.

Please note that the opinions expressed here do not necessarily represent neither EPRI's nor Westinghouse's positions.

Please do not hesitate to call if any of the above points requires further clarification.

Sincerely,

David Squarer

David Squarer

DS/p

cc: W. B. Loewenstein/EPRI
R. C. Vogel/EPRI
M. C. Leverett/EPRI
E. P. Rahe, Jr./W
D. C. Richardson/W

NS-RAT-DS-85-003

TO: C. Allen/T. Ginsberg
FROM: D. Squarer

SUBJECT: COMMENTS ON PRELIMINARY DRAFT OF STEAM EXPLOSION REVIEW

DATE: January 14, 1985

The comments refer to the page number of your preliminary draft accompanied by a cover letter dated December 26, 1984.

Page 9, 3rd - 4th line from top: "It is noted that the respondents felt that BWR failure probability would be less than that for a PWR".

You should note that at least two studies (NUREG CR/2307, Oct. 1981 and Draft NSAC/EPRI, August 1982) have concluded that BWR failure probability is higherthan that of a PWR by an order of magnitude.

Page 9, lines 6-9: You may want to add that the members felt that the failure probability for high pressure sequence would be no higher than for the low pressure sequence, inspite of the anticipated increased mixing at high pressure, due to increased difficulty in triggering at high pressure.

Page 9, line 14: (i) Initial conditions - you should add that the initial conditions assumé core melt which does not necessarily follows each severe accident.

Page 9, line 20: typo - "mal" should read "melt".

Page 9, lines 34-35: "mixing and conversion limitations significantly reduce the likelihood of large-scale steam explosion" - It does not reduce the likelihood, it reduces the yield, so you may want to add: the "likelihood of an energetic or an efficient large scale steam explosion". You may notice that the "general perception" you refer to on line 34 is actually not supported by experimental evidence, and thus warrants the word "likelihood".

Page 9, Lines 31-41: This paragraph leaves the reader with the impression that of the three stages listed (i), (ii), (iii), stage (ii) has a consensus of opinions and understanding, whereas stages (i) and (iii) are less understood. In fact, I believe that stage (iii) is the least difficult to interpret and to analyze since it is subject to an "exact" analyses with analytical methods which are currently available, which is not the case with stages (i) and (ii). I believe that this message should be conveyed in this paragraph.

Page 9, 3rd and 2nd line from bottom: "... the difficulty in intimately mixing large masses of core melt with adequate water" - even if large scale mixing occurs, the conversion ratio is small since much of the melt would contact steam and not liquid, which is a necessary condition for an efficient melt-coolant interaction. You may want to stress this point.

Page 12 - 14 lines from bottom: "The major criticism of the methodology is that the authors did not adequately make use of physical models..." - add "adequately"

Page 12 - 11 lines from bottom: add after engineering judgement "when extrapolating beyond available experimental data", led the authors to exaggerate, ...

Page 15, lines 14-16 - The sentence should read as follows: Squarer would agree with the need for a modified version of the SEALS tests and design concept, if one felt that the probability of the alpha mode of failure is too large to live with (i.e., closer to 10^{-1} than to 10^{-4}) and the uncertainty in determining this probability is excessively large (i. e., span evenly the above range).

Page 15 - 9 lines from the bottom: add after the last line of FITSX: (iv) that experiments be performed with prototypic melt rather than simulant melts (i.e., corium instead of iron-alumina).

Page 15 - 4th line to 2nd line from bottom - I believe the sentence should read as follows: This geometry is possible in the postulated accident situation and may be helpful in the measurement and fundamental understanding of the proposed "pre-mixing length scale" and the "coupling length scale". Incidently, I don't believe that there was a general support from the committee members for these two length scales.

Page 17 - Table 1 - The title of this Table is misleading. Many of the entries in this Table are <u>assumptions</u> rather than "mechanistic calculations". For example, Bankoff has <u>assumed</u> 24,000 kg/sec rather than calculated; all entries under pour diameter are pure assumptions; the 'mixing depth' should be replaced by 'characteristic length' since in some cases it represents drop size; the entries under specific energy of the melt are certainly an assumption, and so are entries under conversion ratio. Fauske, Mayinger, Theofanous and Corradini did publish <u>estimates</u> of melt mass participating but in fact only Fauske and Corradini presented mechanistic models for coarse pre-mixing. In addition, the presentation of the 'best estimate containment failure probability' in Table 1 implies direct relationship between the 'mechanistic calculations' and the probability, which is not at all obvious, since if in all cases the explosion energy is lower than ~2GJ why the probability is not ~0 as Fauske noted. Thus, if there is a need at all for Table 1, it has to be rewritten and stated as to what is assumed and what is mechanistically calculated and why the probability is greater than ~0.

Page 19 - Table 3 - The line opposite Squarer should be modified as follows: under 'initial conditions' change 10^{-3} to 10^{-2}. The reason is that in our assessment of the 'initial conditions' we assumed an additional 10^{-1} as the probability of core melt given a severe accident. However, this stage is not addressed in this document since a core melt is already assumed.

Under 'mixing and conversion', on the line opposite Squarer, add 10^{-1}. This is justified as documented in my response to Dr. Ross's first question (p. 3 of my November 5, 1984 letter) and because some of the mixed melt would contact steam rather than liquid thus producing limited mass, inefficient interaction (as shown by Bankoff et al). This would result in a total failure probability given a core melt of 10^{-4} to 10^{-5} rather than 10^{-4}.

Page 20 - Table 4 - I suggest that the heading of 'initial conditions' should read 'initial conditions given a core melt'. The line opposite Squarer should read 10^{-1} instead of 10^{-2} under 'established pool conditions' and 'coherent pour conditions' for the same reasons given for Table 3 above. The 10^{-1} under 'water available' needs an asterisk since it is for 'subcooled water available'. As explained above under 'mixing and conversion' for Table 3 the value of 1.0 should be replaced by 0.1 due to melt/steam interaction, difficulty of triggering with saturated water and low conversion ratio due to steam/melt interaction. The corresponding 'containment failure probability' should be 10^{-4} to 10^{-5}.

Appendix D

Page 1 - 15 lines from bottom: "the pour stream ... may be aided in breakup by the lower internal structure". It should be added that the lower structure may also prevent large scale melt mixing (due to freezing and clutter - i.e., flow resistance).

Page 1 - ' fuel-coolant mixing' - (a) a reference should be made here to the 'coarse pre-mixing' which was used extensively in the literature, since 'pre-mixing length scale' is another name for 'coarse pre-mixing', (b) a reference should be made here to fine fragmentation, since the 'coupling length scale' is another name for the fine fragmentation which is used extensively in the literature.

Single and multiple explosion - page 2 lines 15-16 - (a) flooding and/or fluidization limits on fuel - coolant coarse pre-mixing and the subsequent explosion seemed to be valid concepts. I suggest adding 'coarse pre-mixing'.

PURDUE
UNIVERSITY SCHOOL OF NUCLEAR ENGINEERING

January 14, 1984

Cardis Allen
Nuclear Regulatory Commission
Mail Stop AR 5003
Washington, DC 20555

Dear Cardis:

Here are my comments and suggestions on the Droff report of The Steam Explosion Review Panel. p. 1. 'Unlikely' means nothing. I would prefer the wording 'physically unreasonable'.

p. 1. Eliminate second sentence of second paragraph. The Sandia report did not present any new methodology to warrant such a statement. Furthermore there are a lot of problems with their methodology.

p. 2. Replace: 'to reduce ... probabilities.' by 'to understand better certain aspects of steam explosion phenomenology'.

p. 5. Replace 'differences in' on line 6 by 'key points of'.

p. 7. Delete first two paragraphs. Too repetitive and too detailed.

p. 7. First line under 2.1.1. The groups response was much-much stronger than 'unlikely'. Unlikely means nothing.

p. 7. Change 1.2 to 2.1.2.

p. 8. Change 1.3 and 1.4 to 2.1.3 and 2.1.4.

p. 14. Third paragraph. This implies that uncertainties are too high still, which is in contradiction to results from question 1. Any statements on future work must be clear, carefully worded and put into the proper perspective. Also, recommendation should be made that any future small scale tests must be throughly defined and reviewed in the manner done for large scale tests in this report. Note that the SLN proposal reviewed covered only large scale tests and the recommendations made for small scale tests were made ad hoc without the necessary careful consideration - This should be made clear under items (i), (ii), (iii) and (iv) of p. 14.

Purdue University
West Lafayette, Indiana 47907

p. 15 Add an item (v) to convey that non of the arguments presented against current mechanistic estimates and low probabilities (i.e. multiple explosion etc), are being addressed by SEALS.

p. 15. On the FITSX tests. This gives the impression that the panel blessed these tests in their detail. This is most certainly not the case. See also previous comment above. Recommend remove statement on agreement for general content of test matrix.

p. 15. The ACM tests. This should be coupled with the FITSX section with all qualifications mentioned above. Again no discussion or agreement on test matrix were made.

p. 17. My probability number recommended was 10^{-4}. Please make correction.

p. 18. Under my name both columns should show $< 10^{-4}$.

p. 19. Both columns under my name show $< 10^{-4}$.

p. 20. Both columns under my name show $< 10^{-4}$. Put also strong emphasis in 'Large Scale Intimate Mixing Column' for me.

In conclusion I like to comment Mike Corradini for putting together a very nice status summary in Appendix D.

Sincerely,

T.G. Theofanous
Professor

TGT/pf

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U.S. NUCLEAR REGULATORY COMMISSION

BIBLIOGRAPHIC DATA SHEET

E INSTRUCTIONS ON THE REVERSE

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NUREG-1116

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A Review of the Current Understanding of the Potential for Containment Failure From In-Vessel Steam Explosions

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4 DATE REPORT COMPLETED

MONTH	YEAR
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AUTHOR(S)

Steam Explosion Review Group (SERG)

6 DATE REPORT ISSUED

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SUPPLEMENTARY NOTES

ABSTRACT *(200 words or less)*

A group of experts was convened to review the current understanding of the potential for containment failure from in-vessel steam explosions during core meltdown accidents in LWRs. The Steam Explosion Review Group (SERG) was requested to provide assessments of: (i) the conditional probability of containment failure due to a steam explosion, (ii) a Sandia National Laboratory (SNL) report entitled "An Uncertainty Study of PWR Steam Explosions," NUREG/CR-3369, (iii) a SNL proposed steam explosion research program. This report summarizes the results of the deliberations of the review group. It also presents the detailed response of each individual member to each of the issues. The consensus of the SERG is that the occurrence of a steam explosion of sufficient energetics which could lead to alpha-mode containment failure has a low probability. The SERG members disagreed with the methodology used in NUREG/CR-3369 for the purpose of establishing the uncertainty in the probability of containment failure by a steam explosion. A consensus was reached among SERG members on the need for a continuing steam explosion research program which would improve our understanding of certain aspects of steam explosion phenomenology.

DOCUMENT ANALYSIS – a KEYWORDS/DESCRIPTORS

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