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NSD-NRC-97-5184
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June 13, 1997

Document Control Desk
U.S. Nuclear Regulatory Commission
Washington, DC 20555

ATTENTION: T. R. QUAY

SUBJECT: RESPONSE TO NRC COMMENTS AND PEER REVIEW COMMENTS ON THE LOWER HEAD INTEGRITY UNDER IN-VESSEL STEAM EXPLOSION LOADS REPORT

Dear Mr. Quay:

A meeting was held between Westinghouse, DOE, and NRC staff on April 15, 1997 to discuss NRC questions on the DOE-supported Advanced Reactor Severe Accident Program report entitled "Lower Head Integrity Under In-Vessel Steam Explosion Loads" (DOE/ID-10541) and its companion reports "Escalation and Propagation of Steam Explosions: ESPROSE.m Verification Studies" (DOE/ID-10503) and "Premixing of Steam Explosions: PM-ALPHA Verification Studies" (DOE/ID-10504). An NRC letter dated May 9, 1997 provides a summary of the meeting and the nine questions/actions Westinghouse and NRC agreed to at the meeting.

Enclosed with this letter are:

- Responses to the nine NRC comments on the subject reports
- Addendums for Chapters 3 through 6 and Appendix B of the DOE/ID-10541 report
- An addendum for Appendix C of the DOE/ID-10503 report.
- Appendix F, Experts' Comments and Authors' Responses.

The addendums to the reports were made to support responses to the NRC and expert peer reviewers' comments.

This material is being provided for NRC review.

The nine questions of the NRC are listed in OITS as numbers 5508 through 5516. The enclosed material closes, from the Westinghouse perspective, the NRC comments. The Westinghouse status column will be changed in the OITS to "Closed." The NRC should review the enclosed material and inform Westinghouse of the status to be designated in the "NRC Status" column of the OITS. Note that the NRC status within OITS for #5514 (corresponding to question 7) is listed as "Resolved" per the NRC's May 9 letter.

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Please contact Cynthia L. Haag on (412) 374-4277 if you have any questions concerning this transmittal.

D. O. Lindgren /FOR
Brian A. McIntyre, Manager
Advanced Plant Safety and Licensing

jml

Enclosure

cc: J. Sebrosky, NRC (Enclosure)
N. J. Liparulo, Westinghouse (w/o Enclosure)

Enclosure to Westinghouse
Letter DCP/NRC0915

June 13, 1997

RESPONSES TO THE NRC QUESTIONS

1. The growth constants λ used for metallic and oxidic melts are shown in Figures 1 and 2, respectively. Freezing in the Zr plug region would be faster still.

2. The partition of thermal power into the up, horizontal, and downward directions depends only on the natural convection processes in the pool. This gives the 0.02 MW/m^2 value imposed on the top of the blockage under discussion. In addition to this power, the blockage will deliver the power generated within it, as shown in Eq. (4.3) of the report. The blockage effective thermal conductivity, on the other hand, determines the thickness of the blockage. As can be seen from Eq. (4.4), the larger the thermal conductivity (k), the larger the blockage thickness (L). While the metallic blockage has a larger thermal conductivity (compared to the oxidic one), it carries much lower decay power, so that the difference between the two cases is to a great extent compensated. Another important factor is blockage porosity. It reduces both effective power density in the blockage and thermal conductivity. These reduce blockage thickness and downward heat flux. To elucidate these trends, a series of calculations were carried out and the results are summarized in the addendum to Chapter 4.

3. See item 4 below.

4. The points in Figure 3.9 are **calculated** results (i.e., "data") using actual material properties. The value of β is 0.05, and it is **not** assumed, but rather chosen, so that the line **fits** the data. The **assumption** is the form of Eq. (3.8), and the collapse of all data on a single line, as in Figure 3.8, shows that this assumption is correct.

   The use of the two extreme points in Figure 3.8 to "deduce" pouring rates that will fail the vessel is not appropriate. There is no basis to assume that the "impulse is proportional to the release rate," and indeed our results show that in the range examined, 200 to 400 kg/s, there is no discernible dependence (on this see also new, more detailed results and interpretations, in the addendum to Chapter 6).

   Finally, as discussed at the meeting, the geometry in AP600 is so different from that in TMI-2 as to forbid any attempt at relating respective failure areas. In TMI, the core baffle plate was less than an inch thick, and it was subjected to a long duration impingement from the melt (~90 s) displaced from the pool region, probably by some falling-in of solids from above.

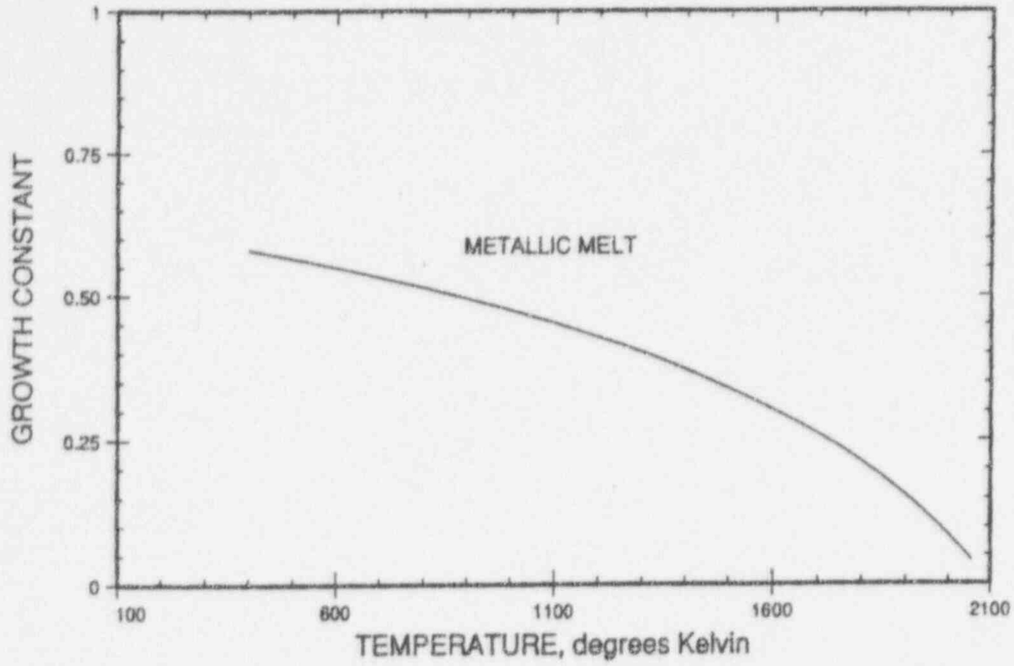


Figure 1. Dependency of the solidification growth constant upon the underlying frozen substrate temperature for the case of metallic melt having negligible molten superheat.

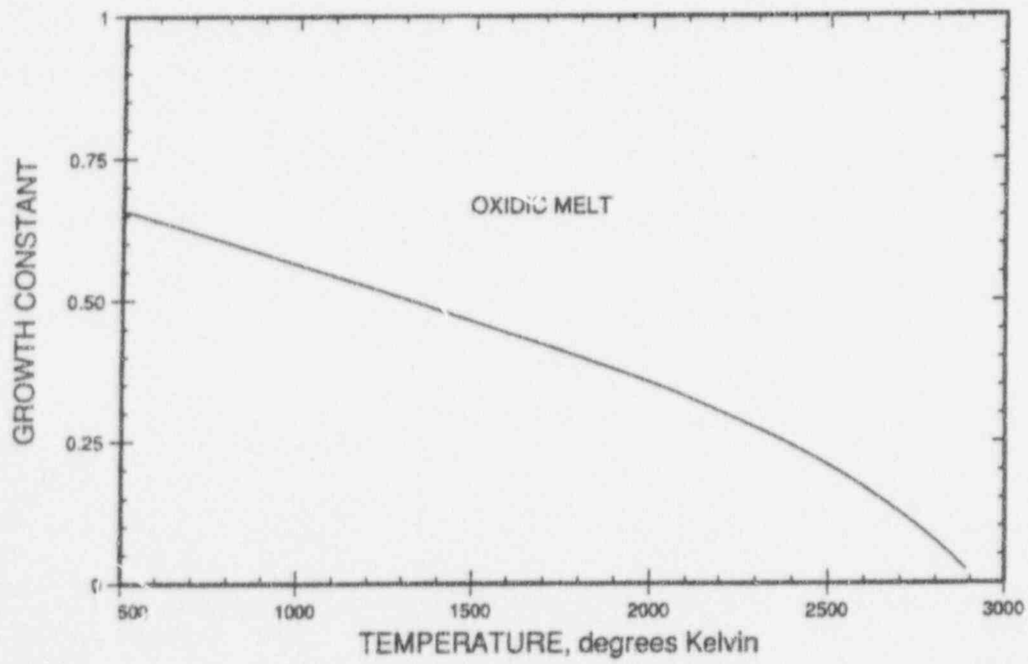


Figure 2. Dependency of the solidification growth constant upon the underlying frozen substrate temperature for the case of oxidic melt having negligible molten superheat.

5. The no breakup cases considered show clearly that the strength of an explosion is moderated by the limited interfacial area for microinteractions. So, using a larger initial size would be even more limiting. Of course, we could break this up too, but then we would end up in the same kinds of premixtures as in the 2 cm cases considered.
6. See addendum to Chapter 6.
7. Resolved.
8. Addition will be made in the final report, as suggested.
9. The failure mode in the reflood case is by uniform elongation (i.e., global), as compared to the very localized bending modes found in the premixed explosions. The latter were taken to fail conservatively, when ~50% of the cross section reached 11% plastic strain. We believe a 20% value is more appropriate for a global failure mode, as the one of interest here. Note, however, that an 11% value would produce the same conclusion; namely, that reduction of strain by a factor of 2 would reduce the impulse and hence the pressure by a factor of 1.4 —see Eq. (8.1) — so that instead of 3500 bar we would now need, for failure, 2500 bar, i.e., equally unlikely.

[[ADDENDUM TO CHAPTER 3]]

The purpose of this addendum is to address the following three points raised in the review:

1. Effect of actual yield strength on the fragility curve;
2. Behavior for very small loaded areas; and
3. Applicability of rate dependent yield strength of mild steel to A508B.

1. The quoted material strength for steel A508B is 330 MPa, while the reported value for the steel from Japan Steel Works is 450 MPa. In all calculations we have used the former, to be conservative. When we use the latter, however, we obtain a significant increase in safety margins, as shown in Figure 3.a1.

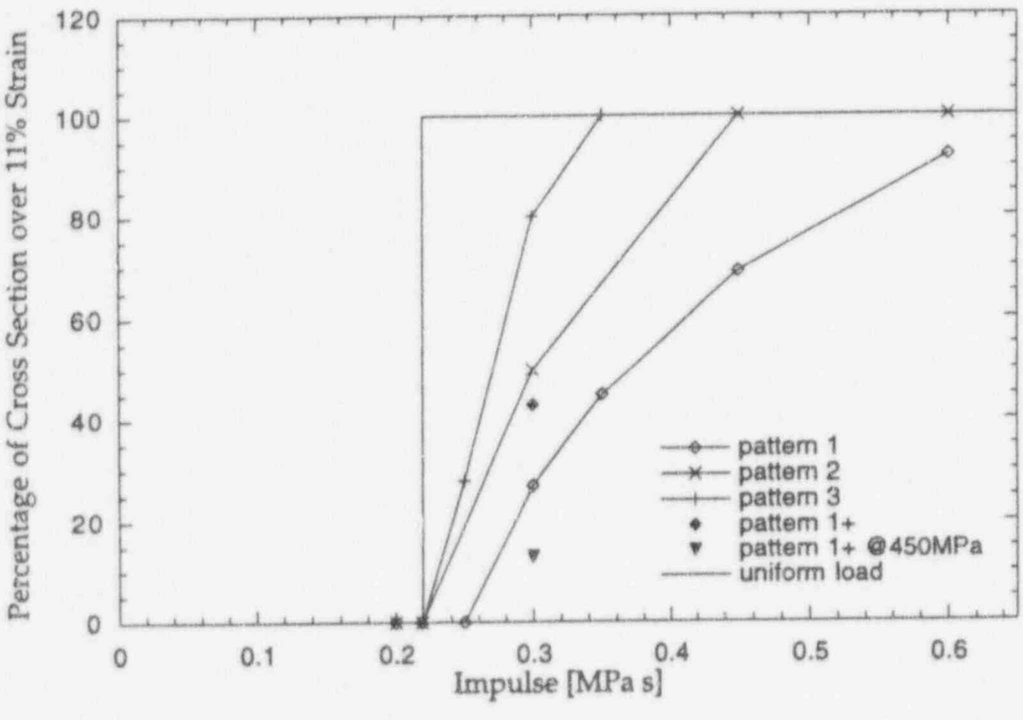


Figure 3.a1. The effect of increasing the yield stress from 330 to 450 MPa on the fragility of the lower head.

2. The smallest loading pattern utilized in Chapter 3 had an inner diameter of 0.94 m and an outer diameter of 1.4 m. We have made test runs with $d_i = 0.39$ and $d_o = 0.87$ m (pattern 0), and with $d_i = 0.55$ m and $d_o = 0.94$ m (pattern 0+). The corresponding equivalent plastic strains are shown plotted in Figures 3.a2 and 3.a3, with respect to the impulse and the effective impulse. From these figures we see that the equation for the effective impulse is still rather good but somewhat conservative.

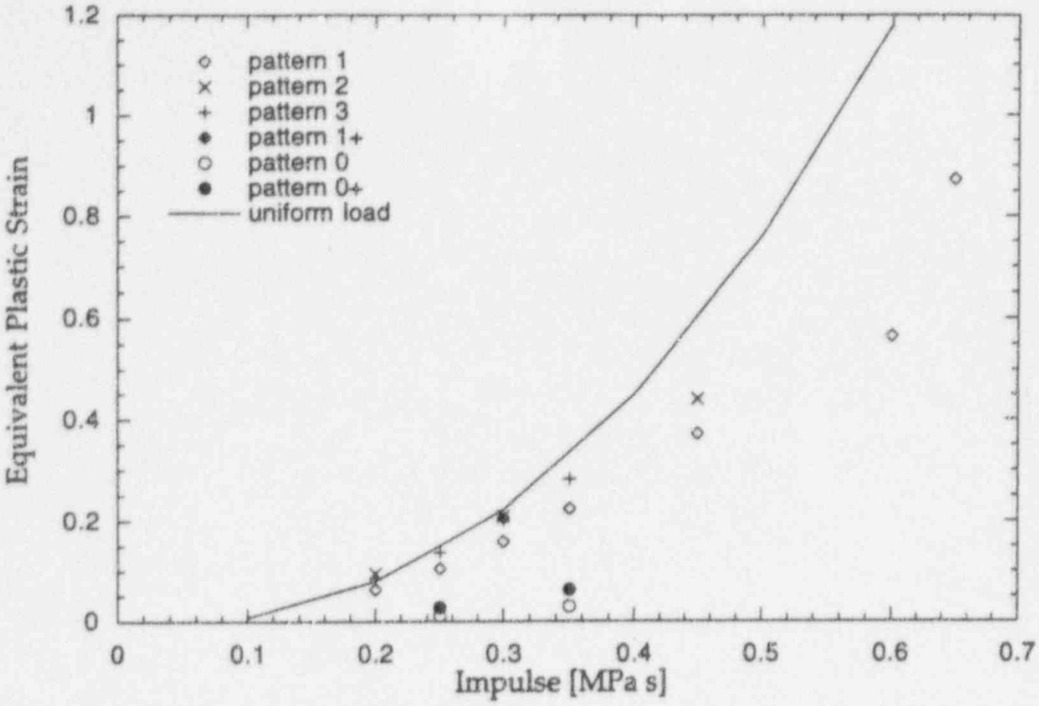


Figure 3.a2. The new results obtained with loading patterns 0 and 0+.

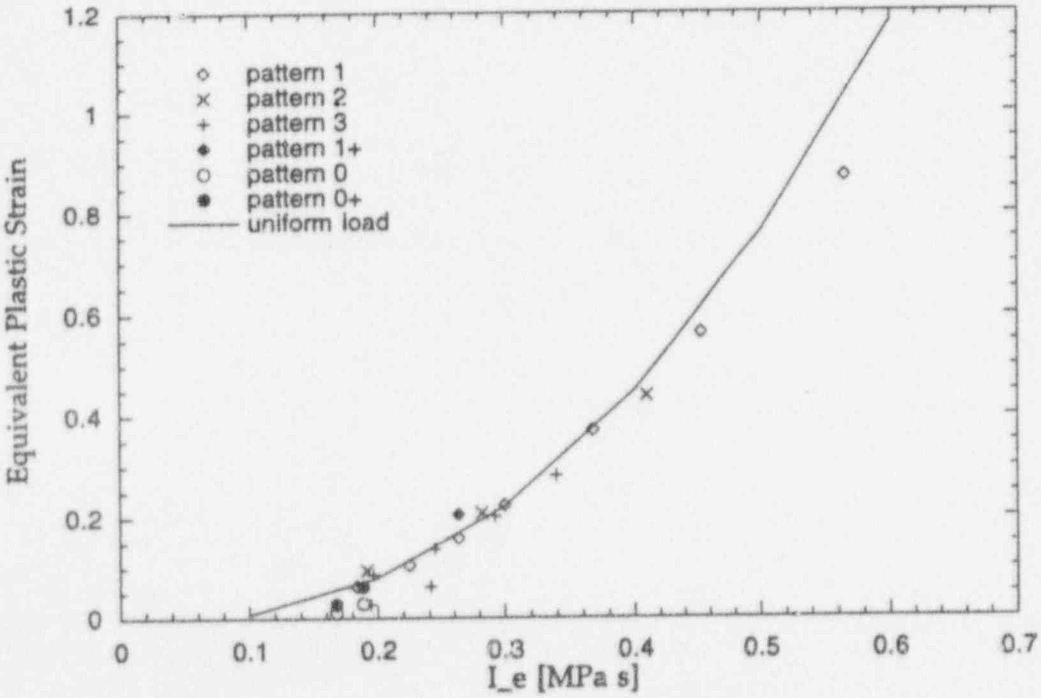


Figure 3.a3. Depiction of the new results obtained with loading patterns 0 and 0+ in the effective impulse formulation.

3. The calculation performed in Chapter 3 utilized the strain rate dependency of yield stress as found experimentally for mild steel, since A508B has a carbon content that places it into that category and experimental data on A508B at high strain rates do not exist. Yet a similar steel, A533B, with somewhat higher carbon content, has been found tested at high strain rates and shows much less strain rate dependence. As explained in the report, to ensure against this eventuality, we took a lower yield stress, of 330 MPa, that just about compensates for the variation due to the strain rate dependency. We now have two additional calculations, carried out for reactor case C2-20, as summarized in Table 3.a1. The first entry in this table is the old result. We can see that strain rate dependency is not crucial to the conclusion; however, in the absence of directly applicable data, it may not be appropriate to concede this additional safety factor.

Table 3.a1 The Interplay Between Yield Stress and Strain Rate Effects

Yield Stress (MPa)	Strain Rate Dependency	Maximum Equivalent Plastic Strain (%)
330	Yes	0.34
330	No	0.47
450	No	0.23

[[ADDENDUM TO CHAPTER 4]]

A number of questions were raised about the integrity of the lower blockages, and the purpose of this addendum is to provide material relevant to these questions. Specifically we consider in more detail the coolability and resulting radiative heat fluxes downward to the water. Also, we quantify the additional heat sink, due to the core support plate, after the water is vaporized to a level below it.

1. Calculations were carried out with oxidic blockages of various porosities in both the bottom of the active fuel region, as well the Zr plug region. Also, calculations were carried out with metallic blockages, containing 14 w/o fuel, in the bottom of the active fuel region. Effective volumetric powers and thermal conductivities were computed consistently, as described in the report. Relocated fuel in the blockage was taken at the average core power and released volatile fission products as in DOE/ID-10460. Fuel stubs were taken at the local power peaking and with all volatiles in them. The calculated values are shown together with the results in Tables 4.a1 through 4.a3. We find, in all cases, robust blockages. Also, we find that downward heat fluxes are in the range 0.1 to 0.2 MW/m^2 as estimated previously.

Table 4.a1 Oxidic Blockage at Bottom of Zr Plug Region

Blockage Porosity	Blockage Power per Volume MW/m^3	Blockage Effective Thermal Conductivity W/(m·K)	Downward Radiative Heat Flux MW/m^2	Blockage Thickness cm
0.1	1.337	19.9	0.186	12.5
0.1	1.273	19.8	0.183	12.8
0.5	0.743	18.5	0.144	16.7
0.5	0.709	18.5	0.141	17.1
0.8	0.297	17.0	0.096	25.5
0.8	0.283	17.0	0.094	26.9

Table 4.a2 Oxidic Blockage at Bottom of Active Fuel Region				
Blockage Porosity	Blockage Power per Volume MW/m^3	Blockage Effective Thermal Conductivity $W/(m \cdot K)$	Downward Radiative Heat Flux MW/m^2	Blockage Thickness cm
0.1	1.786	6.28	0.213	5.48
0.1	1.694	6.29	0.209	5.63
0.5	1.192	5.61	0.175	6.45
0.5	1.128	5.60	0.171	6.63
0.8	0.746	5.07	0.141	7.75
0.8	0.705	5.60	0.137	7.95

Table 4.a3 Metallic Blockage at Bottom of Active Fuel Region				
Blockage Porosity	Blockage Power per Volume MW/m^3	Blockage Effective Thermal Conductivity $W/(m \cdot K)$	Downward Radiative Heat Flux MW/m^2	Blockage Thickness cm
0.3	0.596	19.5	0.216	16.7
0.3	0.561	19.4	0.210	17.2
0.4	0.575	17.3	0.203	16.1
0.4	0.541	17.2	0.198	16.6

2. The temperatures of the core support plate subjected to a heat flux of 0.20 MW/m^2 at its upper surface (and an adiabatic boundary condition at the lower surface) are shown in Figure 4.a1. It is clear that this heat sink provides significant further margin to the "race" between the 100 minutes estimated to vaporize the water and the ~91 minutes to fail the core barrel. Also, it should be noted that the boiled water was taken to escape the vessel, while in reality some significant fraction of it would be expected to reflux back.

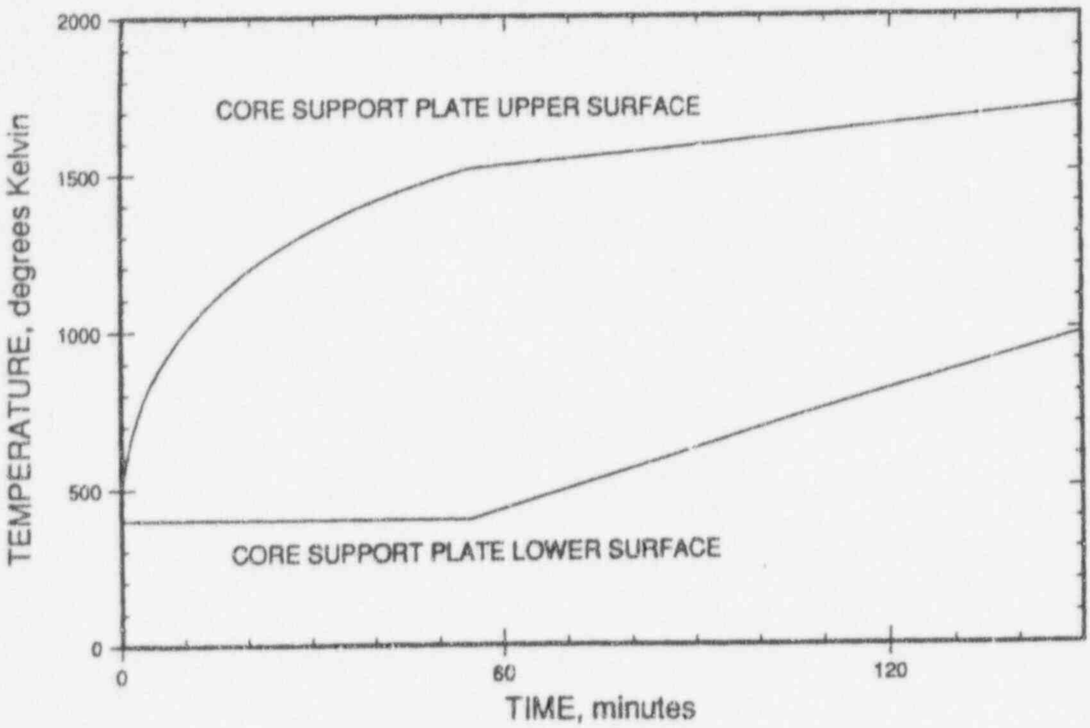


Figure 4.a1. Heatup of the core supported plate under a heat flux of 0.2 MW/m^2 applied to the top surface. The emissivity of the plate was taken as 0.4 and the holes were ignored (conservative by ~25%).

[[ADDENDUM TO CHAPTER 5]]

The purpose of this addendum is to:

1. Extend the calculations presented earlier to longer premixing times;
2. Present new results at higher pressure (3 bar) and in the presence of subcooling (10 °C);
3. Present new results with a finer grid (about half the previous size); and
4. Present a new, more clear representation of such results, so one can visually appreciate the explosive "quality" of the premixtures.

Starting with the last item, the idea is to provide snapshots in the $\theta_f - \alpha$ plane, showing the composition of each computational cell by a point, the color of which is keyed to a colored length scale. Looking at a sequence of such snapshots one can visualize the extent of spatial volumes involving fuel and water together with the corresponding fuel length scales.

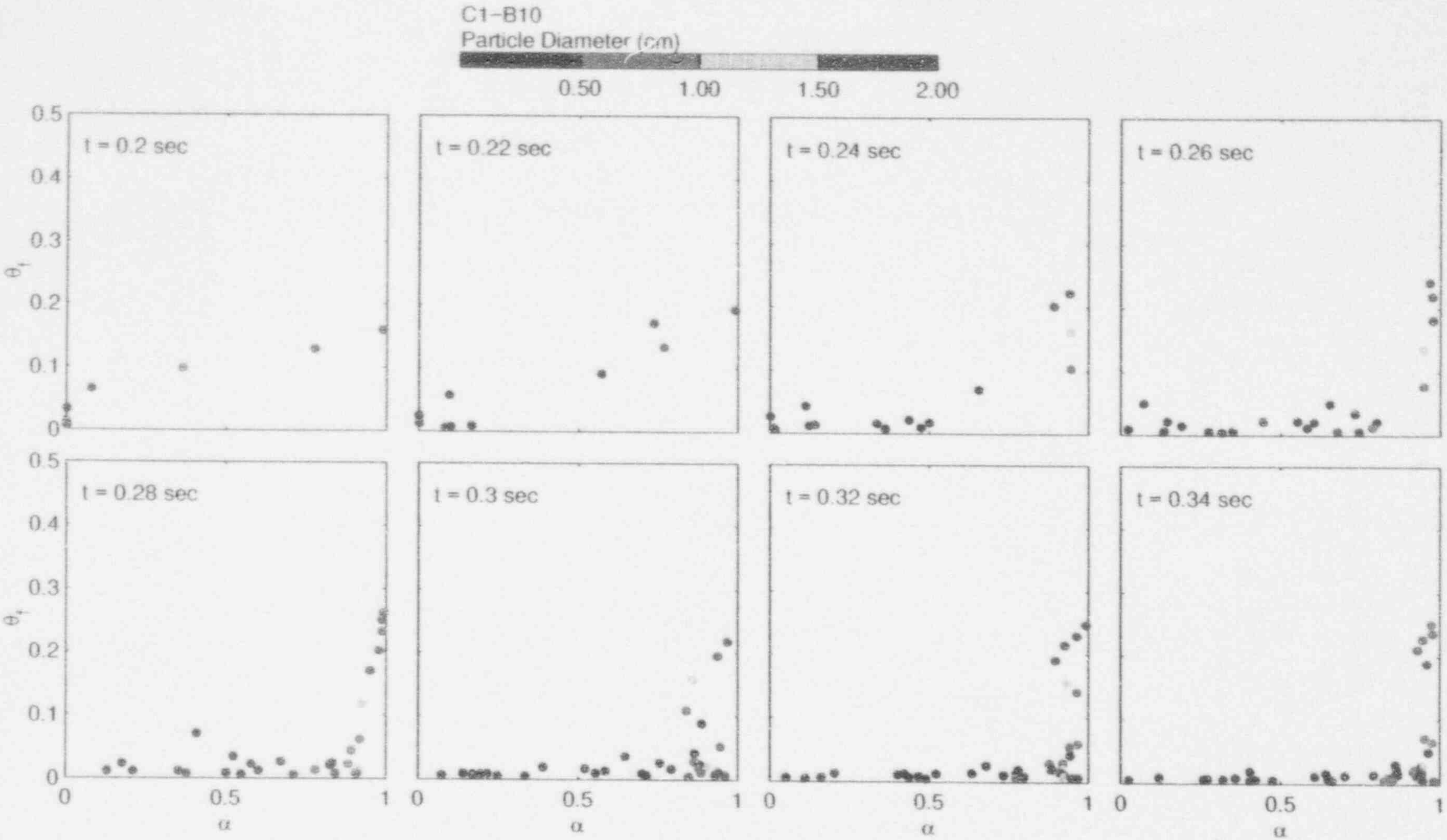
The following figures are such plots, each marked by the run identification number and the time. The first 13 figures show in expanded form those portions of each run for which the premixture is most energetic (assuming it can be triggered). The relation of the so-deduced energetic quality of the premixtures with quantitative results obtained through ESPROSE.m can be confirmed by comparison to the results shown in the addendum to Chapter 6. The rest of the figures in the present addendum show, in a smaller format, the complete premixing histories for the PM-ALPHA runs made.

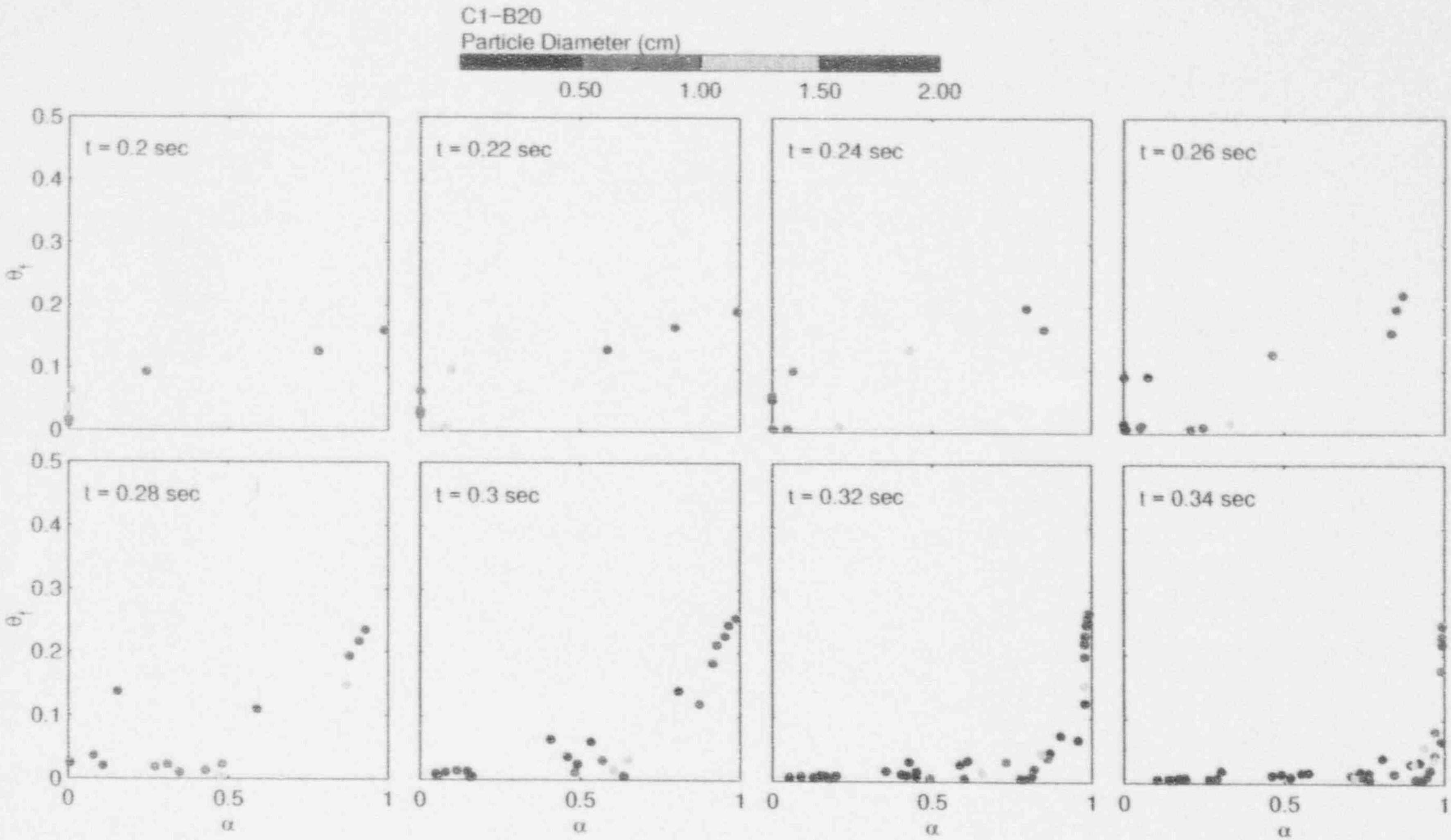
The runs made are summarized in Table 5.a1 below, where the nomenclature utilized is the same as in the report. The modifiers "3 bar" and "10 c" are for runs made at 3 bar ambient pressure and 10 °C subcooling, respectively. All runs were started from the beginning of the pour, with an appropriate inlet for the melt at the top of the computational domain. In the overlap region comparison with the old results was good. To save computation time the two RC1 runs were made on a Cartesian (2D) grid, which is a good approximation given the width of the pour relative to the radius of curvature.

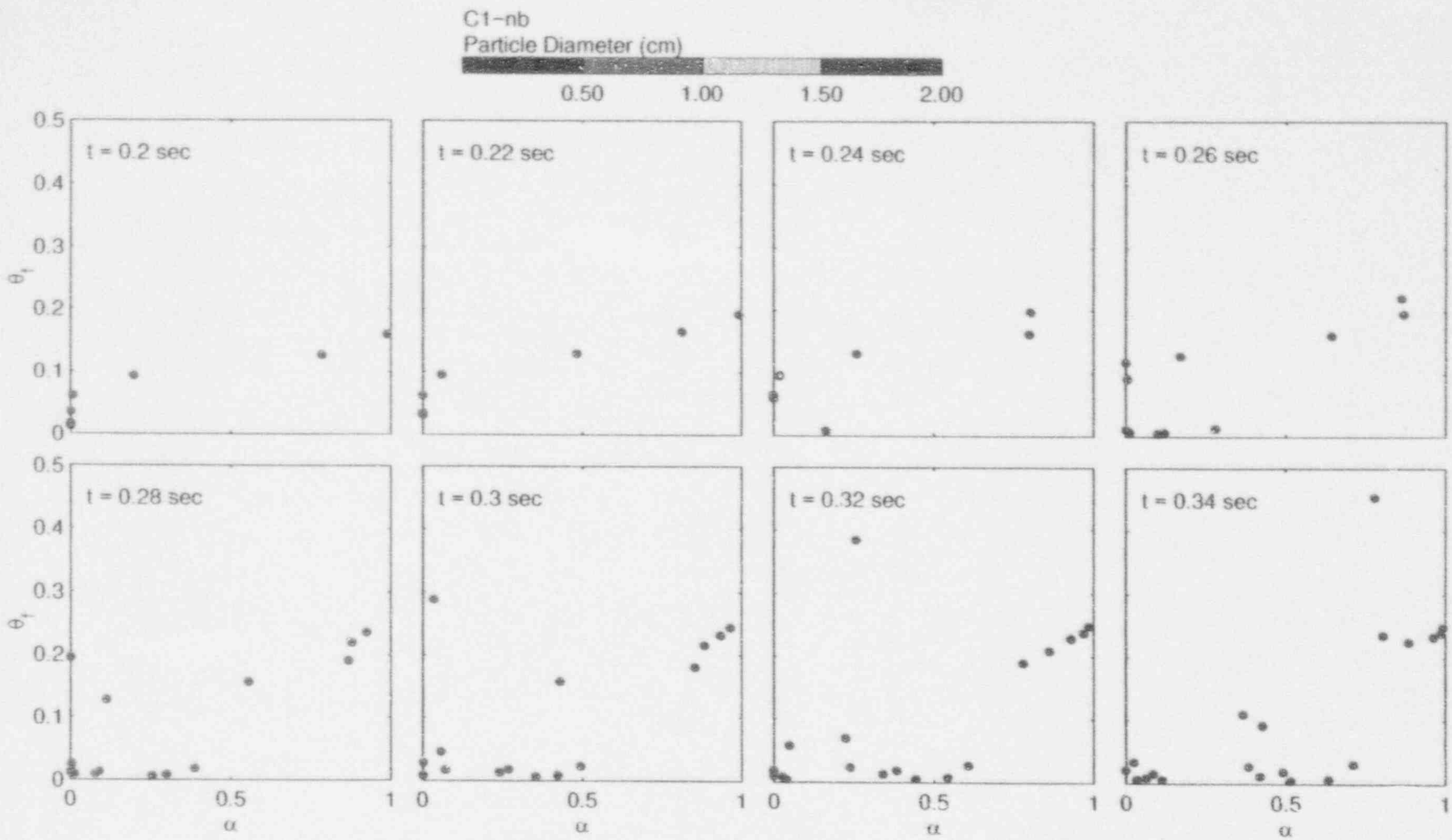
Especially interesting to see, in the long records, is how the $\theta_f - \alpha$ maps converge to a common "shape," with most points concentrated around the $\theta_f \sim 0$ and $\alpha \sim 90\%$ axes. Also, it is clear that the "sensitive" premixtures are small in size and of a short time duration. These trends confirm our previous conclusions, but are more clearly and comprehensively illustrated by the present method. Also, we can see that the 3 bar and 10c runs show nothing unusual compared to the other ones.

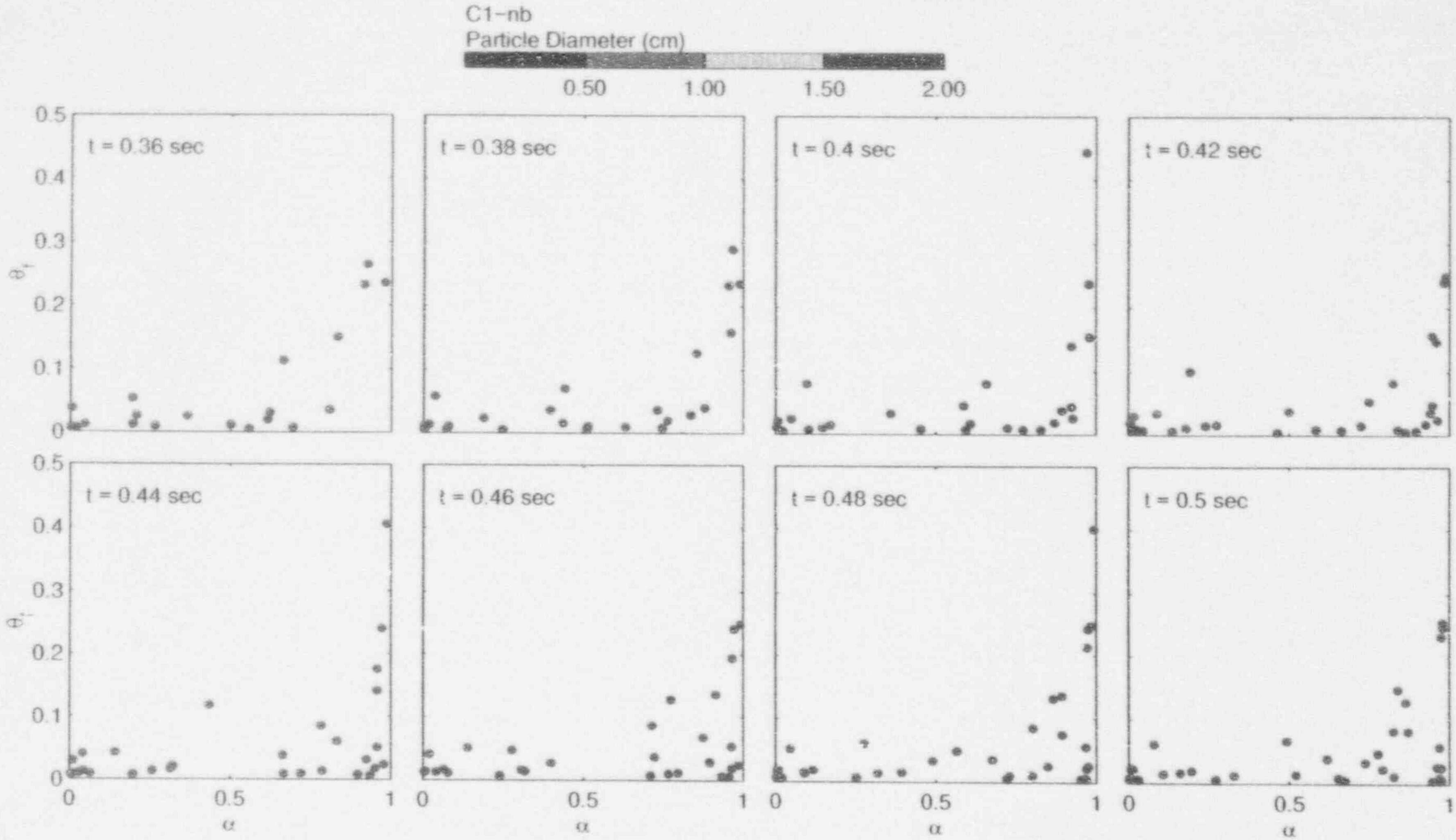
Table 5.a1 Summary of PM-ALPHA.3D Premixing Runs.

β	C1	RC1	C2
10	C1–10		C2–10
20	C1–20 C1–20–3 bar C1–20–10c	C1–20–2.5 cm	C2–20
nb	C1–nb	C1–nb–2 cm	C2–nb

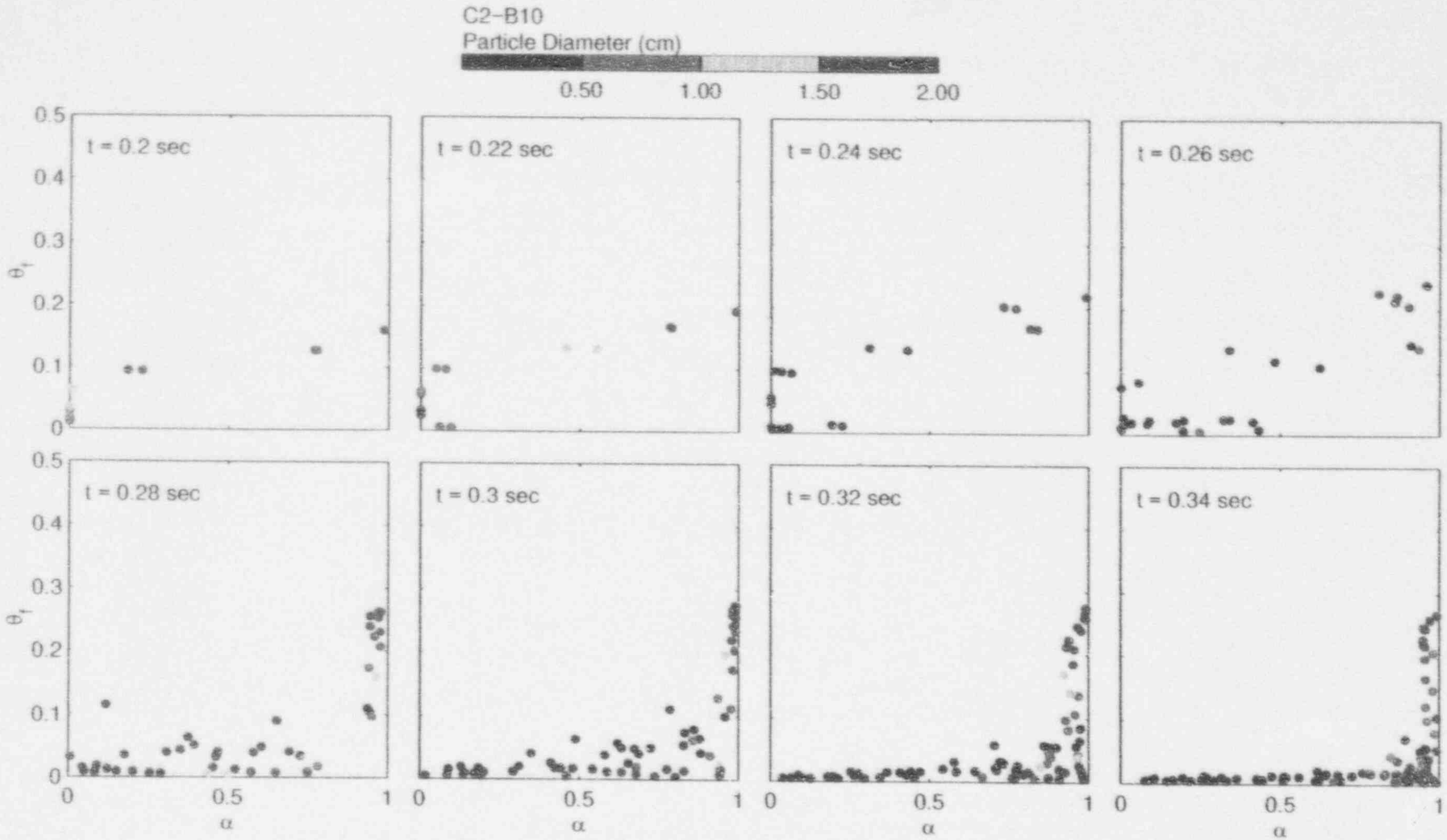

C1-B10
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 0.2 sec
t = 0.22 sec
t = 0.24 sec
t = 0.26 sec
t = 0.28 sec
t = 0.3 sec
t = 0.32 sec
t = 0.34 sec
θ
α
0
0.1
0.2
0.3
0.4
0.5
0.5
1


C1-B20
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 0.2 sec
t = 0.22 sec
t = 0.24 sec
t = 0.26 sec
t = 0.28 sec
t = 0.3 sec
t = 0.32 sec
t = 0.34 sec
θ
α
0
0.1
0.2
0.3
0.4
0.5
0
0.5
1

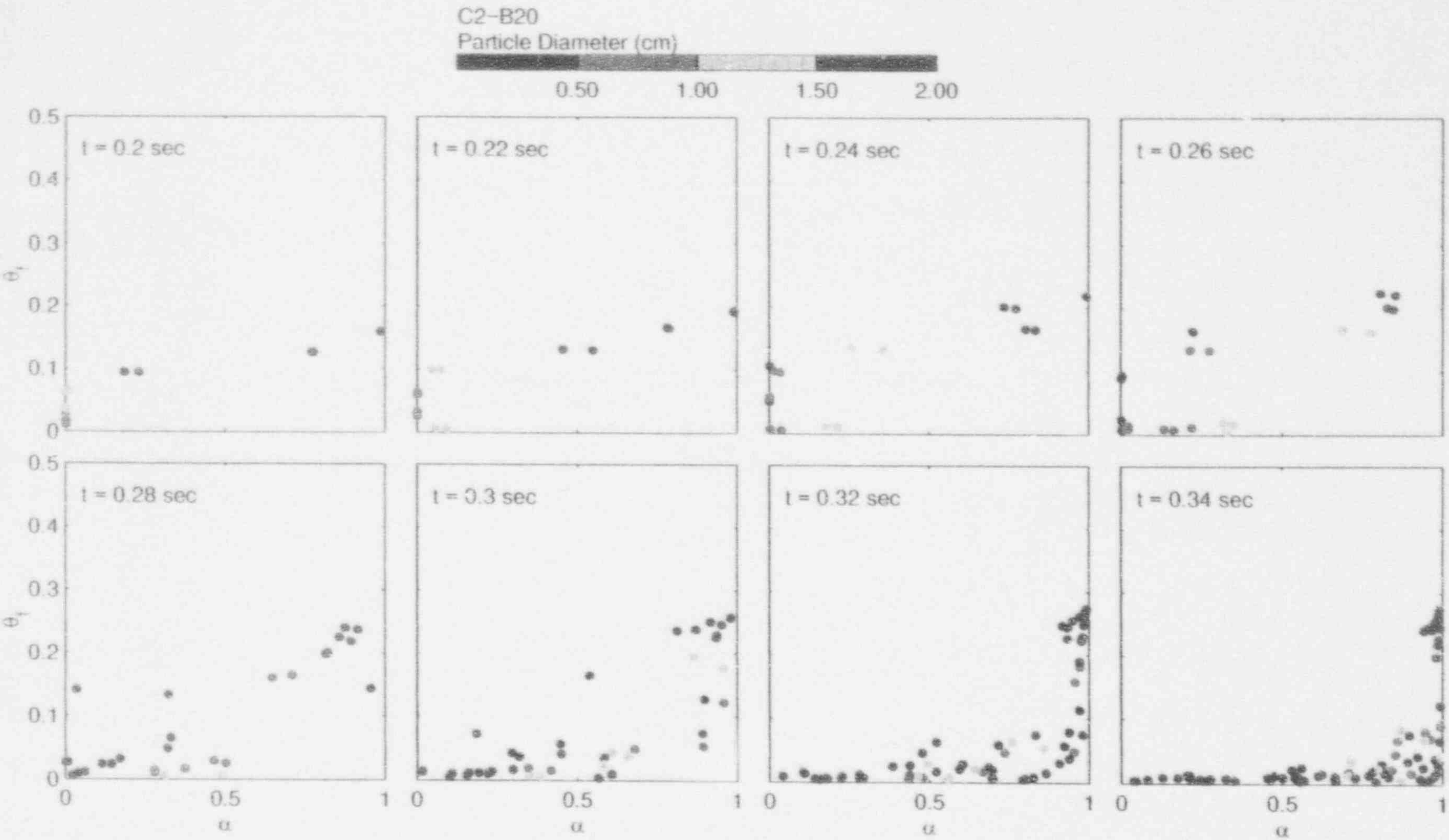

C1-nb
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 0.2 sec
t = 0.22 sec
t = 0.24 sec
t = 0.26 sec
t = 0.28 sec
t = 0.3 sec
t = 0.32 sec
t = 0.34 sec
α
0
0.5
1
0.1
0.2
0.3
0.4



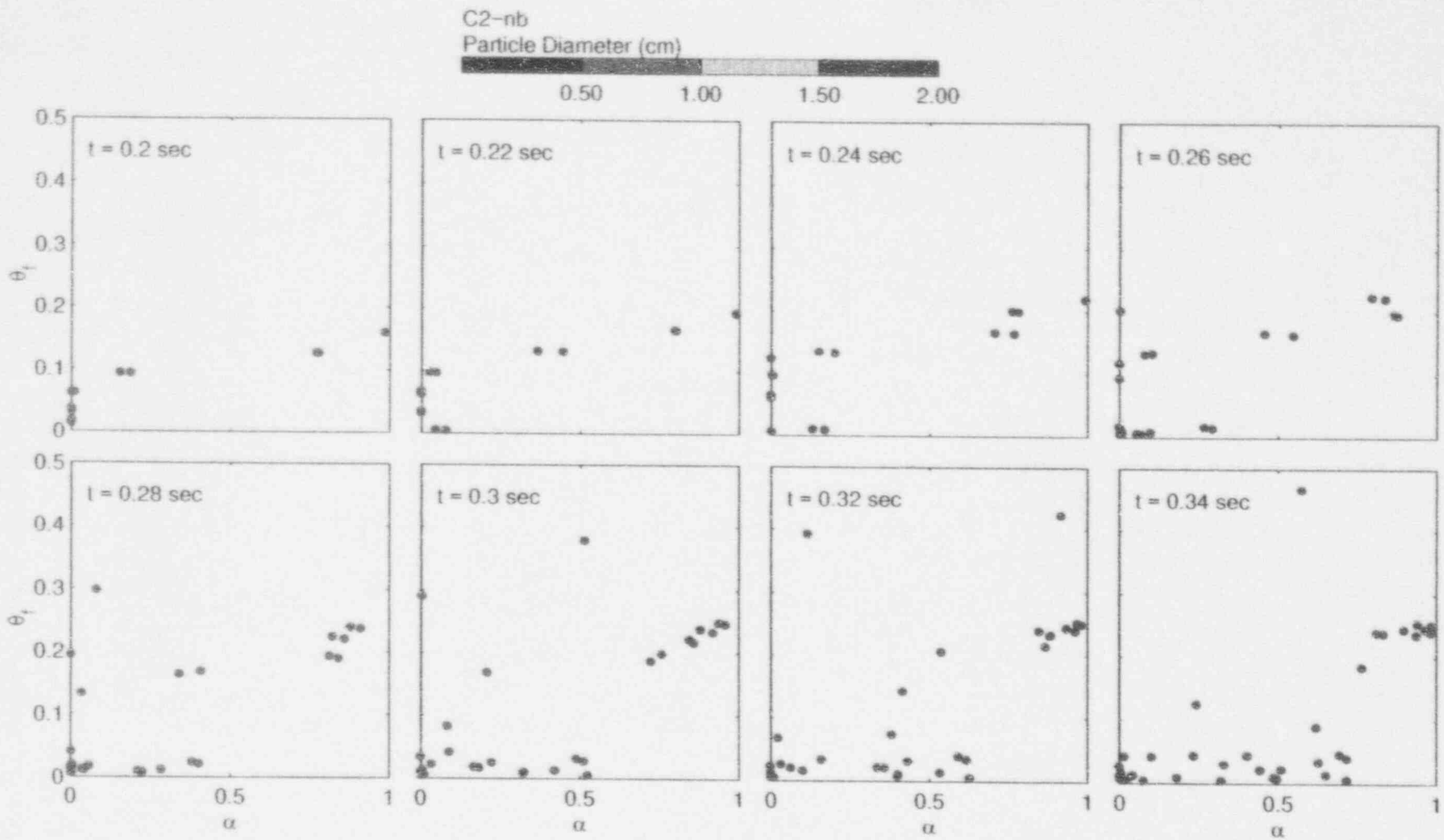
C1-nb
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 0.36 sec
t = 0.38 sec
t = 0.4 sec
t = 0.42 sec
t = 0.44 sec
t = 0.46 sec
t = 0.48 sec
t = 0.5 sec
0.5
0.4
0.3
0.2
0.1
0
0.5
1
α



C2-B10
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 0.2 sec
t = 0.22 sec
t = 0.24 sec
t = 0.26 sec
t = 0.28 sec
t = 0.3 sec
t = 0.32 sec
t = 0.34 sec
θ_t
α
0
0.1
0.2
0.3
0.4
0.5
0.5
1



C2-B20
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 0.2 sec
t = 0.22 sec
t = 0.24 sec
t = 0.26 sec
t = 0.28 sec
t = 0.3 sec
t = 0.32 sec
t = 0.34 sec
θ
α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Particle Diameter (cm)
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1.00
1.50
2.00
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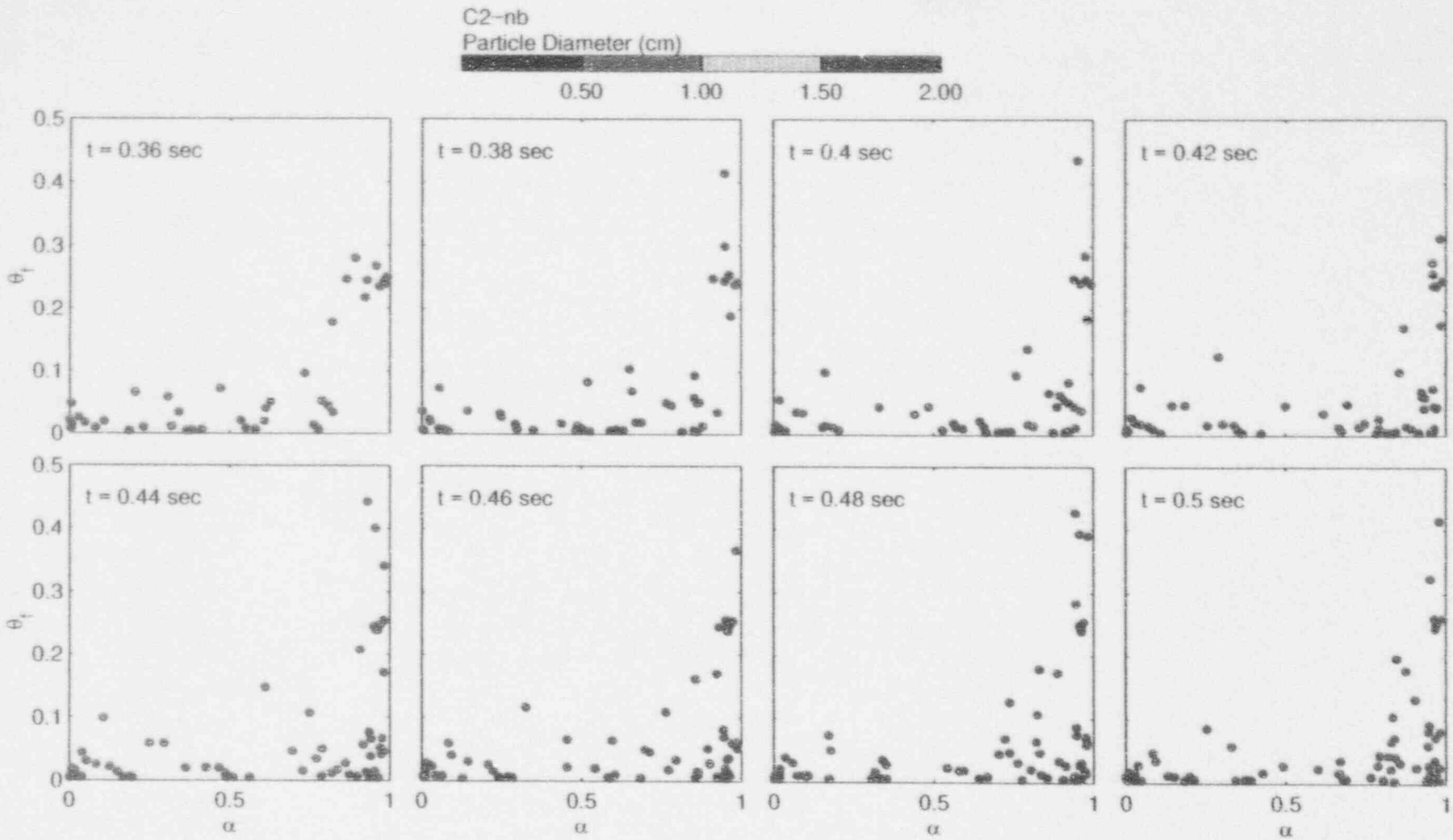
t = 0.24 sec
t = 0.26 sec
t = 0.28 sec
t = 0.3 sec
t = 0.32 sec
t = 0.34 sec
0.5
0.4
0.3
0.2
0.1
0
0.5
1
α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Particle Diameter (cm)
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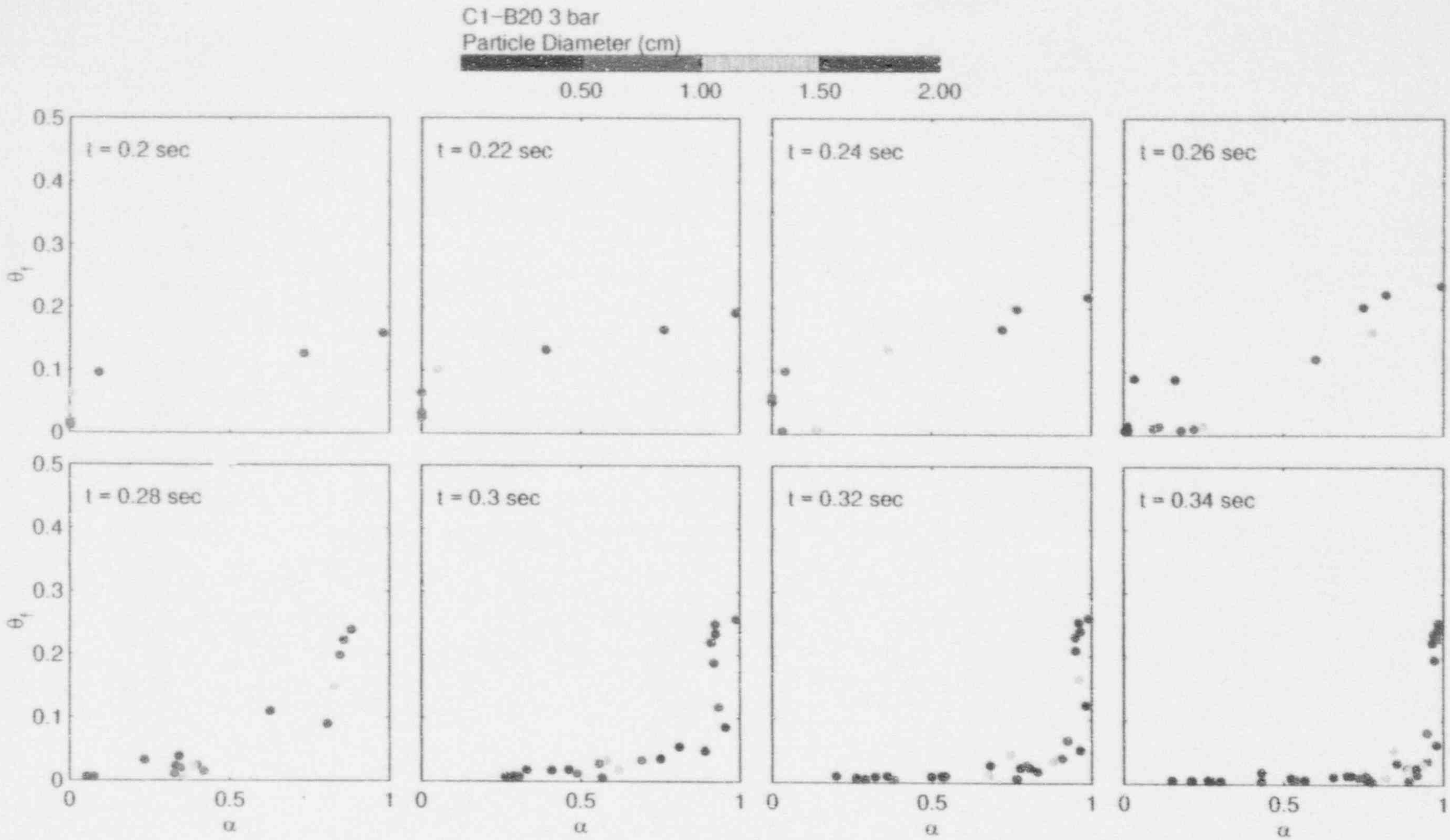
1.00
1.50
2.00
t = 0.36 sec
t = 0.38 sec
t = 0.4 sec
t = 0.42 sec
t = 0.44 sec
t = 0.46 sec
t = 0.48 sec
t = 0.5 sec
θ
α
0
0.1
0.2
0.3
0.4
0.5
0.5
1



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Particle Diameter (cm)
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1.00
1.50
2.00
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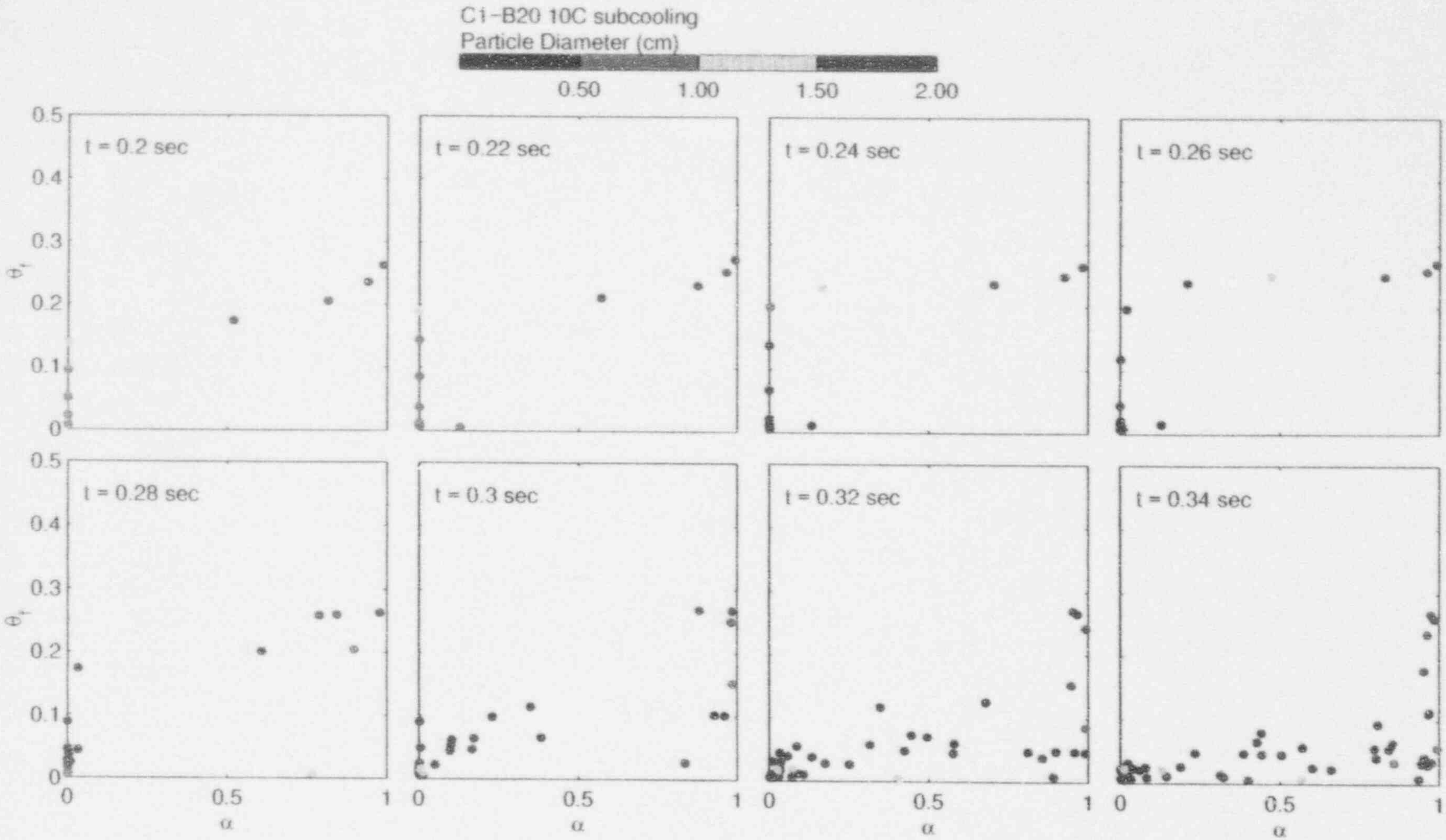
t = 0.22 sec
t = 0.24 sec
t = 0.26 sec
t = 0.28 sec
t = 0.3 sec
t = 0.32 sec
t = 0.34 sec
θf
α


C1-B20 10C subcooling
Particle Diameter (cm)
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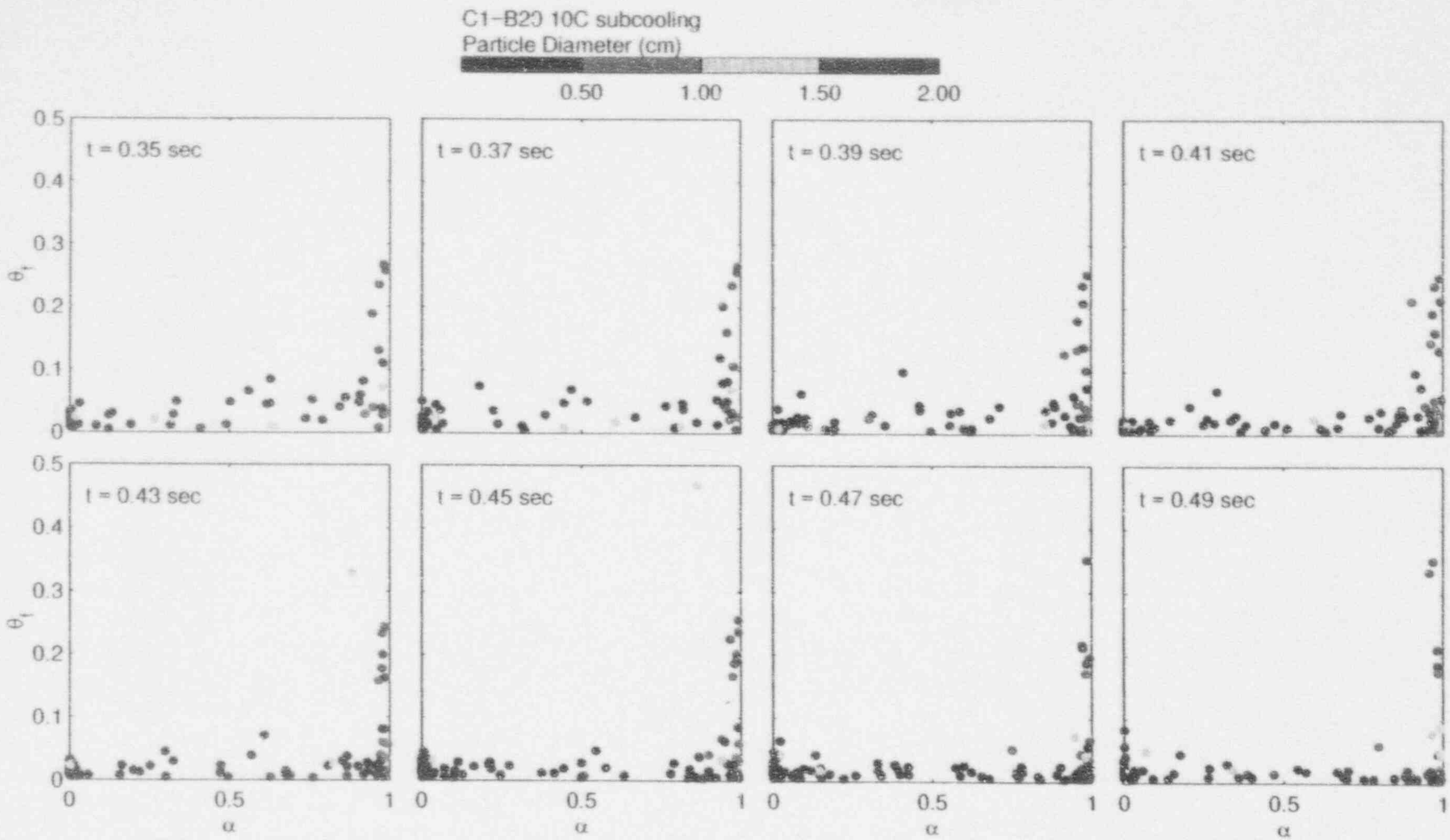
1.00
1.50
2.00
t = 0.2 sec
t = 0.22 sec
t = 0.24 sec
t = 0.26 sec
t = 0.28 sec
t = 0.3 sec
t = 0.32 sec
t = 0.34 sec
θ
α
0
0.1
0.2
0.3
0.4
0.5
0
0.5
1


C1-B20 10C subcooling
Particle Diameter (cm)
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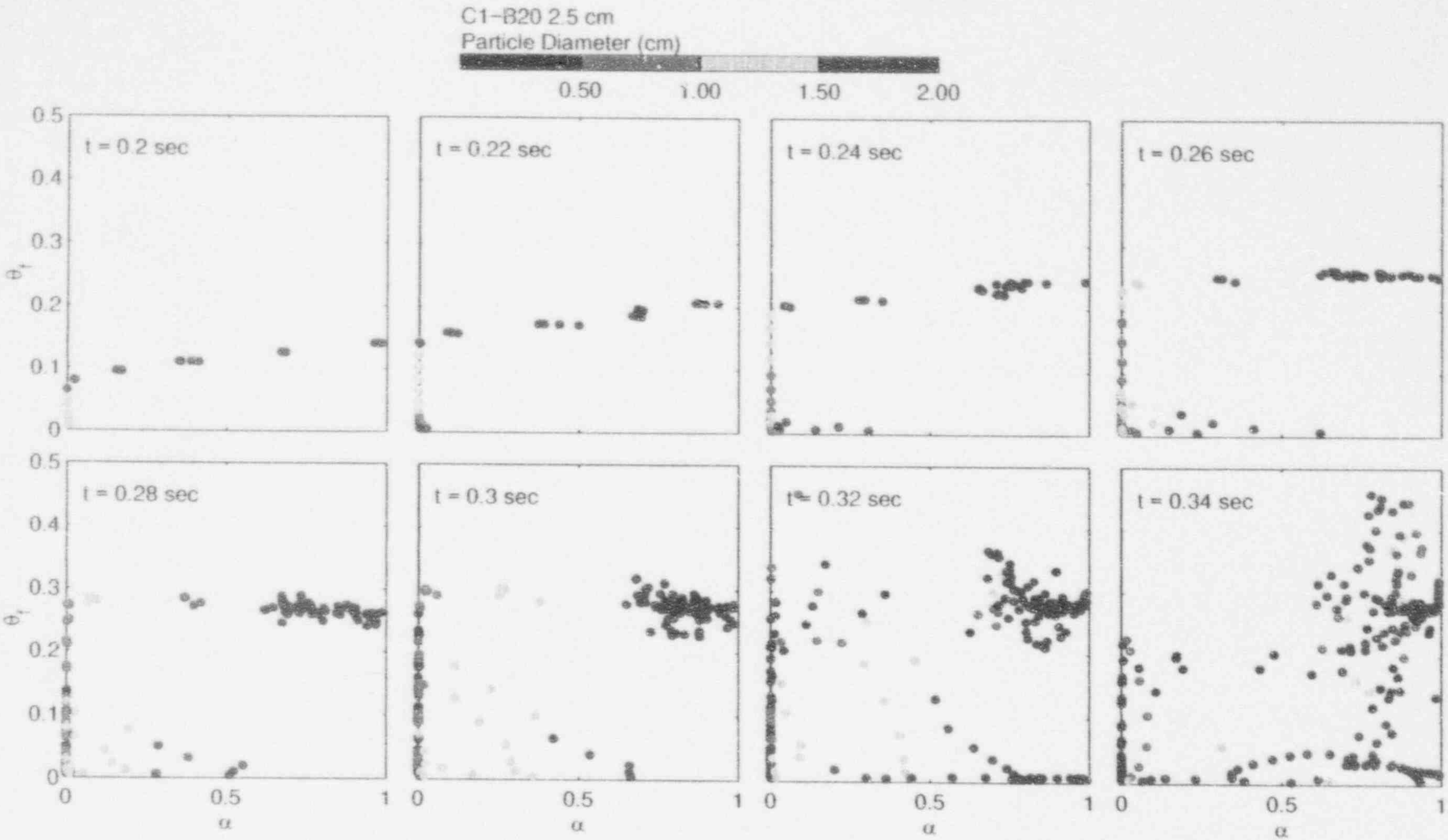
1.00
1.50
2.00
t = 0.35 sec
t = 0.37 sec
t = 0.39 sec
t = 0.41 sec
t = 0.43 sec
t = 0.45 sec
t = 0.47 sec
t = 0.49 sec
θ
α


C1-B20 2.5 cm
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1.50
2.00
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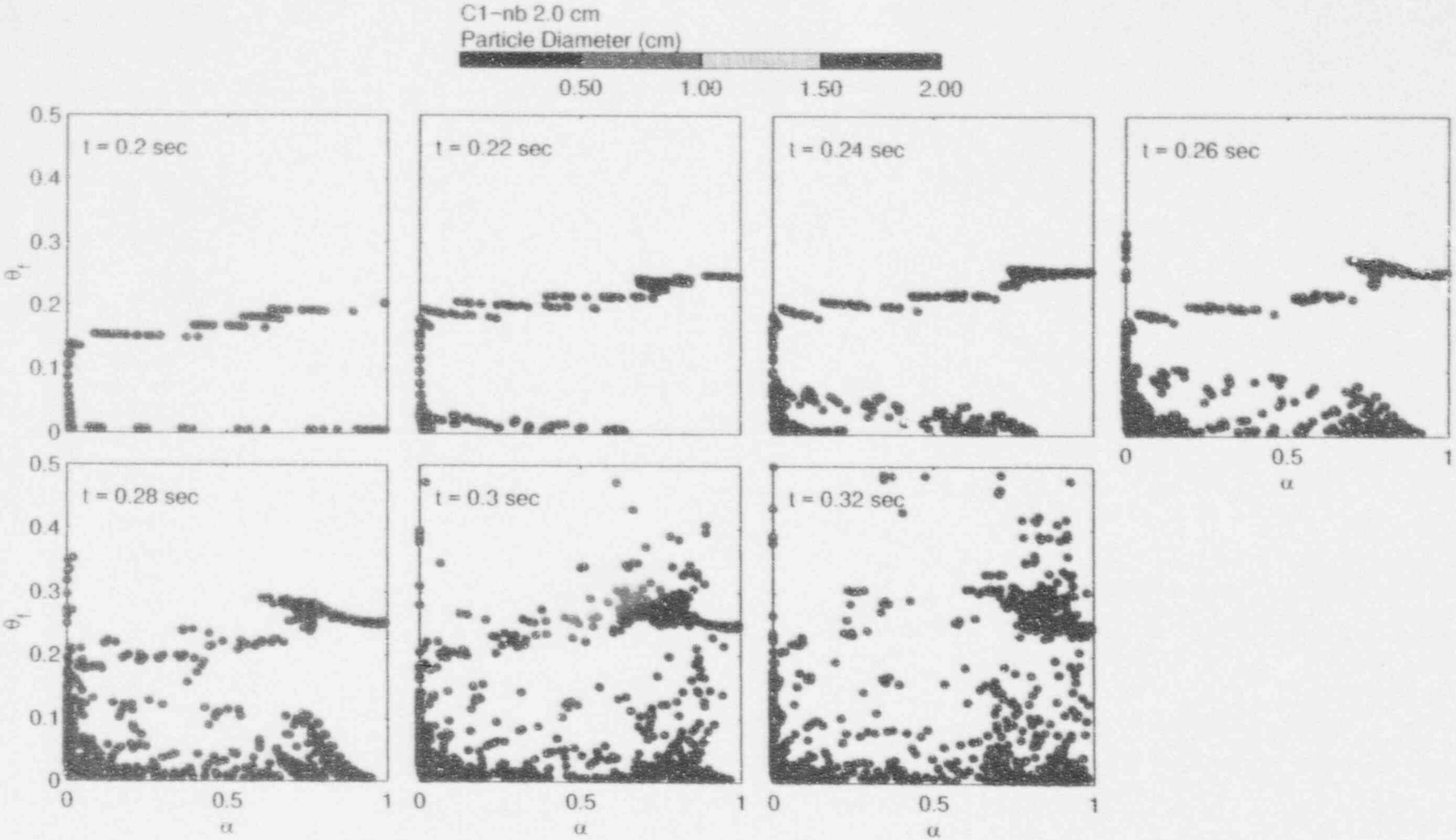
t = 0.26 sec
t = 0.28 sec
t = 0.3 sec
t = 0.32 sec
t = 0.34 sec
θ_t
α
0
0.1
0.2
0.3
0.4
0.5
0.5
1



C1-nb 2.0 cm
Particle Diameter (cm)
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1.00
1.50
2.00
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t = 0.22 sec
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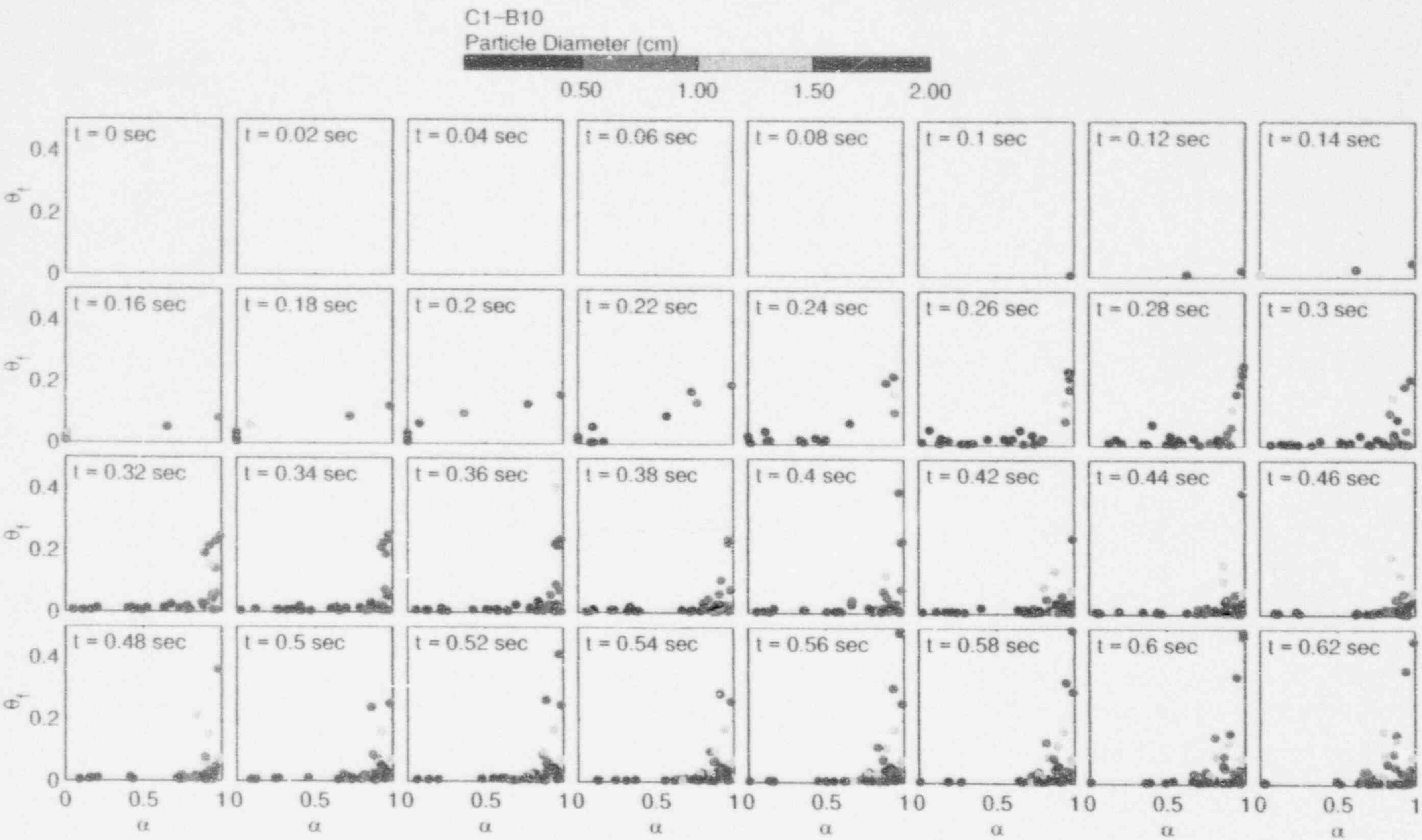
t = 0.26 sec
t = 0.28 sec
t = 0.3 sec
t = 0.32 sec
θ_t
α


C1−B10
Particle Diameter (cm)
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1.00
1.50
2.00
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t = 0.02 sec
t = 0.04 sec
t = 0.06 sec
t = 0.08 sec
t = 0.1 sec
t = 0.12 sec
t = 0.14 sec
t = 0.16 sec
t = 0.18 sec
t = 0.2 sec
t = 0.22 sec
t = 0.24 sec
t = 0.26 sec
t = 0.28 sec
t = 0.3 sec
t = 0.32 sec
t = 0.34 sec
t = 0.36 sec
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t = 0.4 sec
t = 0.42 sec
t = 0.44 sec
t = 0.46 sec
t = 0.48 sec
t = 0.5 sec
t = 0.52 sec
t = 0.54 sec
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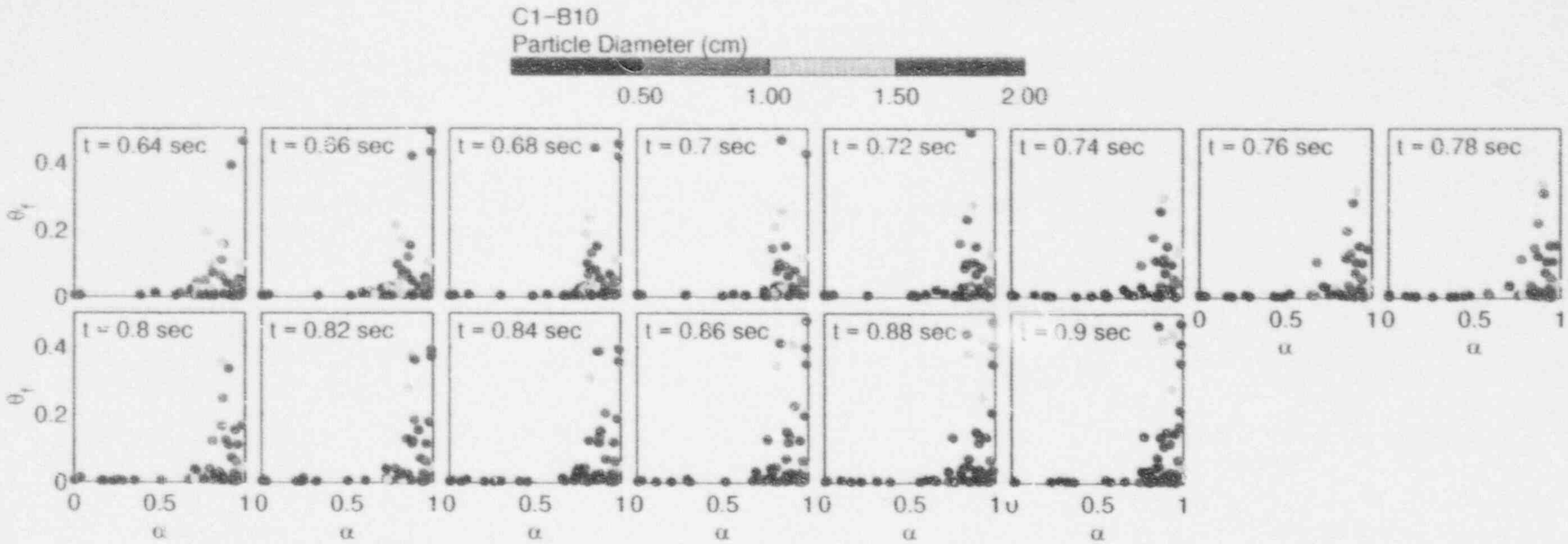
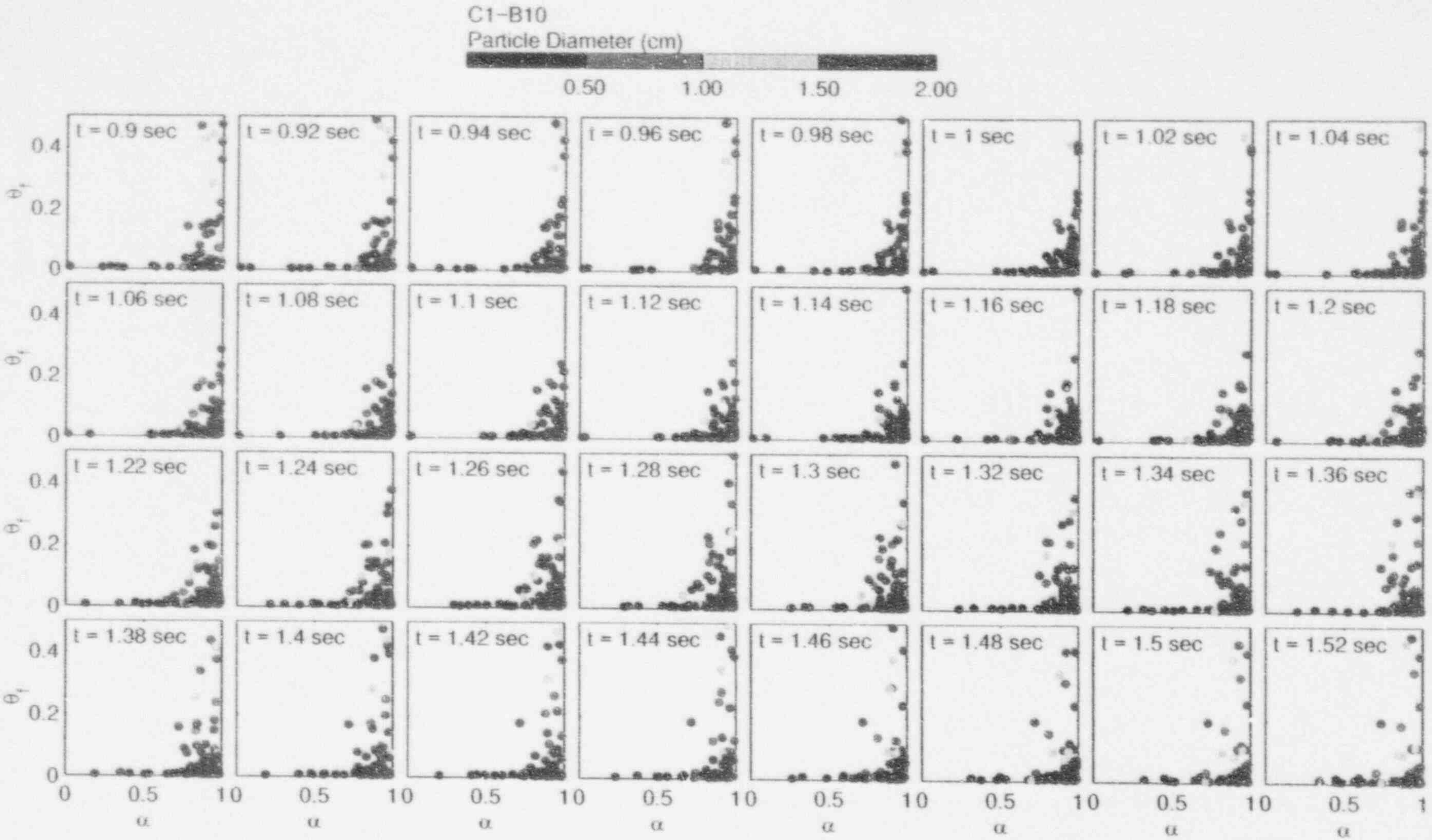
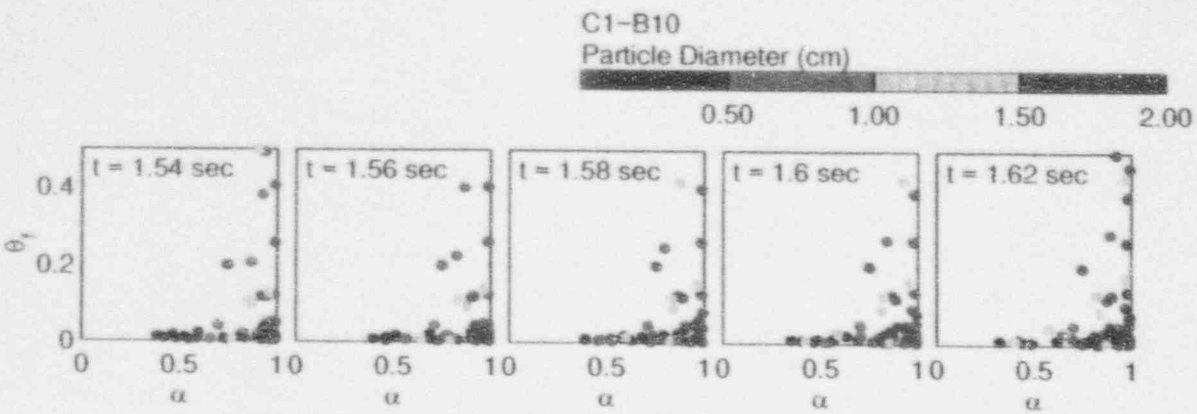
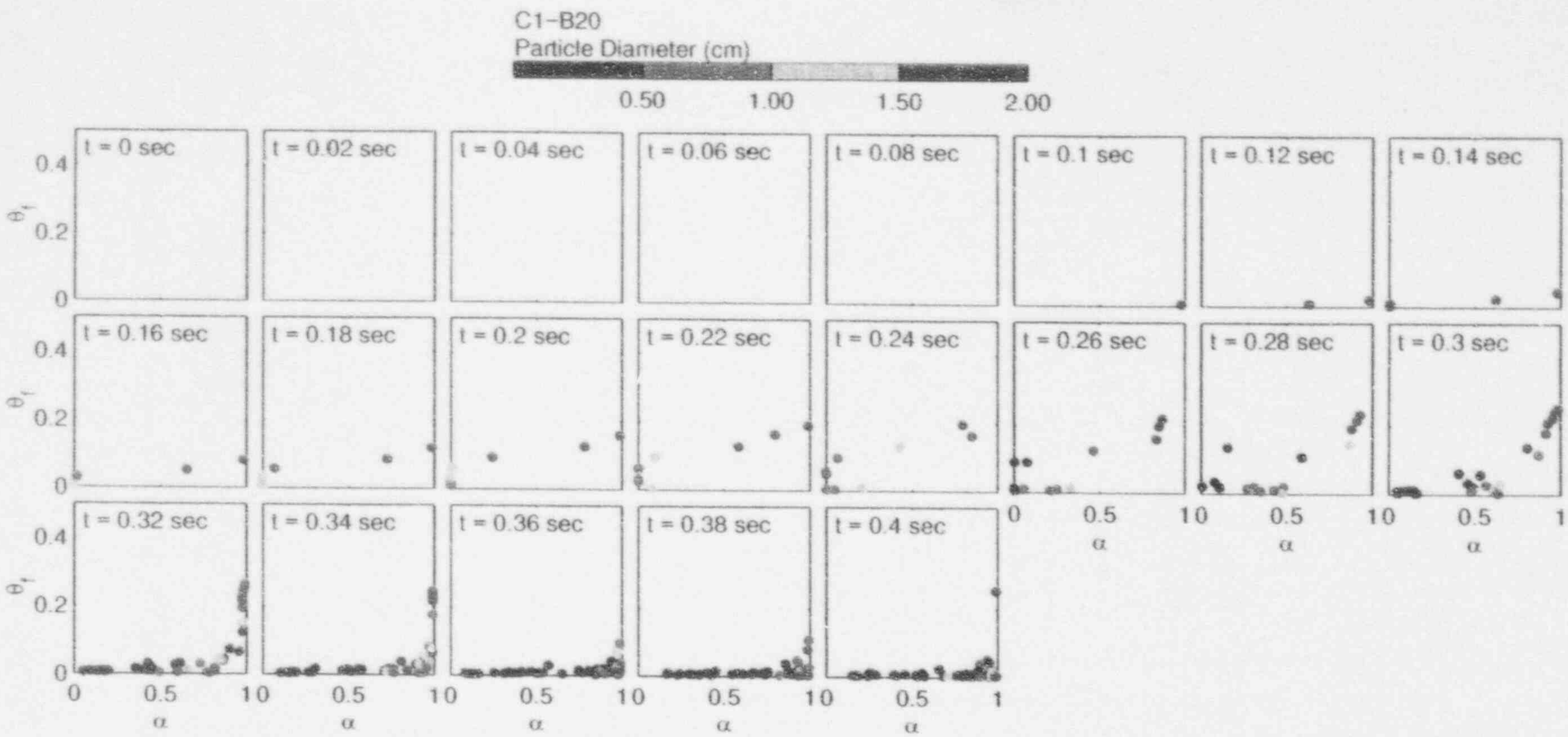
t = 0.62 sec
θ
0.4
0.2
0
0.5
1
α

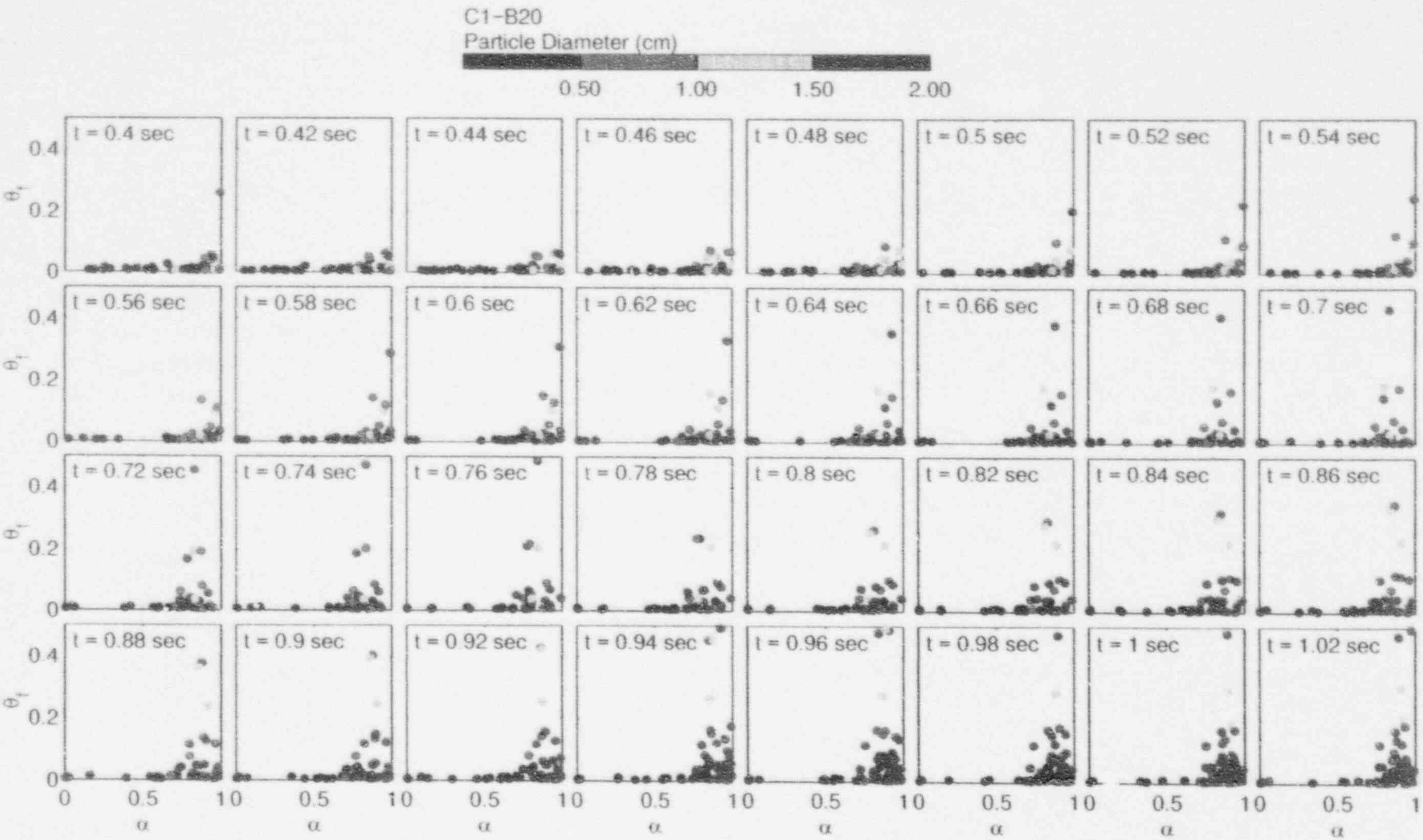

C1-B10
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 0.64 sec
t = 0.66 sec
t = 0.68 sec
t = 0.7 sec
t = 0.72 sec
t = 0.74 sec
t = 0.76 sec
t = 0.78 sec
t = 0.8 sec
t = 0.82 sec
t = 0.84 sec
t = 0.86 sec
t = 0.88 sec
t = 0.9 sec
θ_t
α



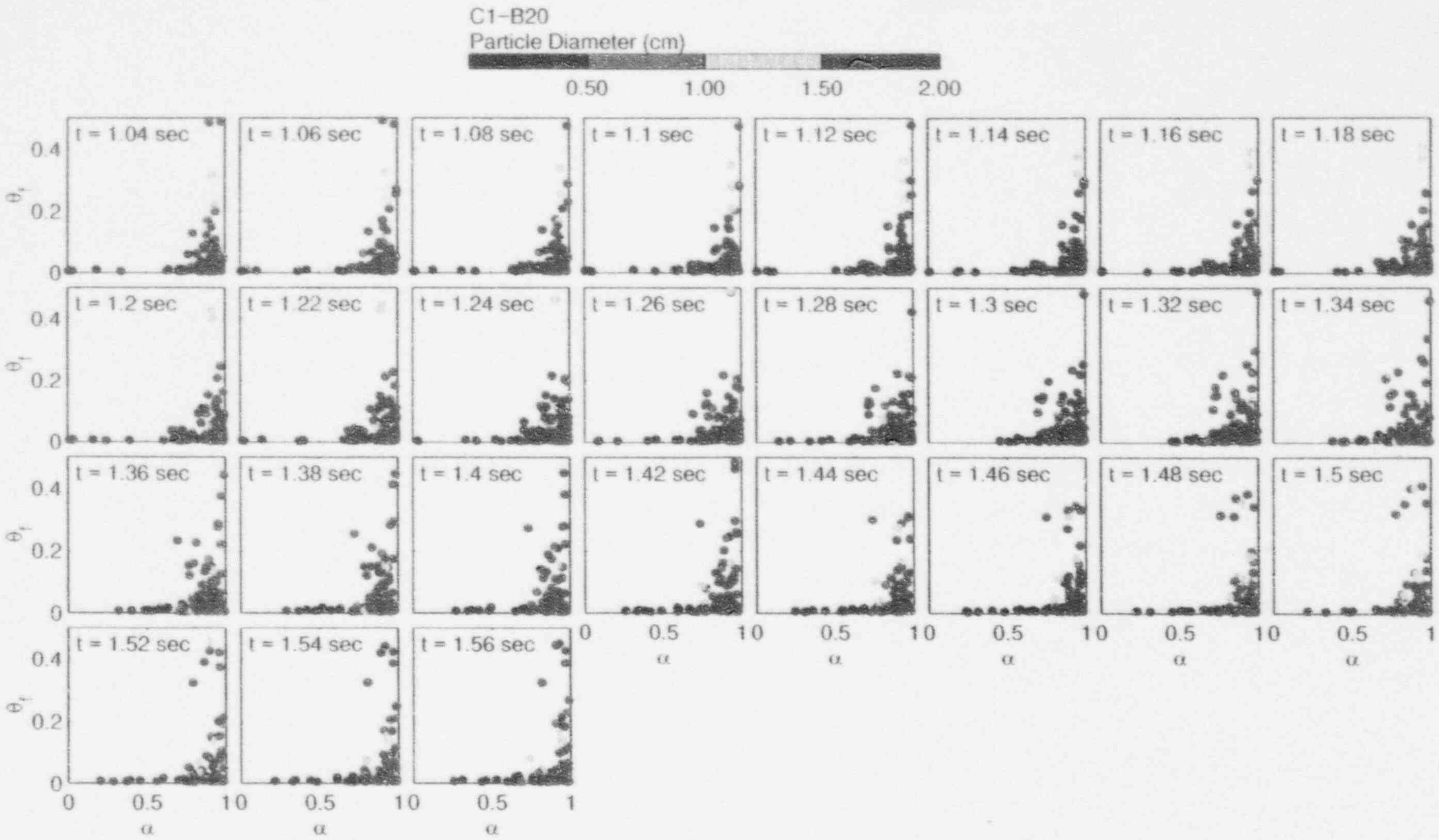
C1-B10
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 0.9 sec
t = 0.92 sec
t = 0.94 sec
t = 0.96 sec
t = 0.98 sec
t = 1 sec
t = 1.02 sec
t = 1.04 sec
t = 1.06 sec
t = 1.08 sec
t = 1.1 sec
t = 1.12 sec
t = 1.14 sec
t = 1.16 sec
t = 1.18 sec
t = 1.2 sec
t = 1.22 sec
t = 1.24 sec
t = 1.26 sec
t = 1.28 sec
t = 1.3 sec
t = 1.32 sec
t = 1.34 sec
t = 1.36 sec
t = 1.38 sec
t = 1.4 sec
t = 1.42 sec
t = 1.44 sec
t = 1.46 sec
t = 1.48 sec
t = 1.5 sec
t = 1.52 sec
0.4
0.2
0
0.5
1
α


C1-B10
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 1.54 sec
t = 1.56 sec
t = 1.58 sec
t = 1.6 sec
t = 1.62 sec
0.4
0.2
0
0
0.5
1
α
θ

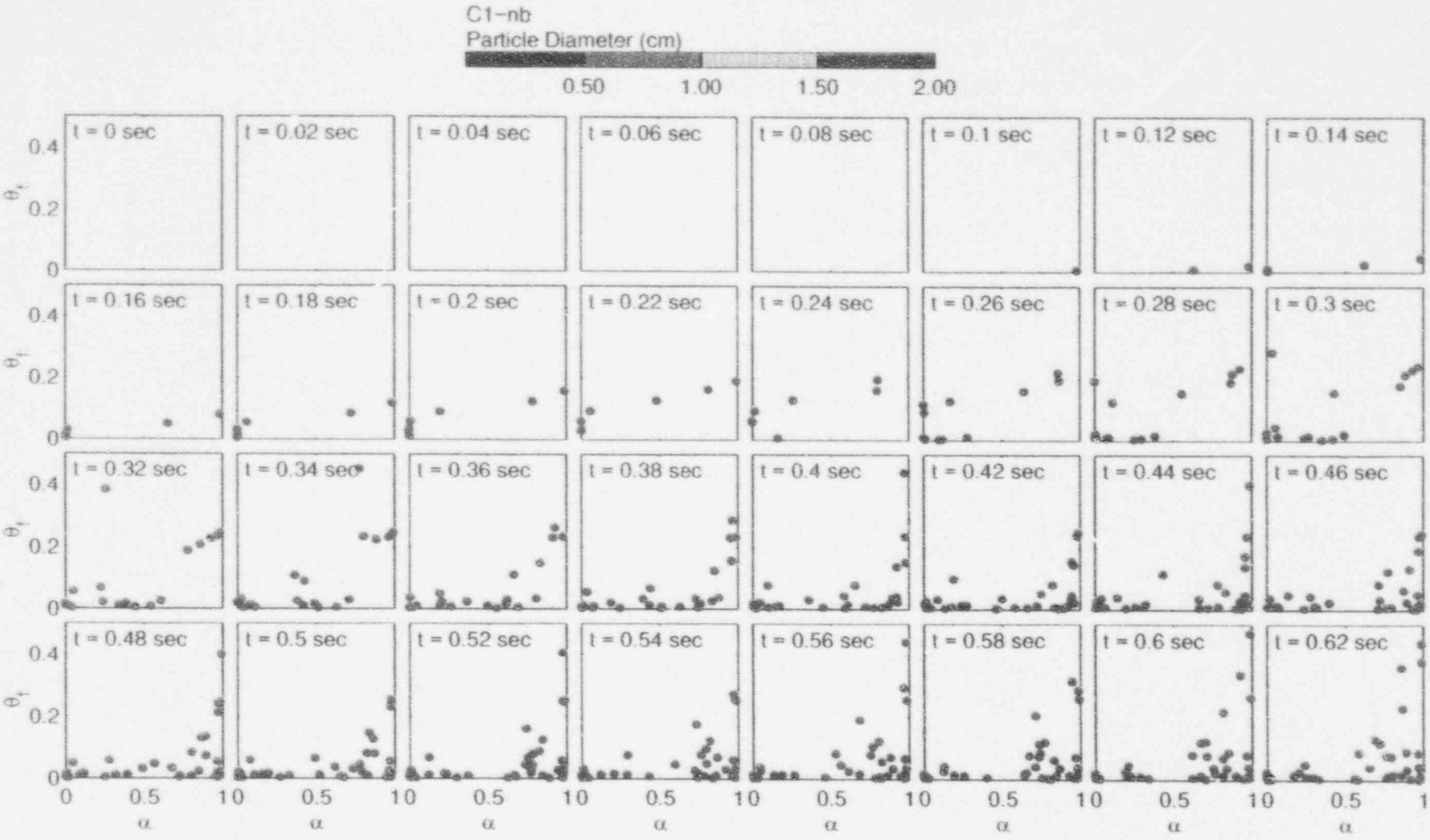

C1−B20
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 0 sec
t = 0.02 sec
t = 0.04 sec
t = 0.06 sec
t = 0.08 sec
t = 0.1 sec
t = 0.12 sec
t = 0.14 sec
t = 0.16 sec
t = 0.18 sec
t = 0.2 sec
t = 0.22 sec
t = 0.24 sec
t = 0.26 sec
t = 0.28 sec
t = 0.3 sec
t = 0.32 sec
t = 0.34 sec
t = 0.36 sec
t = 0.38 sec
t = 0.4 sec
θ_f
α

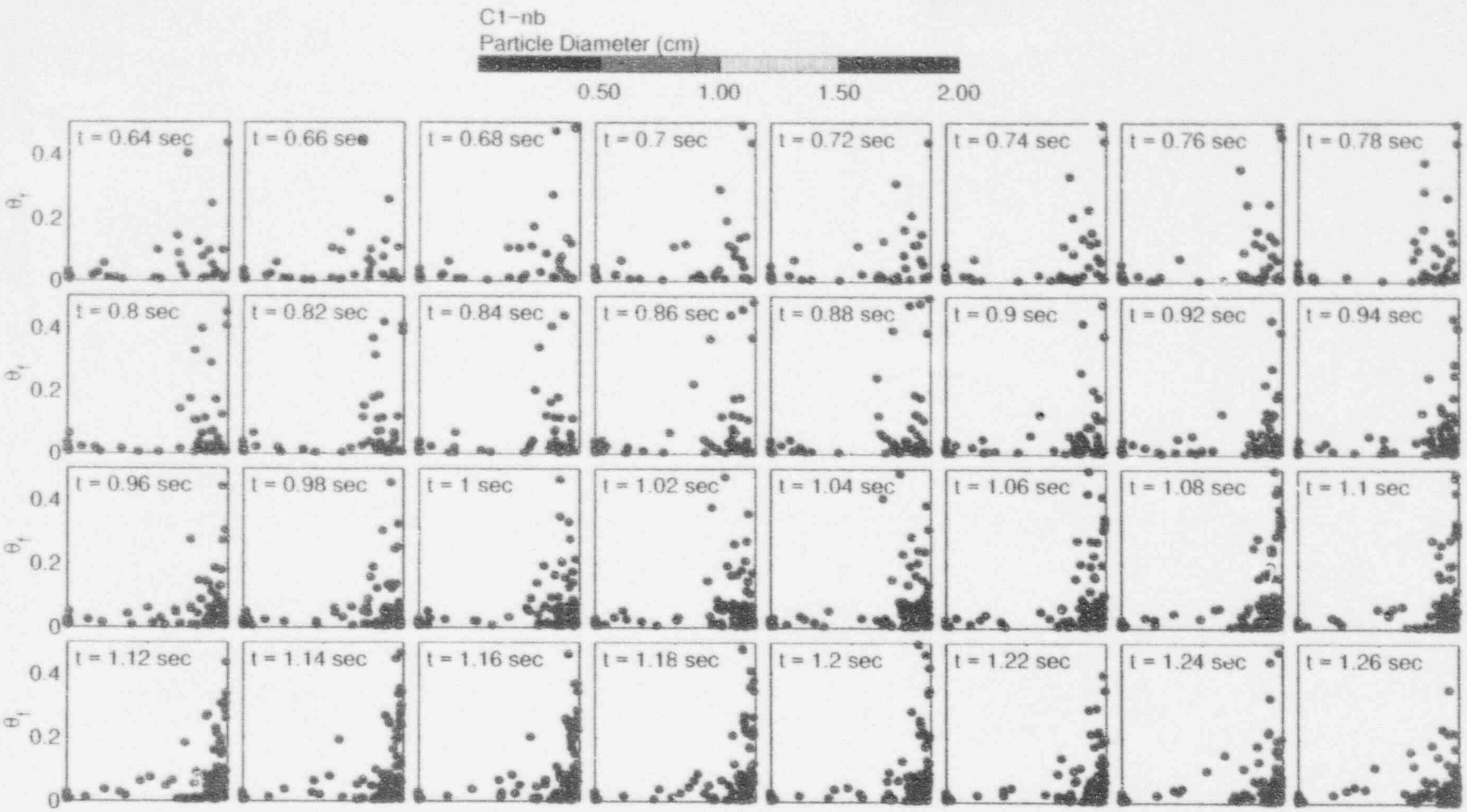


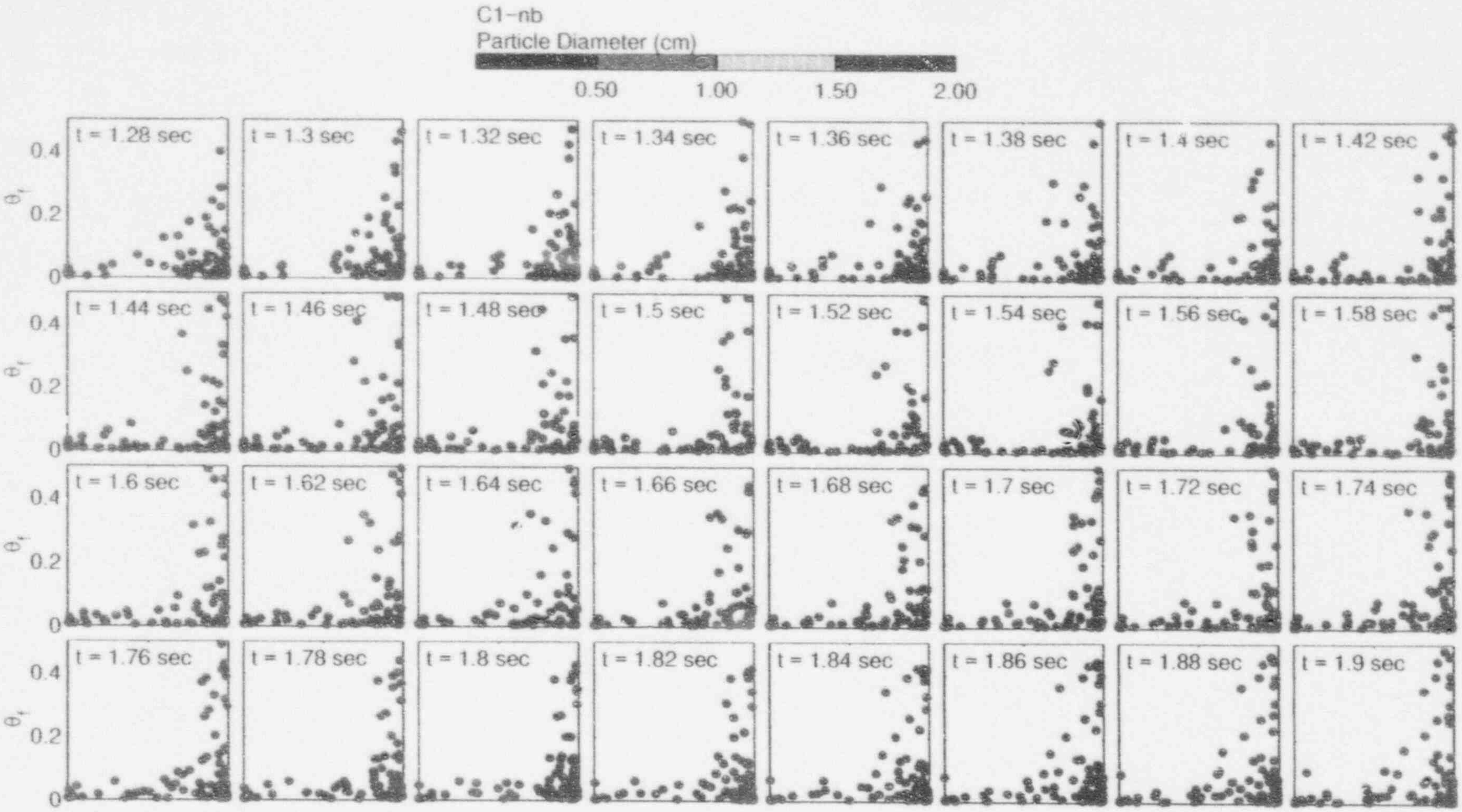
C1-B20
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 0.4 sec
t = 0.42 sec
t = 0.44 sec
t = 0.46 sec
t = 0.48 sec
t = 0.5 sec
t = 0.52 sec
t = 0.54 sec
t = 0.56 sec
t = 0.58 sec
t = 0.6 sec
t = 0.62 sec
t = 0.64 sec
t = 0.66 sec
t = 0.68 sec
t = 0.7 sec
t = 0.72 sec
t = 0.74 sec
t = 0.76 sec
t = 0.78 sec
t = 0.8 sec
t = 0.82 sec
t = 0.84 sec
t = 0.86 sec
t = 0.88 sec
t = 0.9 sec
t = 0.92 sec
t = 0.94 sec
t = 0.96 sec
t = 0.98 sec
t = 1 sec
t = 1.02 sec
θ
0.4
0.2
0
0.5
1
α

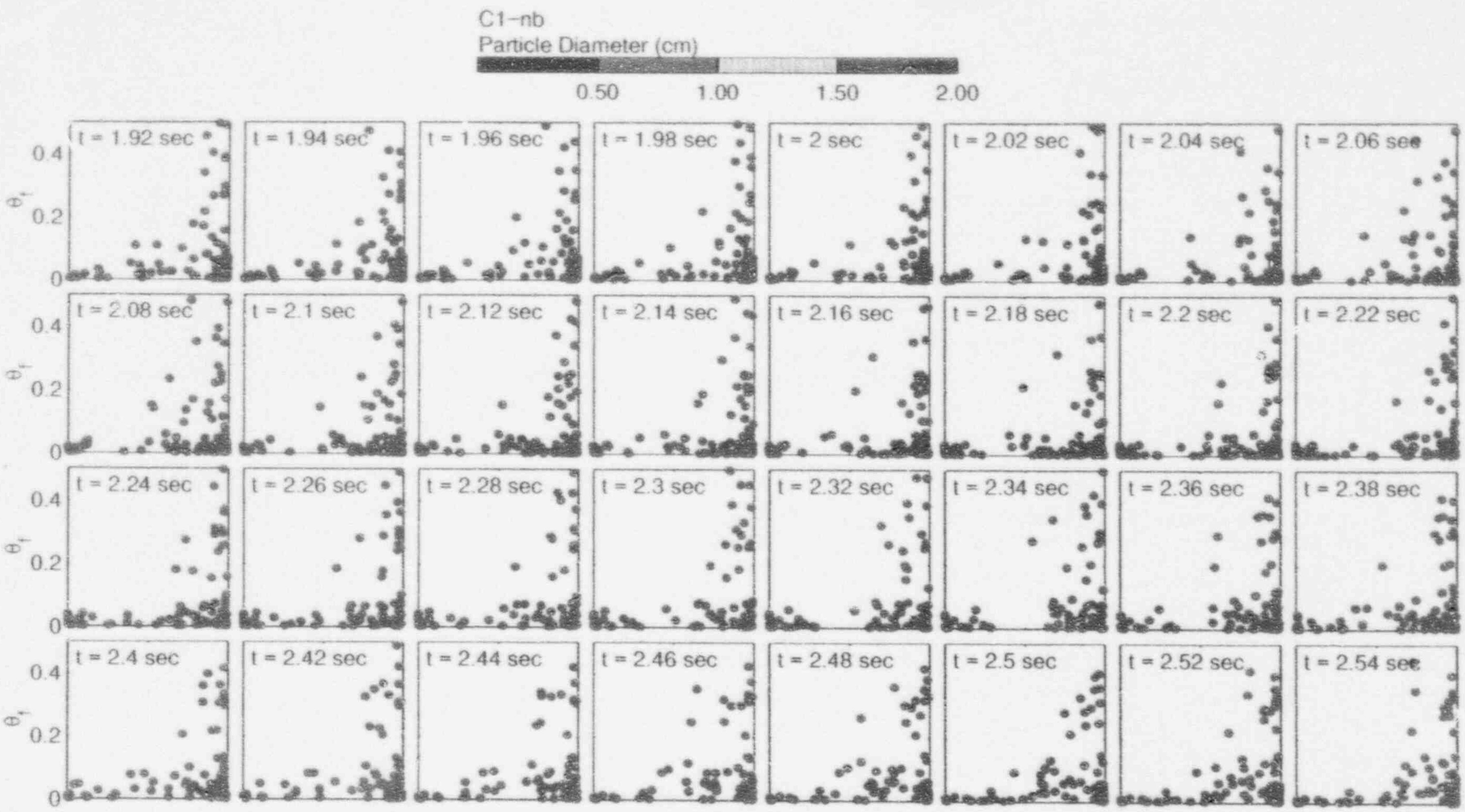


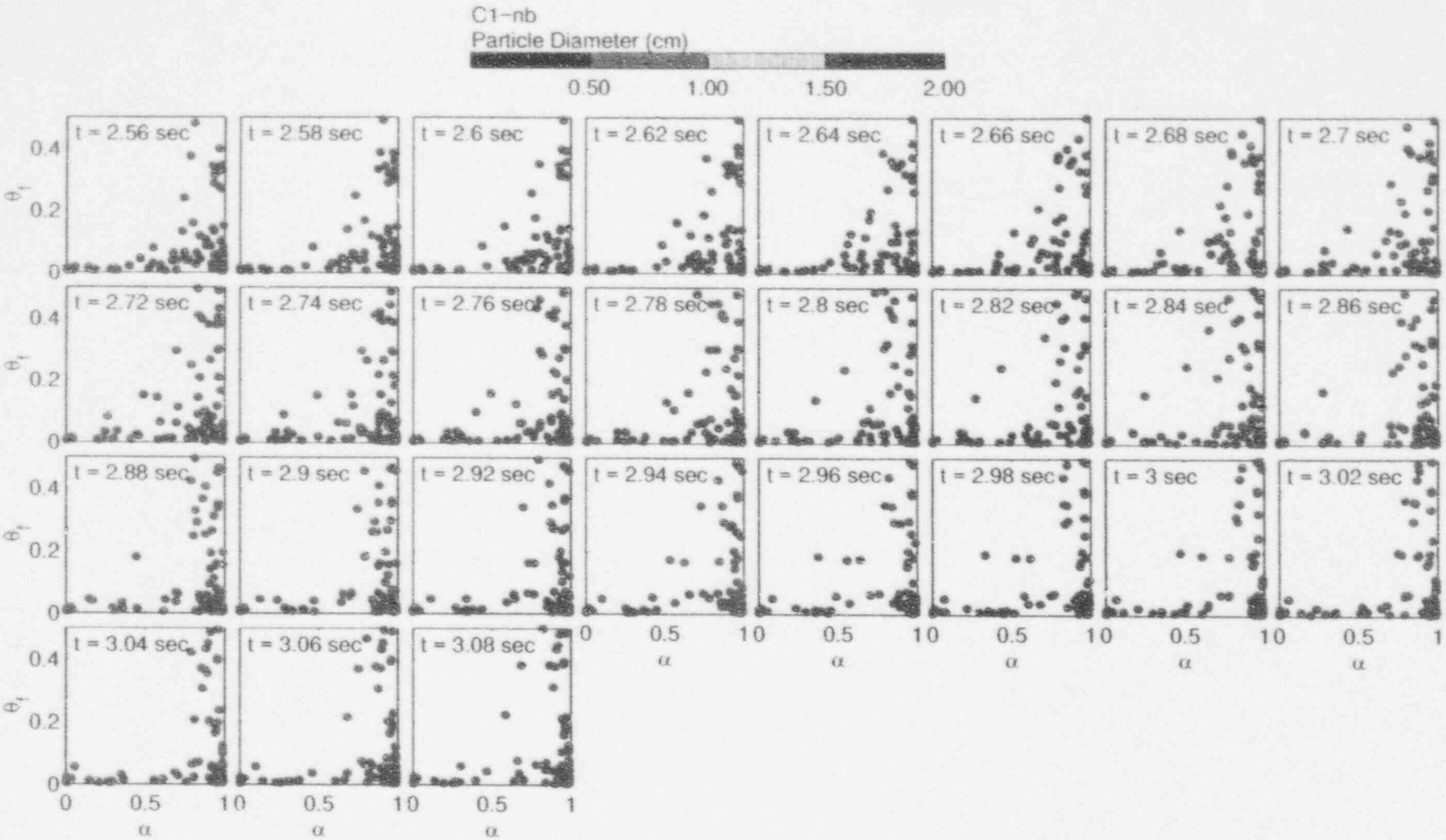
C1-B20
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 1.04 sec
t = 1.06 sec
t = 1.08 sec
t = 1.1 sec
t = 1.12 sec
t = 1.14 sec
t = 1.16 sec
t = 1.18 sec
t = 1.2 sec
t = 1.22 sec
t = 1.24 sec
t = 1.26 sec
t = 1.28 sec
t = 1.3 sec
t = 1.32 sec
t = 1.34 sec
t = 1.36 sec
t = 1.38 sec
t = 1.4 sec
t = 1.42 sec
t = 1.44 sec
t = 1.46 sec
t = 1.48 sec
t = 1.5 sec
t = 1.52 sec
t = 1.54 sec
t = 1.56 sec
θ_f
α

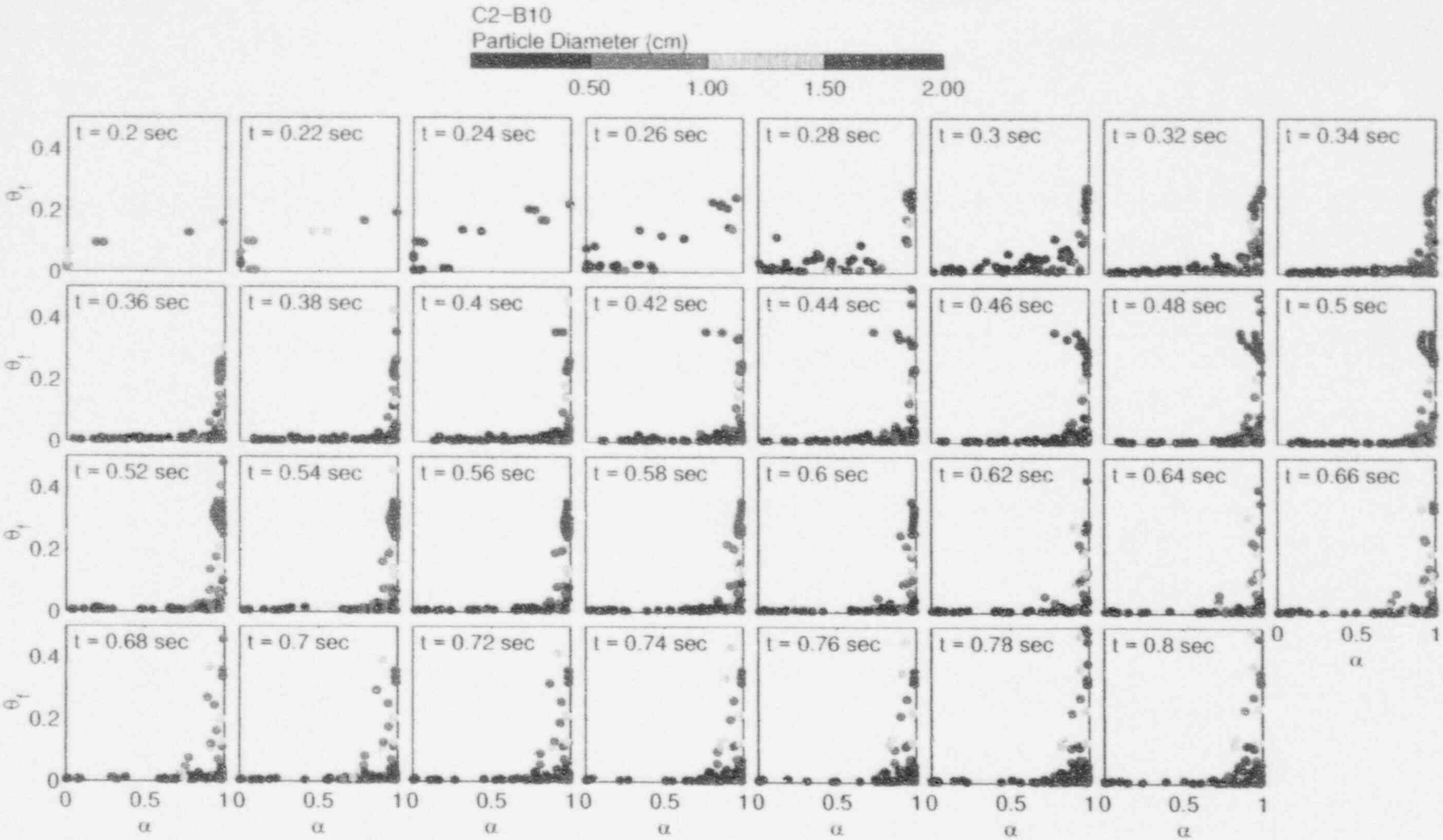

C1-nb
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 0 sec
t = 0.02 sec
t = 0.04 sec
t = 0.06 sec
t = 0.08 sec
t = 0.1 sec
t = 0.12 sec
t = 0.14 sec
t = 0.16 sec
t = 0.18 sec
t = 0.2 sec
t = 0.22 sec
t = 0.24 sec
t = 0.26 sec
t = 0.28 sec
t = 0.3 sec
t = 0.32 sec
t = 0.34 sec
t = 0.36 sec
t = 0.38 sec
t = 0.4 sec
t = 0.42 sec
t = 0.44 sec
t = 0.46 sec
t = 0.48 sec
t = 0.5 sec
t = 0.52 sec
t = 0.54 sec
t = 0.56 sec
t = 0.58 sec
t = 0.6 sec
t = 0.62 sec
θ
0.4
0.2
0
0.5
1
α


C1-nb
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 0.64 sec
t = 0.66 sec
t = 0.68 sec
t = 0.7 sec
t = 0.72 sec
t = 0.74 sec
t = 0.76 sec
t = 0.78 sec
t = 0.8 sec
t = 0.82 sec
t = 0.84 sec
t = 0.86 sec
t = 0.88 sec
t = 0.9 sec
t = 0.92 sec
t = 0.94 sec
t = 0.96 sec
t = 0.98 sec
t = 1 sec
t = 1.02 sec
t = 1.04 sec
t = 1.06 sec
t = 1.08 sec
t = 1.1 sec
t = 1.12 sec
t = 1.14 sec
t = 1.16 sec
t = 1.18 sec
t = 1.2 sec
t = 1.22 sec
t = 1.24 sec
t = 1.26 sec
θ_t
0.4
0.2
0

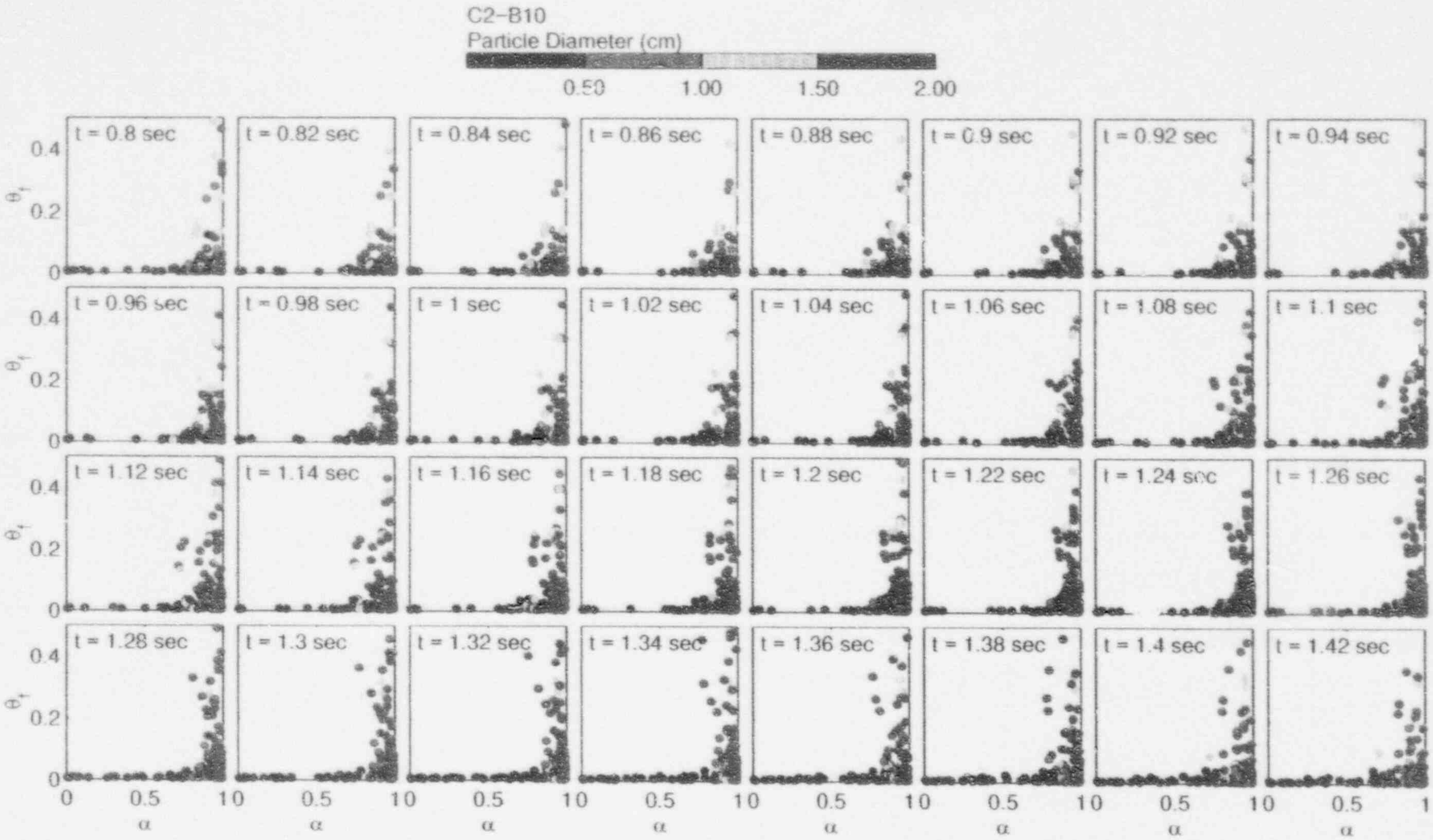

C1-nb
Particle Diameter (cm)
0.50
1.00
1.50
2.00
θ_f
0
0.2
0.4
t = 1.28 sec
t = 1.3 sec
t = 1.32 sec
t = 1.34 sec
t = 1.36 sec
t = 1.38 sec
t = 1.4 sec
t = 1.42 sec
t = 1.44 sec
t = 1.46 sec
t = 1.48 sec
t = 1.5 sec
t = 1.52 sec
t = 1.54 sec
t = 1.56 sec
t = 1.58 sec
t = 1.6 sec
t = 1.62 sec
t = 1.64 sec
t = 1.66 sec
t = 1.68 sec
t = 1.7 sec
t = 1.72 sec
t = 1.74 sec
t = 1.76 sec
t = 1.78 sec
t = 1.8 sec
t = 1.82 sec
t = 1.84 sec
t = 1.86 sec
t = 1.88 sec
t = 1.9 sec

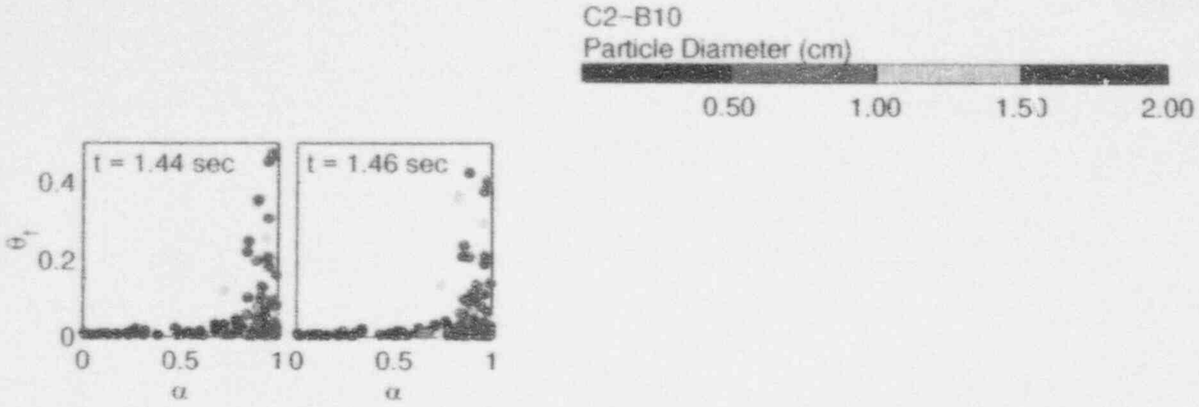

C1-nb
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 1.92 sec
t = 1.94 sec
t = 1.96 sec
t = 1.98 sec
t = 2 sec
t = 2.02 sec
t = 2.04 sec
t = 2.06 sec
t = 2.08 sec
t = 2.1 sec
t = 2.12 sec
t = 2.14 sec
t = 2.16 sec
t = 2.18 sec
t = 2.2 sec
t = 2.22 sec
t = 2.24 sec
t = 2.26 sec
t = 2.28 sec
t = 2.3 sec
t = 2.32 sec
t = 2.34 sec
t = 2.36 sec
t = 2.38 sec
t = 2.4 sec
t = 2.42 sec
t = 2.44 sec
t = 2.46 sec
t = 2.48 sec
t = 2.5 sec
t = 2.52 sec
t = 2.54 sec
θ_t
0.4
0.2
0

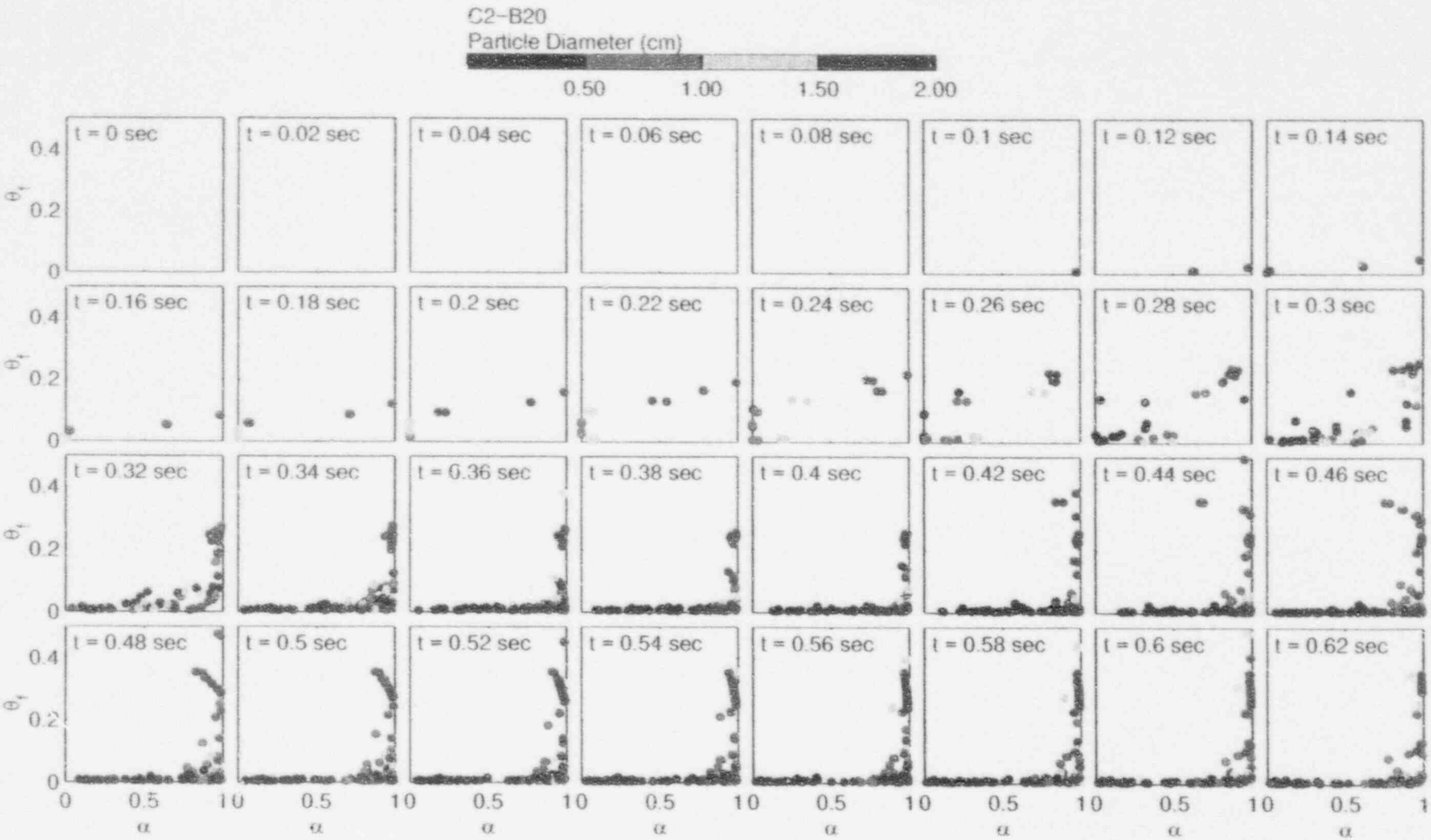

C1-nb
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 2.56 sec
t = 2.58 sec
t = 2.6 sec
t = 2.62 sec
t = 2.64 sec
t = 2.66 sec
t = 2.68 sec
t = 2.7 sec
t = 2.72 sec
t = 2.74 sec
t = 2.76 sec
t = 2.78 sec
t = 2.8 sec
t = 2.82 sec
t = 2.84 sec
t = 2.86 sec
t = 2.88 sec
t = 2.9 sec
t = 2.92 sec
t = 2.94 sec
t = 2.96 sec
t = 2.98 sec
t = 3 sec
t = 3.02 sec
t = 3.04 sec
t = 3.06 sec
t = 3.08 sec
θ_f
α

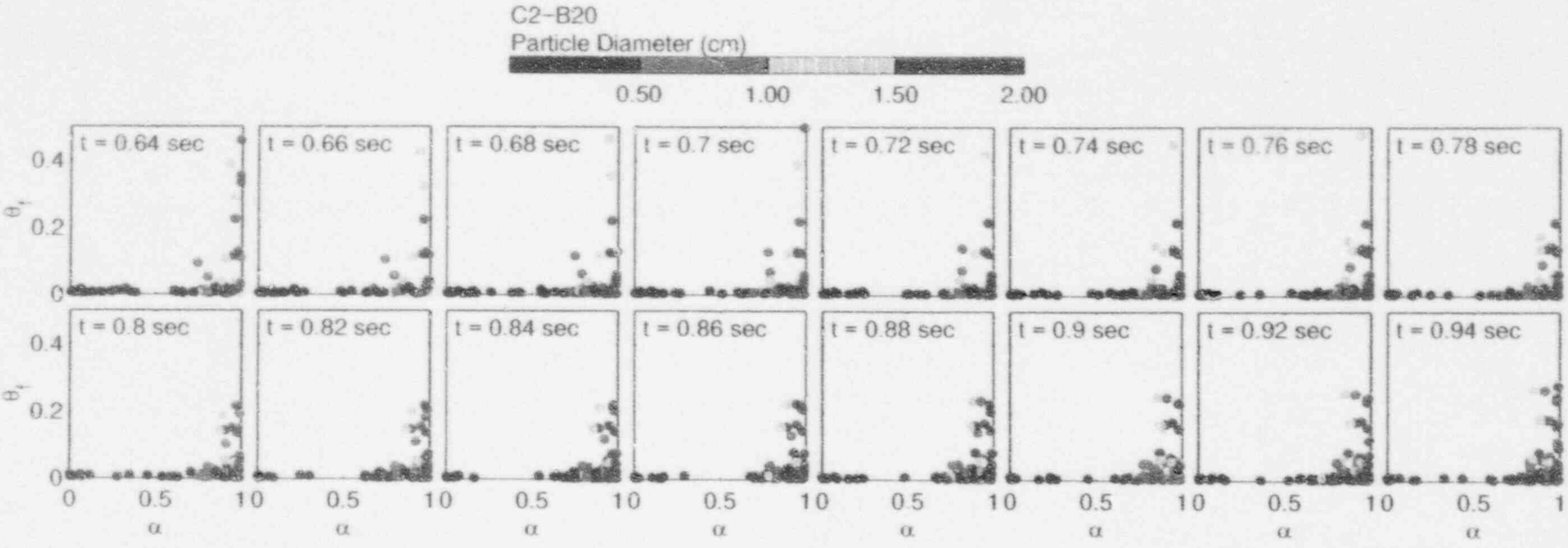


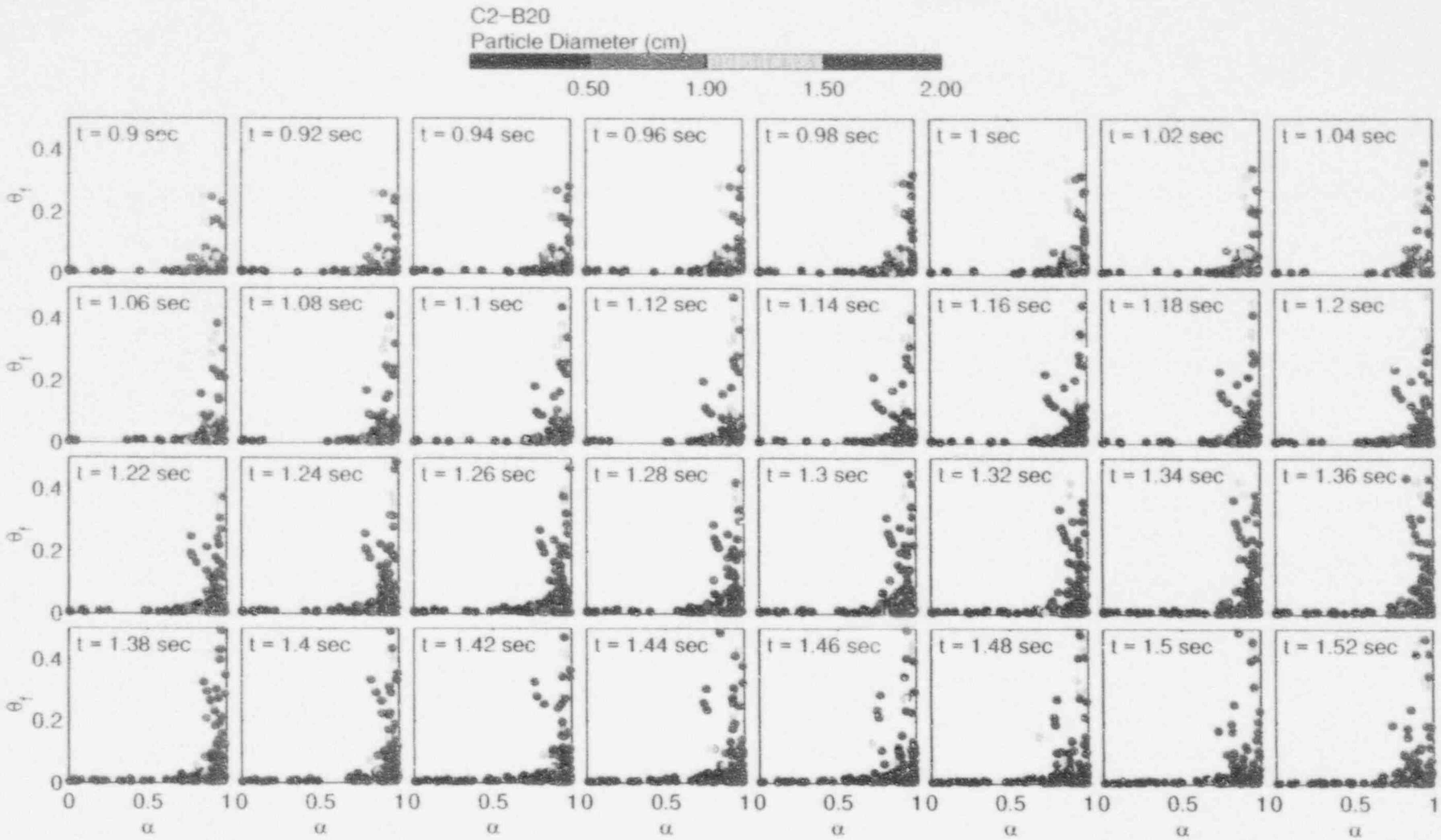
C2-B10
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 0.2 sec
t = 0.22 sec
t = 0.24 sec
t = 0.26 sec
t = 0.28 sec
t = 0.3 sec
t = 0.32 sec
t = 0.34 sec
t = 0.36 sec
t = 0.38 sec
t = 0.4 sec
t = 0.42 sec
t = 0.44 sec
t = 0.46 sec
t = 0.48 sec
t = 0.5 sec
t = 0.52 sec
t = 0.54 sec
t = 0.56 sec
t = 0.58 sec
t = 0.6 sec
t = 0.62 sec
t = 0.64 sec
t = 0.66 sec
t = 0.68 sec
t = 0.7 sec
t = 0.72 sec
t = 0.74 sec
t = 0.76 sec
t = 0.78 sec
t = 0.8 sec
θ
α

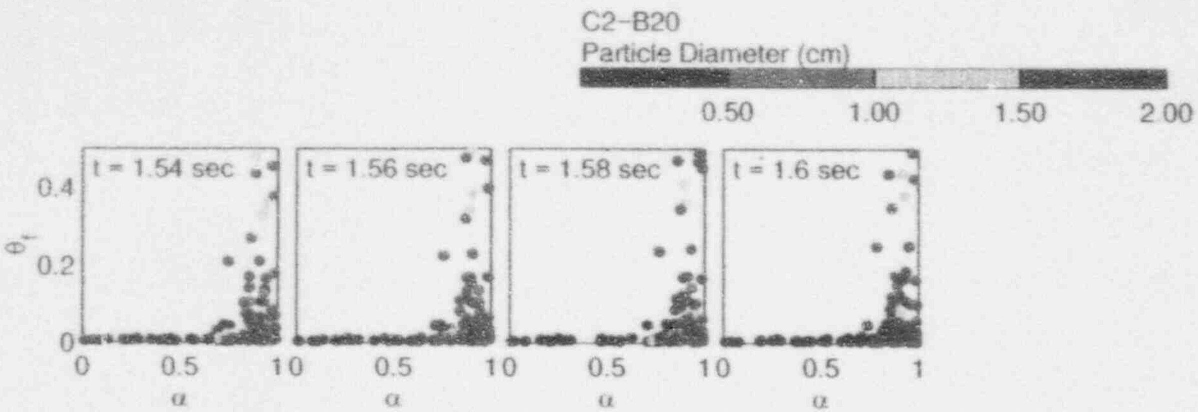

C2-B10
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 0.8 sec
t = 0.82 sec
t = 0.84 sec
t = 0.86 sec
t = 0.88 sec
t = 0.9 sec
t = 0.92 sec
t = 0.94 sec
t = 0.96 sec
t = 0.98 sec
t = 1 sec
t = 1.02 sec
t = 1.04 sec
t = 1.06 sec
t = 1.08 sec
t = 1.1 sec
t = 1.12 sec
t = 1.14 sec
t = 1.16 sec
t = 1.18 sec
t = 1.2 sec
t = 1.22 sec
t = 1.24 sec
t = 1.26 sec
t = 1.28 sec
t = 1.3 sec
t = 1.32 sec
t = 1.34 sec
t = 1.36 sec
t = 1.38 sec
t = 1.4 sec
t = 1.42 sec
θ_f
α

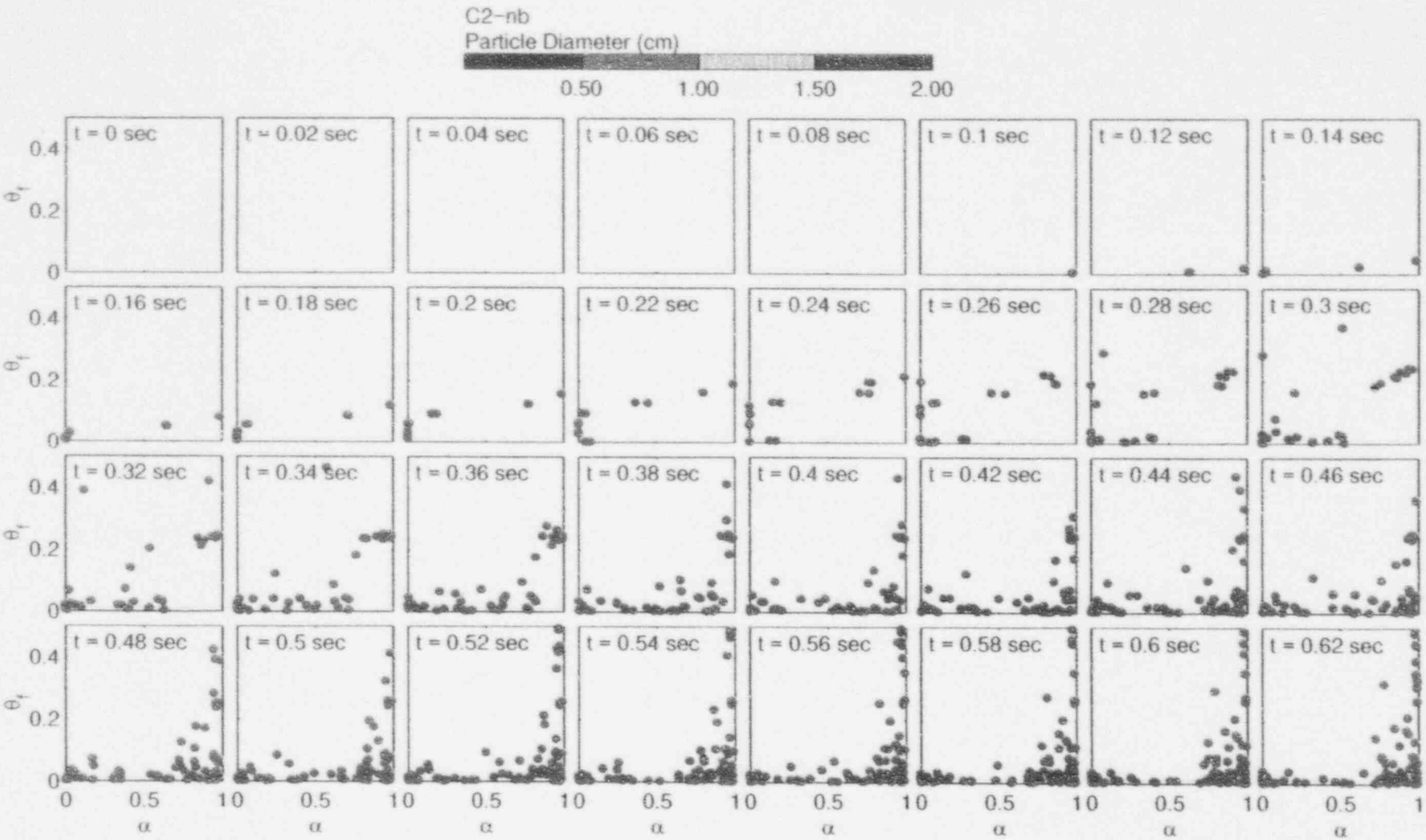

C2-B10
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 1.44 sec
t = 1.46 sec
0.4
0.2
0
0.5
1
θ_f
α

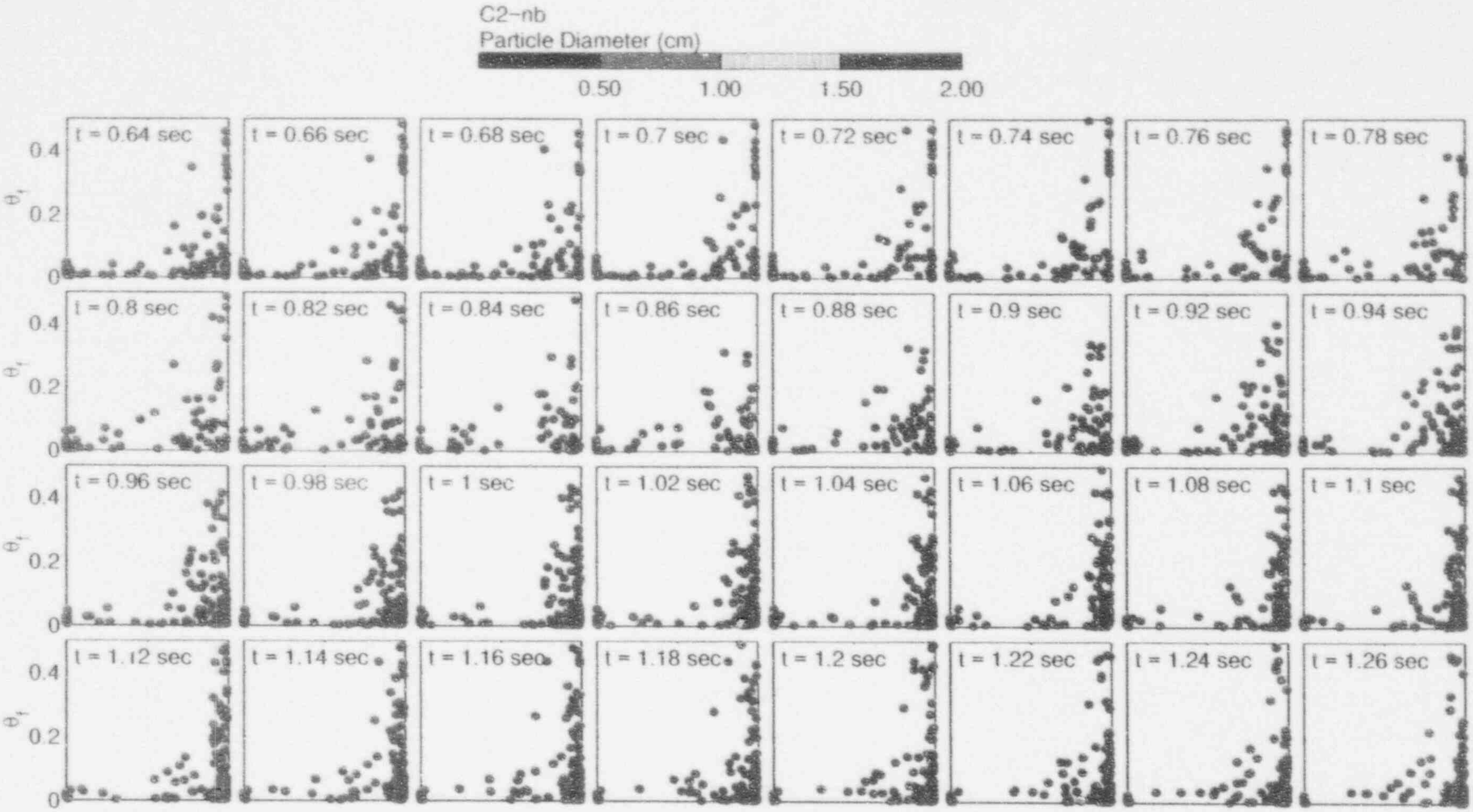

C2-B20
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 0 sec
t = 0.02 sec
t = 0.04 sec
t = 0.06 sec
t = 0.08 sec
t = 0.1 sec
t = 0.12 sec
t = 0.14 sec
t = 0.16 sec
t = 0.18 sec
t = 0.2 sec
t = 0.22 sec
t = 0.24 sec
t = 0.26 sec
t = 0.28 sec
t = 0.3 sec
t = 0.32 sec
t = 0.34 sec
t = 0.36 sec
t = 0.38 sec
t = 0.4 sec
t = 0.42 sec
t = 0.44 sec
t = 0.46 sec
t = 0.48 sec
t = 0.5 sec
t = 0.52 sec
t = 0.54 sec
t = 0.56 sec
t = 0.58 sec
t = 0.6 sec
t = 0.62 sec
θ_t
α

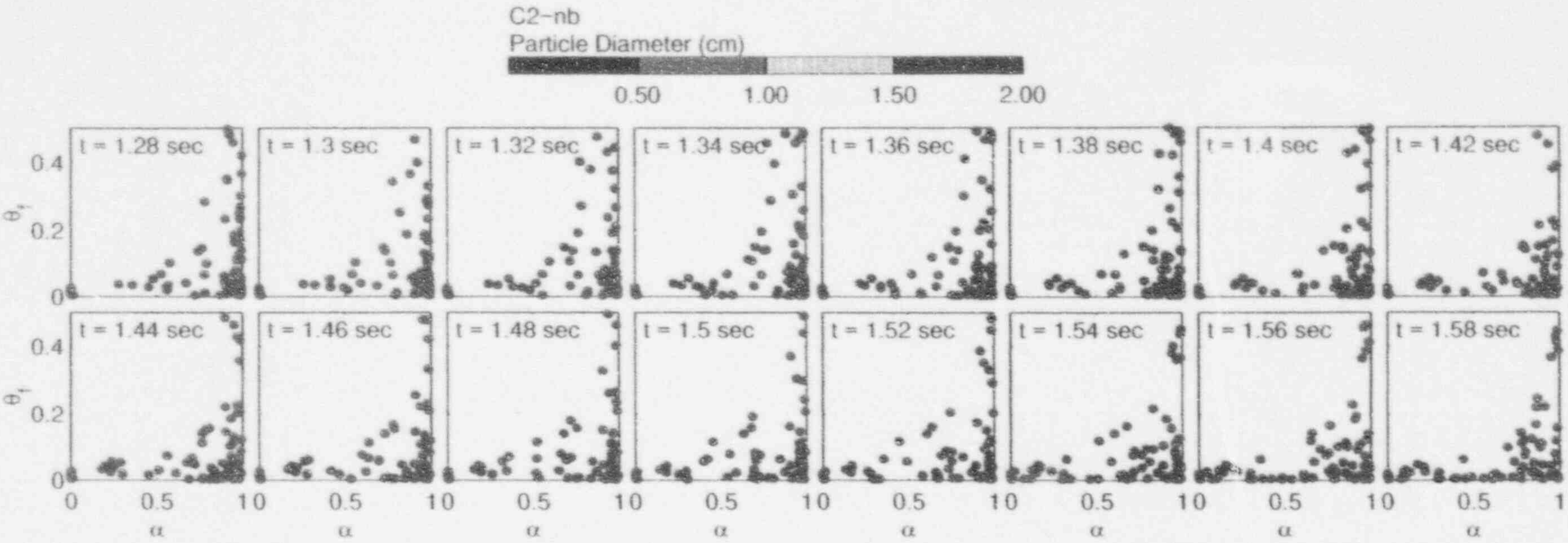

C2-B20
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 0.64 sec
t = 0.66 sec
t = 0.68 sec
t = 0.7 sec
t = 0.72 sec
t = 0.74 sec
t = 0.76 sec
t = 0.78 sec
t = 0.8 sec
t = 0.82 sec
t = 0.84 sec
t = 0.86 sec
t = 0.88 sec
t = 0.9 sec
t = 0.92 sec
t = 0.94 sec
0.4
0.2
0
0.5
1
α


C2-B20
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 0.9 sec
t = 0.92 sec
t = 0.94 sec
t = 0.96 sec
t = 0.98 sec
t = 1 sec
t = 1.02 sec
t = 1.04 sec
t = 1.06 sec
t = 1.08 sec
t = 1.1 sec
t = 1.12 sec
t = 1.14 sec
t = 1.16 sec
t = 1.18 sec
t = 1.2 sec
t = 1.22 sec
t = 1.24 sec
t = 1.26 sec
t = 1.28 sec
t = 1.3 sec
t = 1.32 sec
t = 1.34 sec
t = 1.36 sec
t = 1.38 sec
t = 1.4 sec
t = 1.42 sec
t = 1.44 sec
t = 1.46 sec
t = 1.48 sec
t = 1.5 sec
t = 1.52 sec
θ
α

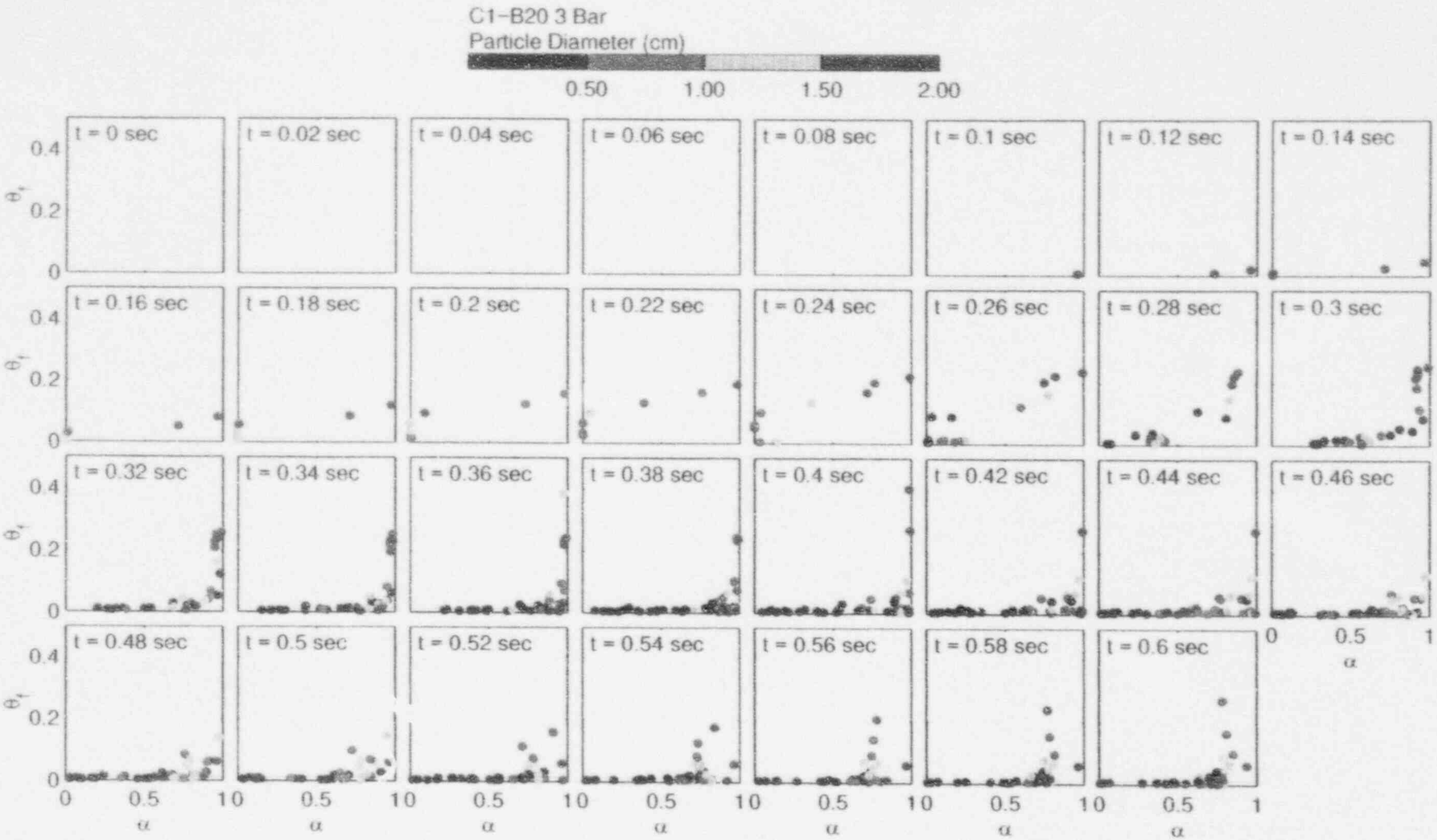

C2-B20
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 1.54 sec
t = 1.56 sec
t = 1.58 sec
t = 1.6 sec
0.4
0.2
0
0
0.5
1
α


C2-nb
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 0 sec
t = 0.02 sec
t = 0.04 sec
t = 0.06 sec
t = 0.08 sec
t = 0.1 sec
t = 0.12 sec
t = 0.14 sec
t = 0.16 sec
t = 0.18 sec
t = 0.2 sec
t = 0.22 sec
t = 0.24 sec
t = 0.26 sec
t = 0.28 sec
t = 0.3 sec
t = 0.32 sec
t = 0.34 sec
t = 0.36 sec
t = 0.38 sec
t = 0.4 sec
t = 0.42 sec
t = 0.44 sec
t = 0.46 sec
t = 0.48 sec
t = 0.5 sec
t = 0.52 sec
t = 0.54 sec
t = 0.56 sec
t = 0.58 sec
t = 0.6 sec
t = 0.62 sec
θ_f
0
0.2
0.4
0
0.5
1
α

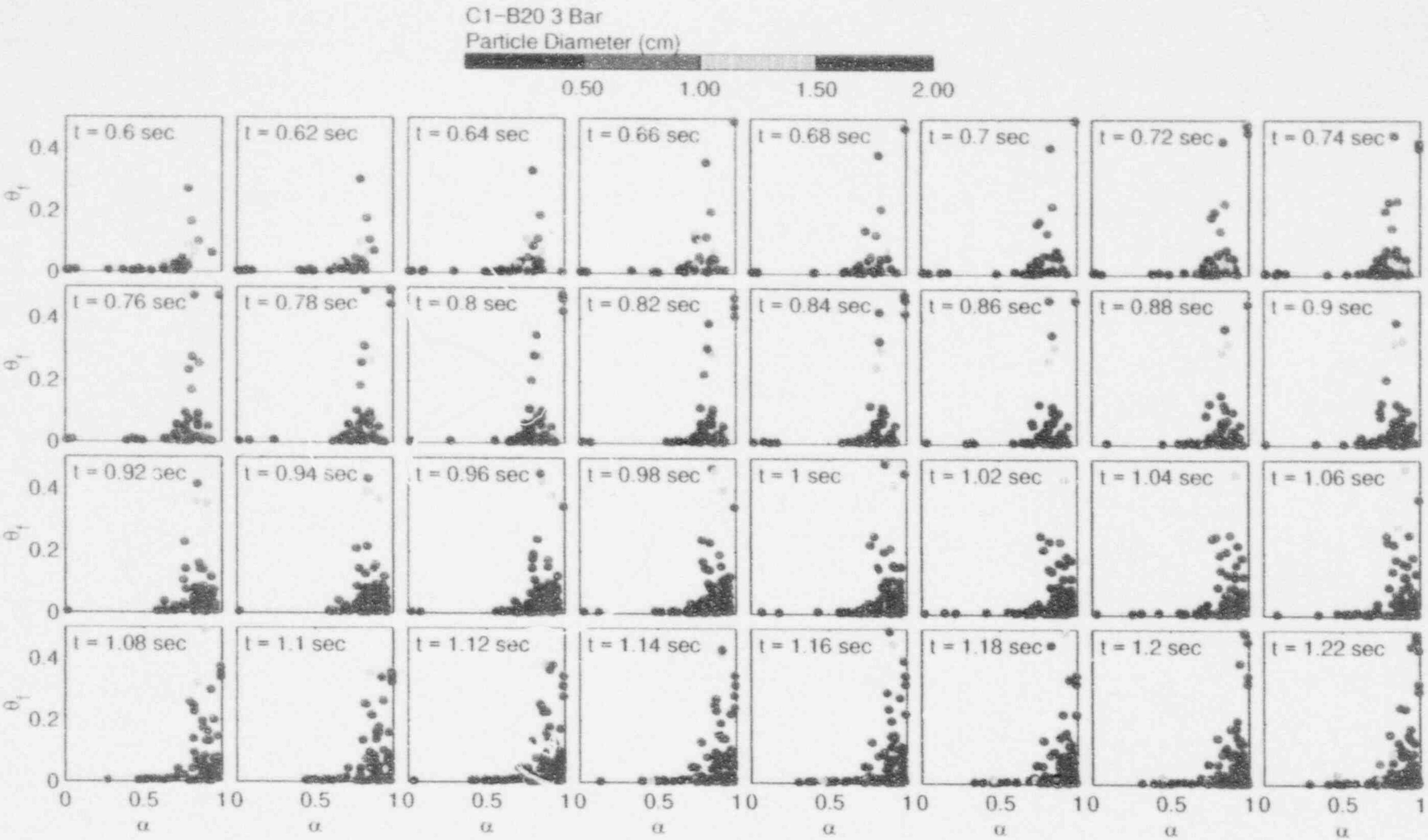

C2-nb
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 0.64 sec
t = 0.66 sec
t = 0.68 sec
t = 0.7 sec
t = 0.72 sec
t = 0.74 sec
t = 0.76 sec
t = 0.78 sec
t = 0.8 sec
t = 0.82 sec
t = 0.84 sec
t = 0.86 sec
t = 0.88 sec
t = 0.9 sec
t = 0.92 sec
t = 0.94 sec
t = 0.96 sec
t = 0.98 sec
t = 1 sec
t = 1.02 sec
t = 1.04 sec
t = 1.06 sec
t = 1.08 sec
t = 1.1 sec
t = 1.12 sec
t = 1.14 sec
t = 1.16 sec
t = 1.18 sec
t = 1.2 sec
t = 1.22 sec
t = 1.24 sec
t = 1.26 sec
0.4
0.2
0
θ_f



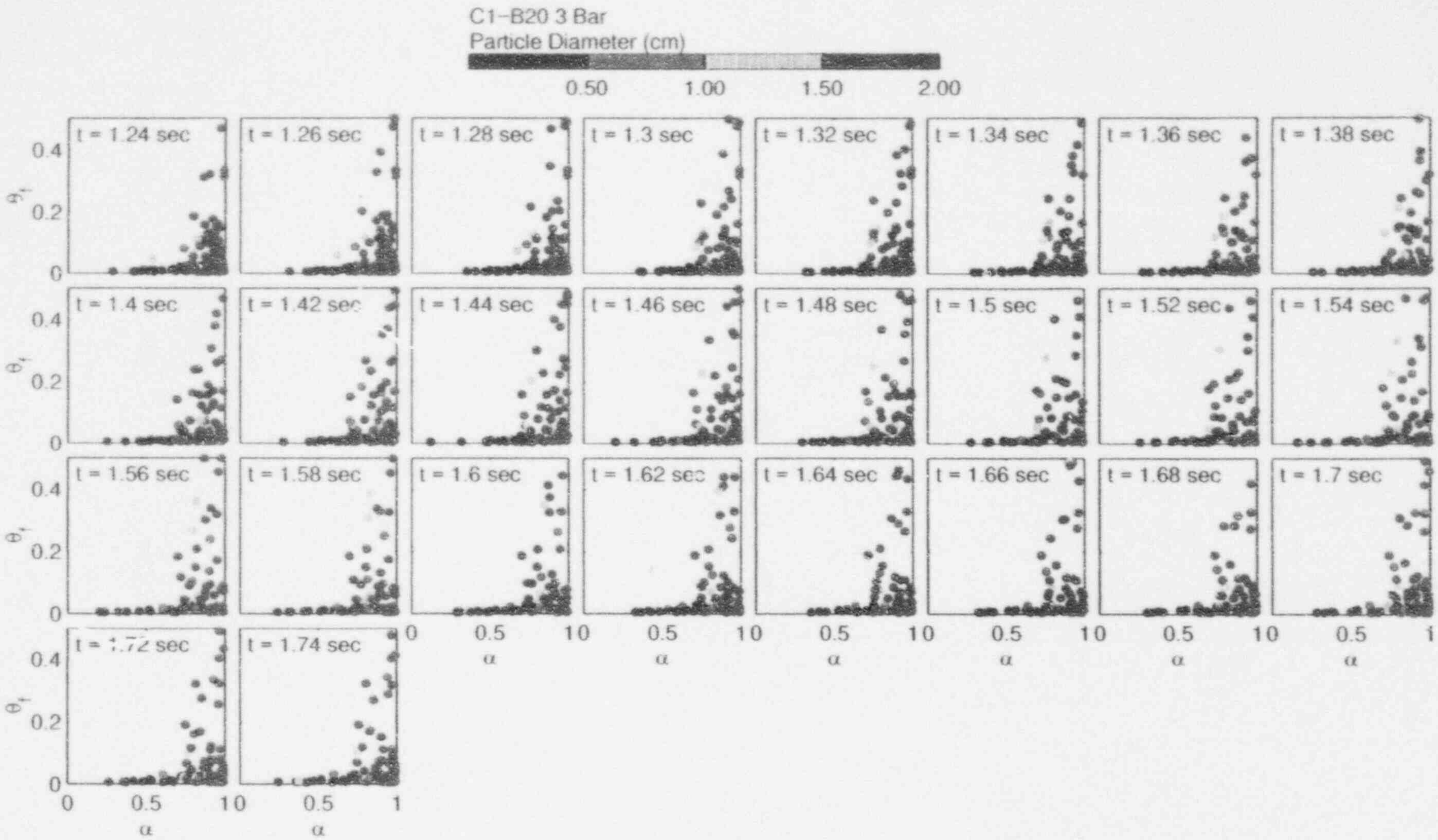
C2-nb
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 1.28 sec
t = 1.3 sec
t = 1.32 sec
t = 1.34 sec
t = 1.36 sec
t = 1.38 sec
t = 1.4 sec
t = 1.42 sec
t = 1.44 sec
t = 1.46 sec
t = 1.48 sec
t = 1.5 sec
t = 1.52 sec
t = 1.54 sec
t = 1.56 sec
t = 1.58 sec
θ_t
α

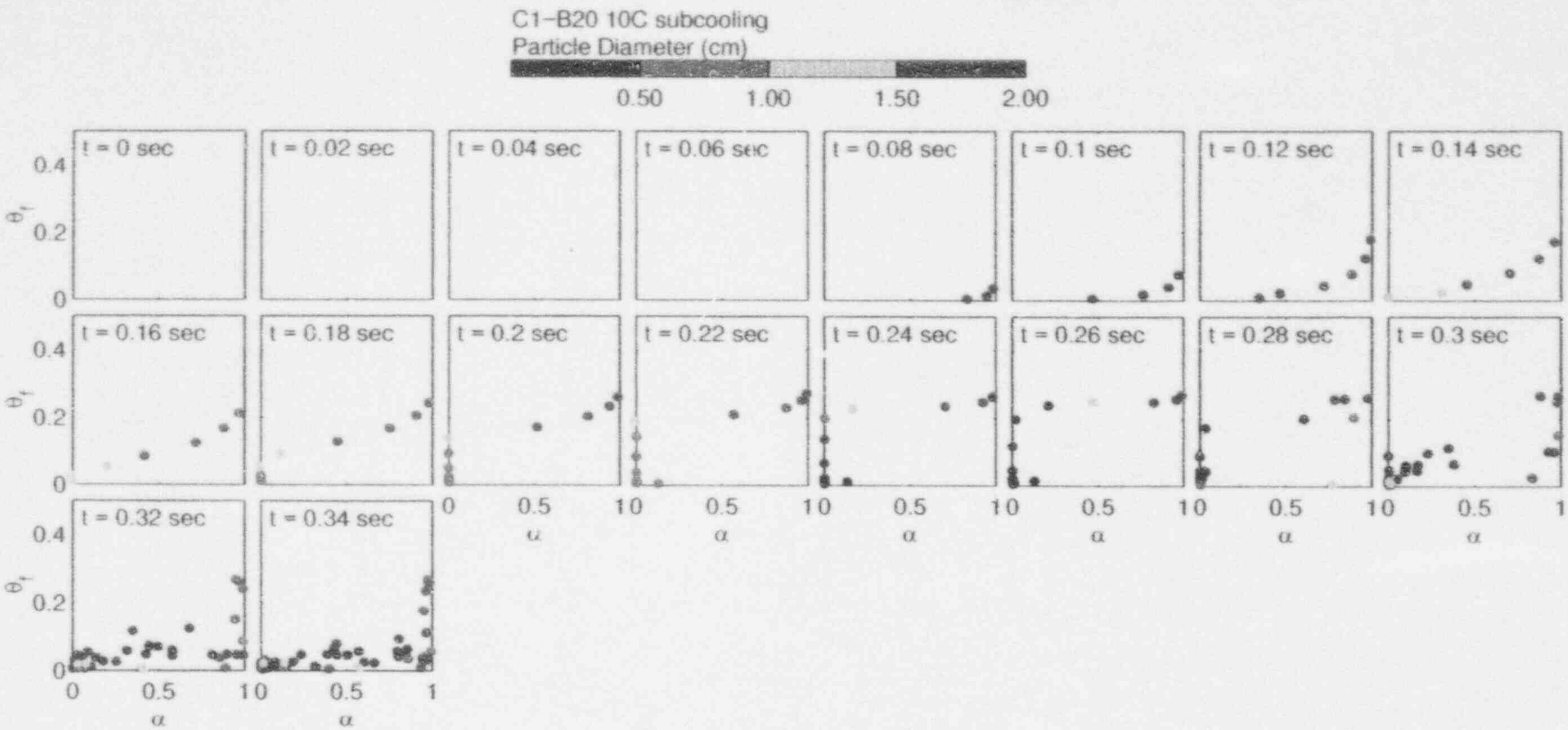


C1-B20 3 Bar
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 0 sec
t = 0.02 sec
t = 0.04 sec
t = 0.06 sec
t = 0.08 sec
t = 0.1 sec
t = 0.12 sec
t = 0.14 sec
t = 0.16 sec
t = 0.18 sec
t = 0.2 sec
t = 0.22 sec
t = 0.24 sec
t = 0.26 sec
t = 0.28 sec
t = 0.3 sec
t = 0.32 sec
t = 0.34 sec
t = 0.36 sec
t = 0.38 sec
t = 0.4 sec
t = 0.42 sec
t = 0.44 sec
t = 0.46 sec
t = 0.48 sec
t = 0.5 sec
t = 0.52 sec
t = 0.54 sec
t = 0.56 sec
t = 0.58 sec
t = 0.6 sec
0.4
0.2
0
0.5
1
θ
α

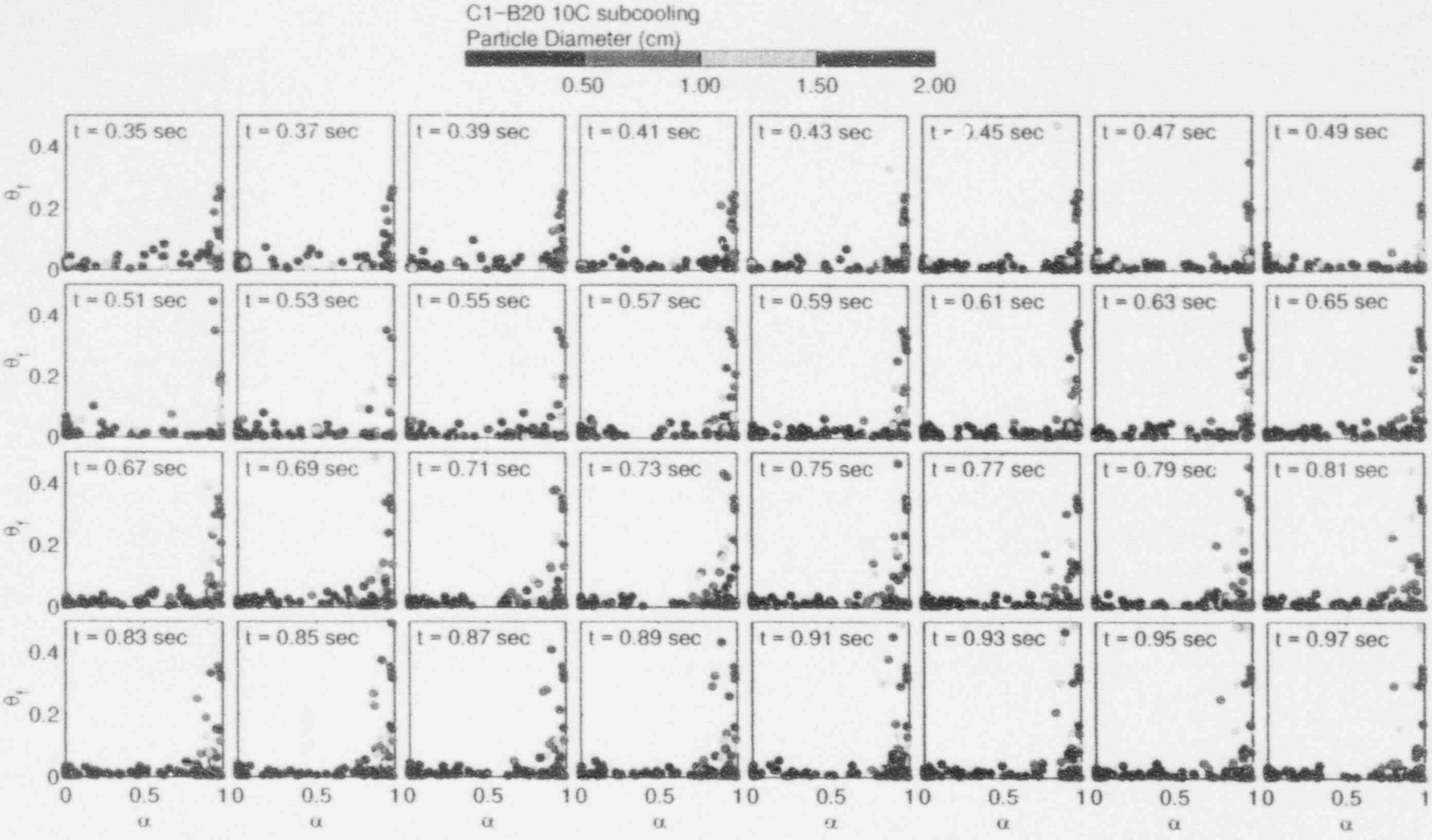


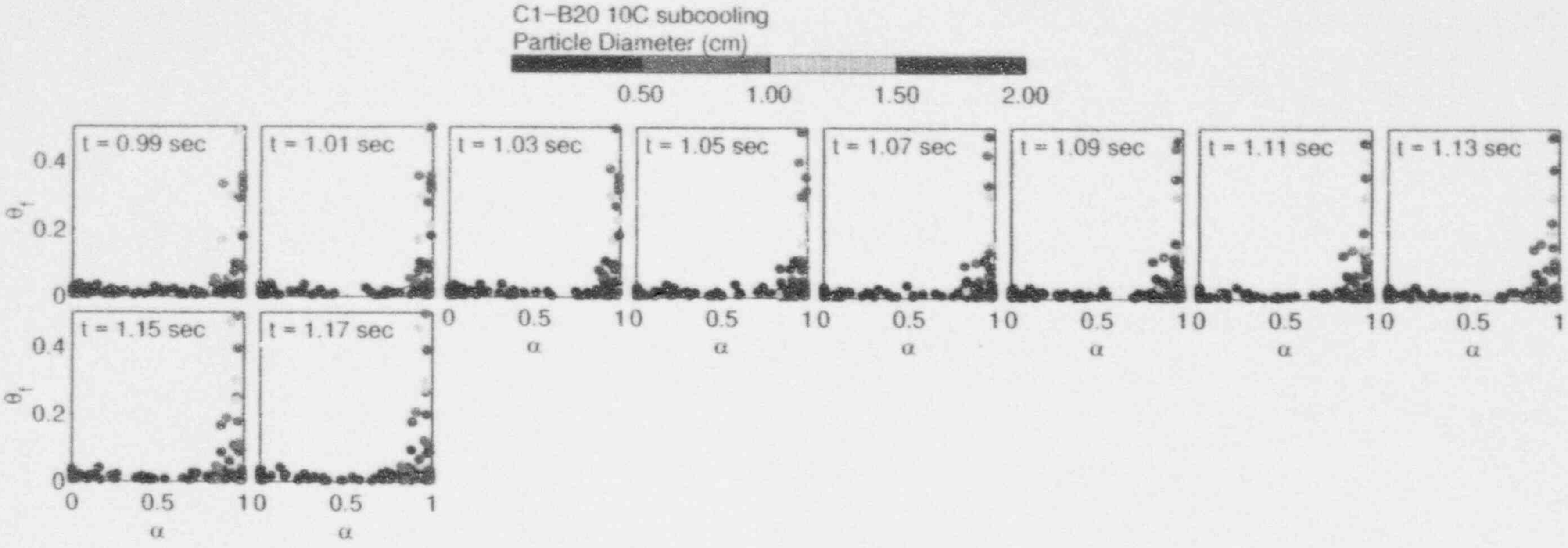
C1-B20 3 Bar
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 0.6 sec
t = 0.62 sec
t = 0.64 sec
t = 0.66 sec
t = 0.68 sec
t = 0.7 sec
t = 0.72 sec
t = 0.74 sec
t = 0.76 sec
t = 0.78 sec
t = 0.8 sec
t = 0.82 sec
t = 0.84 sec
t = 0.86 sec
t = 0.88 sec
t = 0.9 sec
t = 0.92 sec
t = 0.94 sec
t = 0.96 sec
t = 0.98 sec
t = 1 sec
t = 1.02 sec
t = 1.04 sec
t = 1.06 sec
t = 1.08 sec
t = 1.1 sec
t = 1.12 sec
t = 1.14 sec
t = 1.16 sec
t = 1.18 sec
t = 1.2 sec
t = 1.22 sec
θ_t
0.4
0.2
0
0.5
1
α


C1-B20 3 Bar
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 1.24 sec
t = 1.26 sec
t = 1.28 sec
t = 1.3 sec
t = 1.32 sec
t = 1.34 sec
t = 1.36 sec
t = 1.38 sec
t = 1.4 sec
t = 1.42 sec
t = 1.44 sec
t = 1.46 sec
t = 1.48 sec
t = 1.5 sec
t = 1.52 sec
t = 1.54 sec
t = 1.56 sec
t = 1.58 sec
t = 1.6 sec
t = 1.62 sec
t = 1.64 sec
t = 1.66 sec
t = 1.68 sec
t = 1.7 sec
t = 1.72 sec
t = 1.74 sec
θ_t
0
0.2
0.4
0
0.5
1
α



C1-B20 10C subcooling
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 0 sec
t = 0.02 sec
t = 0.04 sec
t = 0.06 sec
t = 0.08 sec
t = 0.1 sec
t = 0.12 sec
t = 0.14 sec
t = 0.16 sec
t = 0.18 sec
t = 0.2 sec
t = 0.22 sec
t = 0.24 sec
t = 0.26 sec
t = 0.28 sec
t = 0.3 sec
t = 0.32 sec
t = 0.34 sec
θ
α


C1-B20 10C subcooling
Particle Diameter (cm)
0.50 1.00 1.50 2.00
t = 0.35 sec
t = 0.37 sec
t = 0.39 sec
t = 0.41 sec
t = 0.43 sec
t = 0.45 sec
t = 0.47 sec
t = 0.49 sec
t = 0.51 sec
t = 0.53 sec
t = 0.55 sec
t = 0.57 sec
t = 0.59 sec
t = 0.61 sec
t = 0.63 sec
t = 0.65 sec
t = 0.67 sec
t = 0.69 sec
t = 0.71 sec
t = 0.73 sec
t = 0.75 sec
t = 0.77 sec
t = 0.79 sec
t = 0.81 sec
t = 0.83 sec
t = 0.85 sec
t = 0.87 sec
t = 0.89 sec
t = 0.91 sec
t = 0.93 sec
t = 0.95 sec
t = 0.97 sec
θ_f
0 0.2 0.4
α
0 0.5 1


C1-B20 10C subcooling
Particle Diameter (cm)
0.50
1.00
1.50
2.00
t = 0.99 sec
t = 1.01 sec
t = 1.03 sec
t = 1.05 sec
t = 1.07 sec
t = 1.09 sec
t = 1.11 sec
t = 1.13 sec
t = 1.15 sec
t = 1.17 sec
θ_t
α

[[ADDENDUM TO CHAPTER 6]]

The purpose of this addendum it to:

1. Extend previous ESPROSE.m runs to longer times;

2. Provide new results with triggering at different times, trying to explore and define better the "sensitive" premixutes; and

3. Provide new results using finer grids; that is, continuing the RC1 run into a triggered explosion on the same, finer, grid.

All runs made and the main results are summarized in Table 6.a1. The RC2 run in this table was triggered with a doubledup (azimuthally) RC1 premixture. Also, it should be noted that while the RC1 premixing run was made on a Cartesian (2D) grid, both the RC1 and RC2 explosion runs were made in 3D (but with the same, small, grid size).

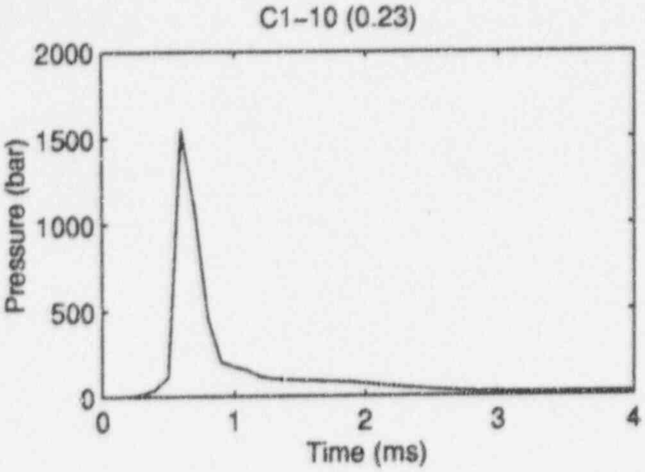
Table 6.a1 Summary of the ESPROSE.m-3D Explosion Runs
The entries show the peak local impulse obtained,
and the number in parenthesis is the trigger time.

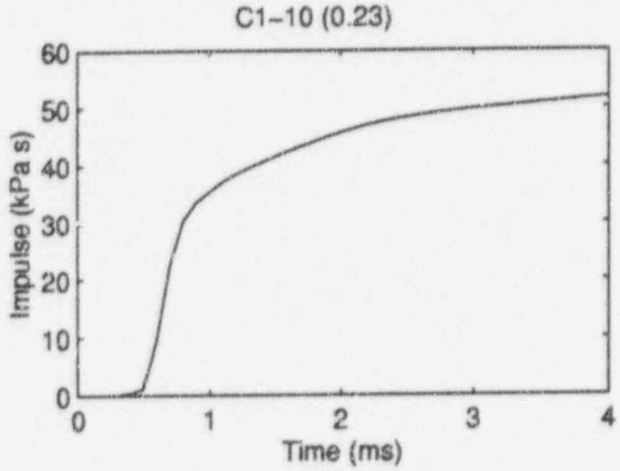
β	C1	RC1	C2	RC2
10	50 (0.23) 50 (0.24) 17 (0.25) 14 (0.30) 14 (0.35)		100 (0.25) 32 (0.30) 13 (0.35)	
20	100 (0.30) 11 (0.35) 11 (0.40)	110 (0.30)	100 (0.25) 120 (0.30) 14 (0.35)	140 (0.30)
nb	30 (0.25) 45 (0.35) 17 (0.45) 26 (1.45)		40 (0.25) 52 (0.35) 25 (0.45) 20 (1.40)	

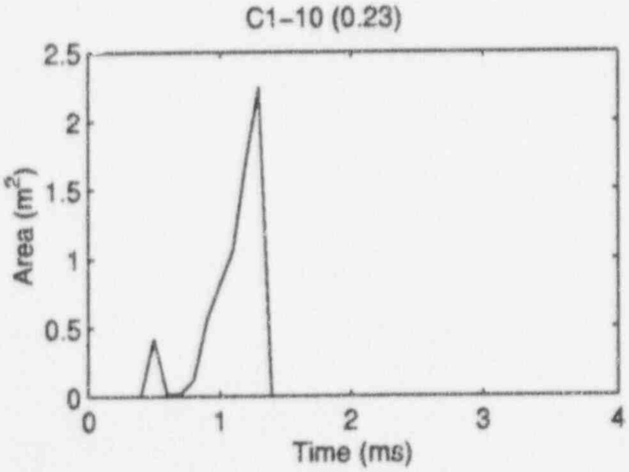
As noted on the table, the trigger times are referenced to the time of fuel arrival to 1 m above the water pool. These are offset relative to the trigger times in Table 6.1 by 0.18 s. In particular, the 200 kPa·s run in Table 6.1 C2–20 (0.120), corresponds to the 120 kPa·s run, C2–20 (0.30) in Table 6.a1.

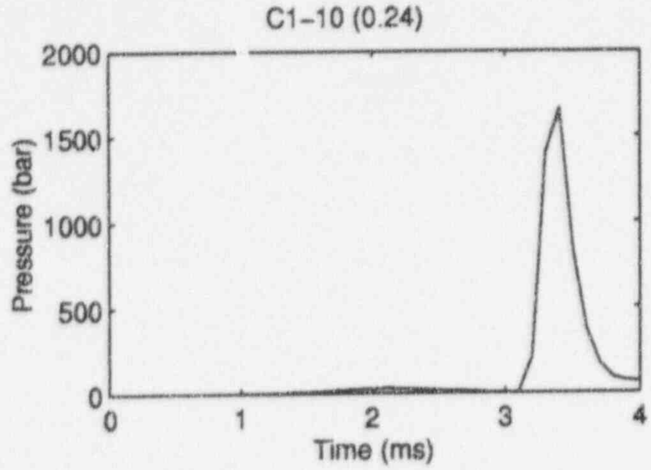
We see that a number of premixtures yield impulses in the 100 kPA·s range, but this occurs over short premixing intervals, while outside those intervals the impulses are in the 10s of kPa·s. Also, we find very good consistency with the finer grid runs. These results are confirmatory of those presented earlier, they elucidate more completely that it is unlikely that a more sensitive premixture has been missed, and support the previous conclusion that lower head failure is physically unreasonable.

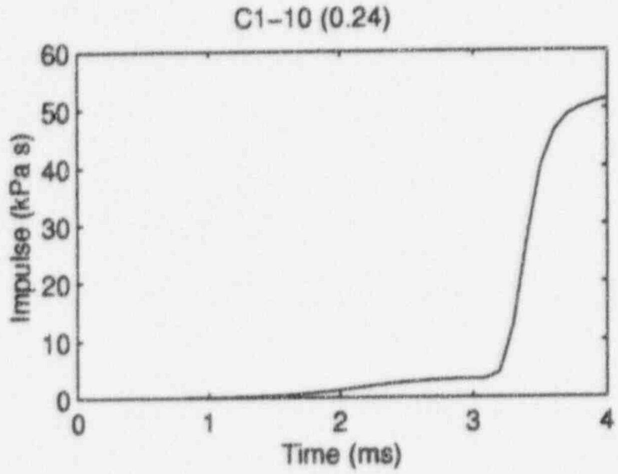
The figures that follow show the detailed new results in the same format as before (like Figures 6.5).

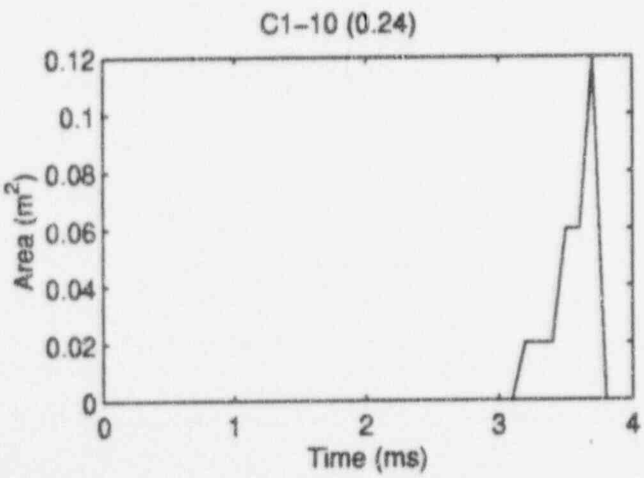

C1-10 (0.23)
Pressure (bar)
2000
1500
1000
500
0
0
1
2
3
4
Time (ms)

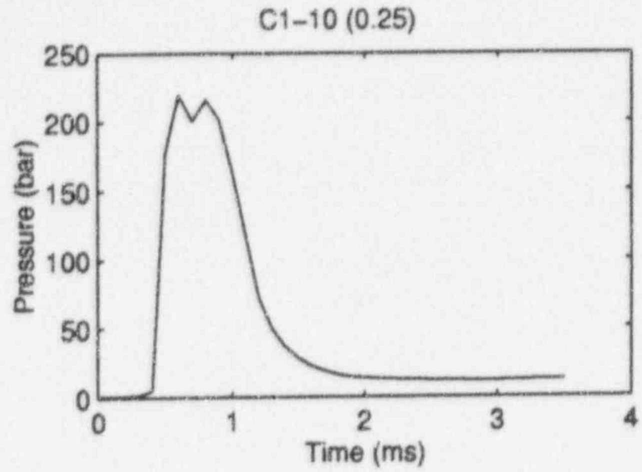

C1-10 (0.23)
Impulse (kPa s)
60
50
40
30
20
10
0
0
1
2
3
4
Time (ms)

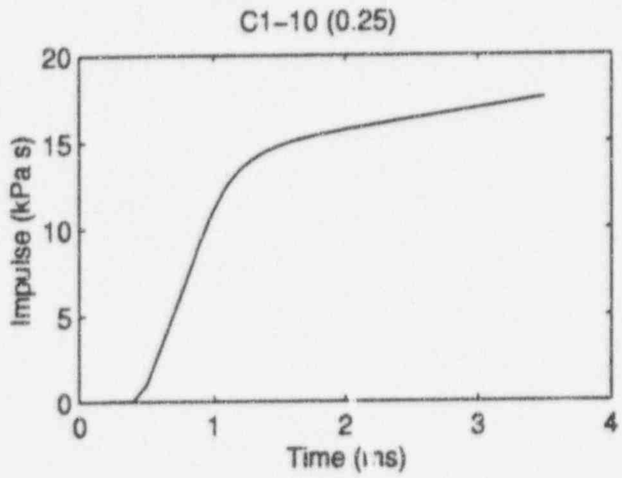

C1-10 (0.23)
Area (m²)
2.5
2
1.5
1
0.5
0
0
1
2
3
4
Time (ms)

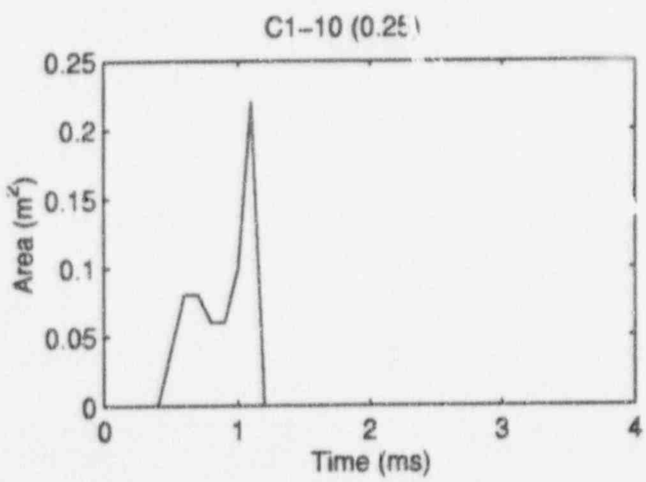

C1-10 (0.24)
Pressure (bar)
Time (ms)

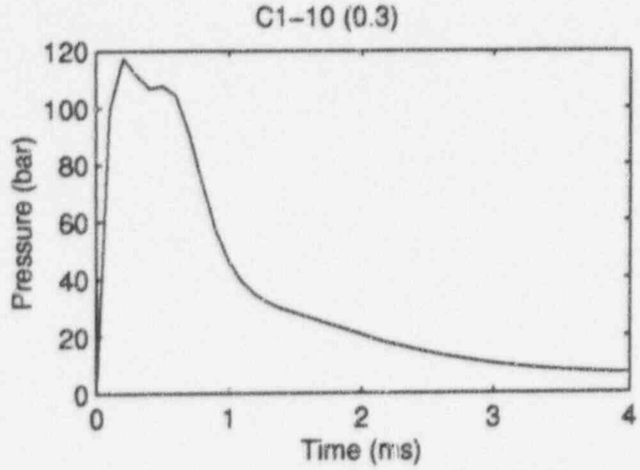

C1-10 (0.24)
Impulse (kPa s)
Time (ms)

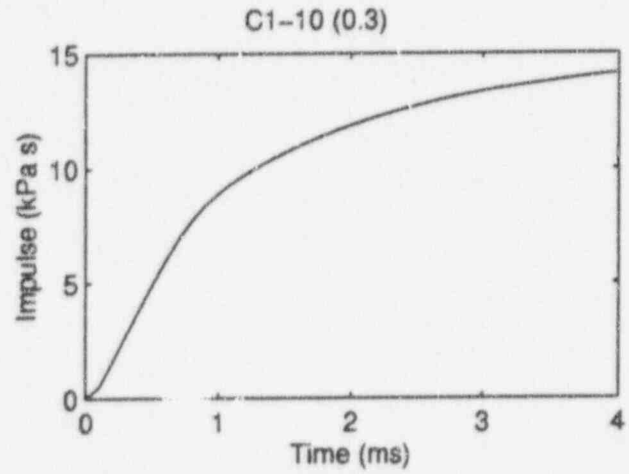

C1-10 (0.24)
Area (m²)
Time (ms)

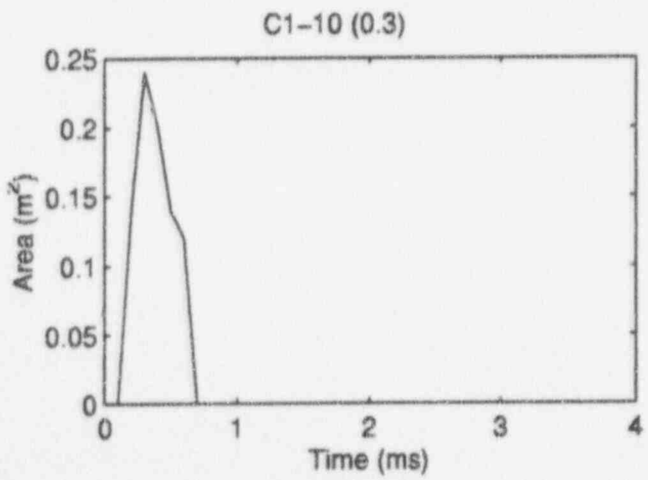

C1-10 (0.25)
Pressure (bar)
Time (ms)

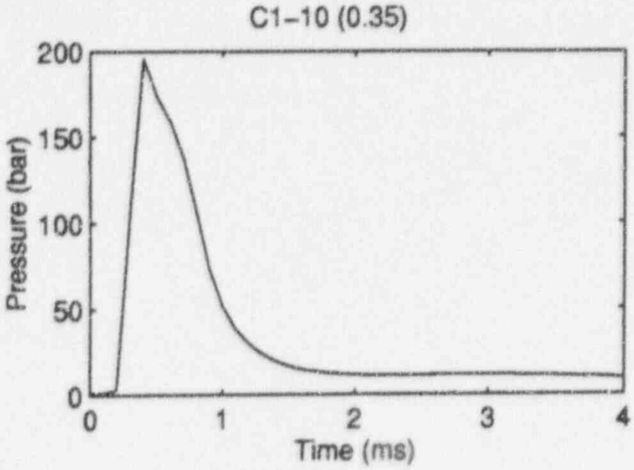

C1-10 (0.25)
Impulse (kPa s)
Time (ms)


C1-10 (0.25)
Area (m²)
Time (ms)

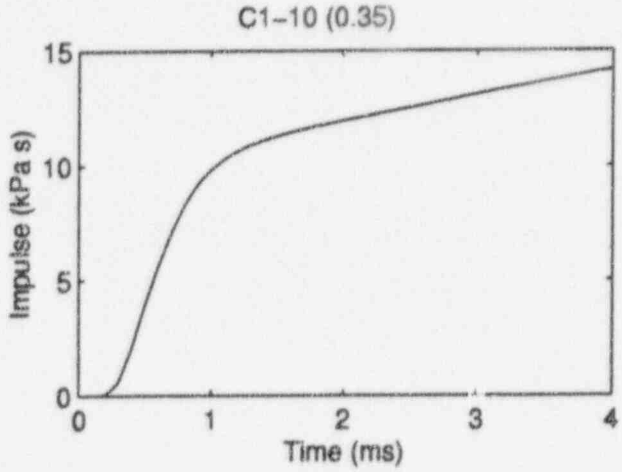

C1-10 (0.3)
Pressure (bar)
Time (ms)


C1-10 (0.3)
Impulse (kPa s)
Time (ms)

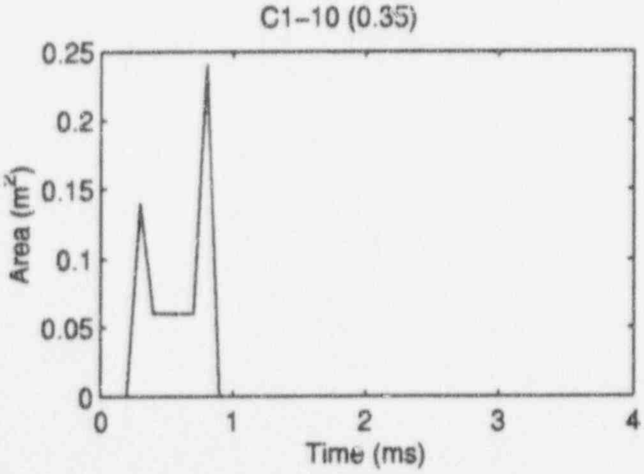

C1-10 (0.3)
Area (m²)
Time (ms)



C1-10 (0.35)
200
150
100
50
0
Pressure (bar)
0
1
2
3
4
Time (ms)

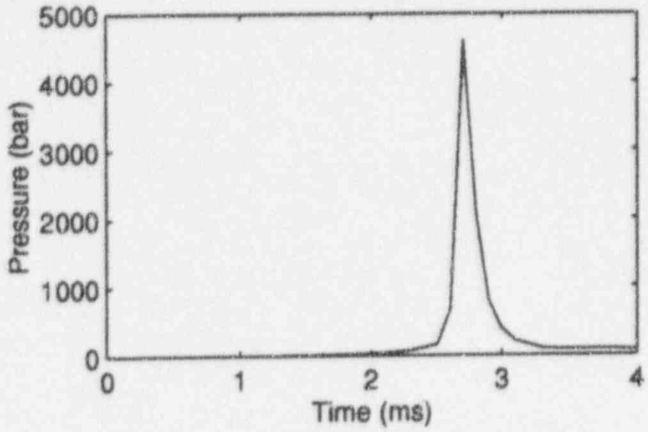


C1-10 (0.35)
15
10
5
0
Impulse (kPa s)
0
1
2
3
4
Time (ms)

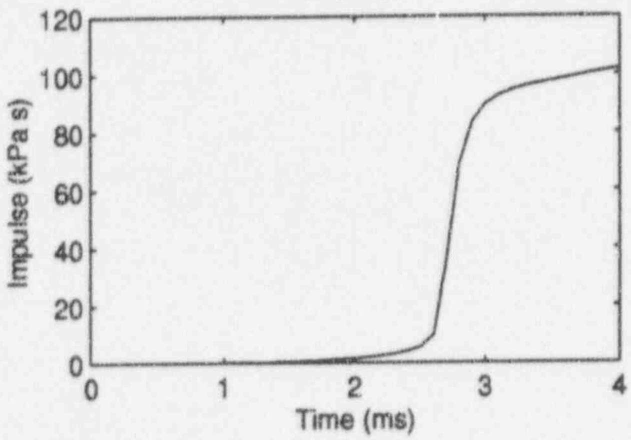


C1-10 (0.35)
0.25
0.2
0.15
0.1
0.05
0
Area (m²)
0
1
2
3
4
Time (ms)

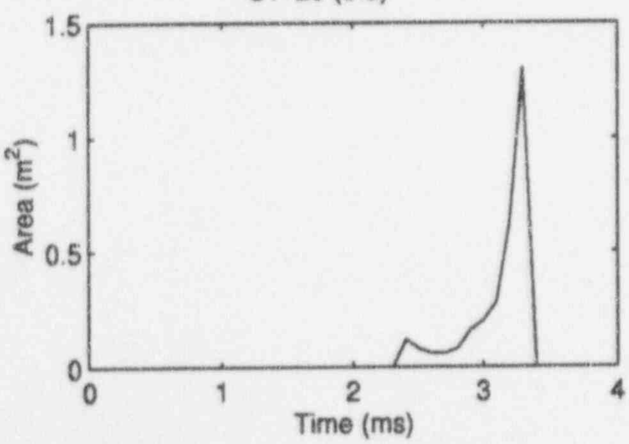
C1-20 (0.3)

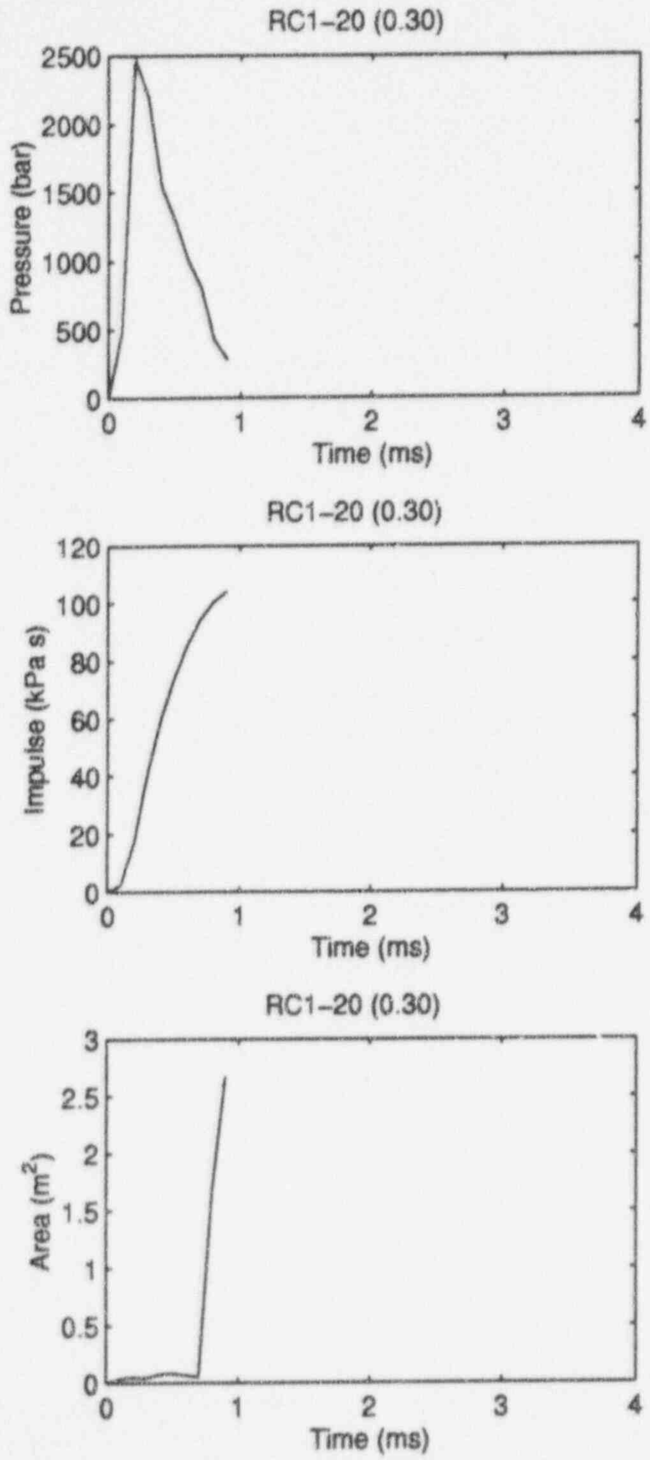


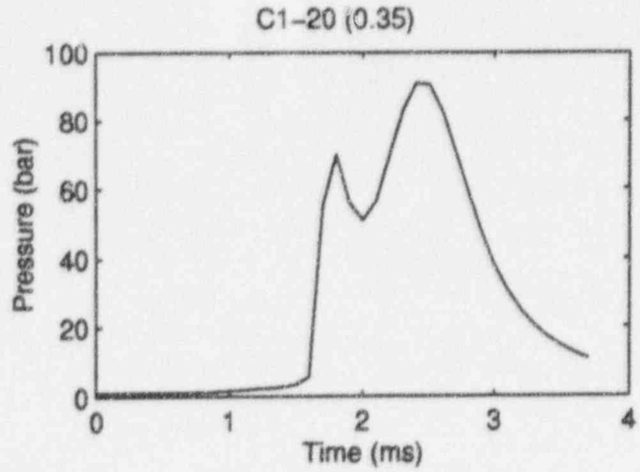
C1-20 (0.3)

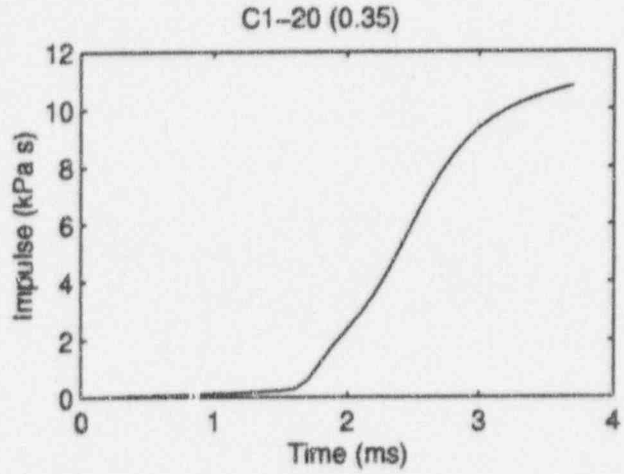


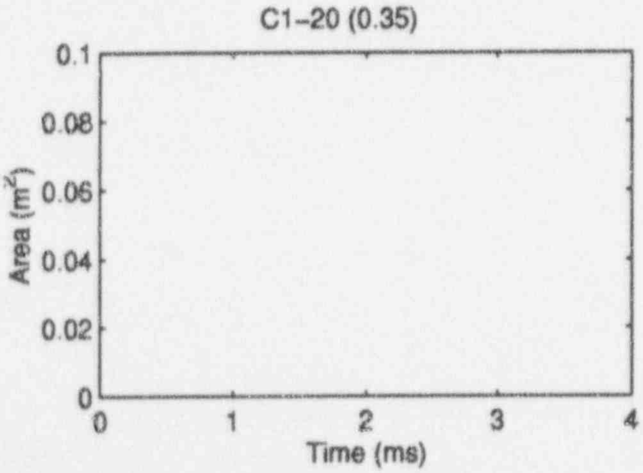
C1-20 (0.3)

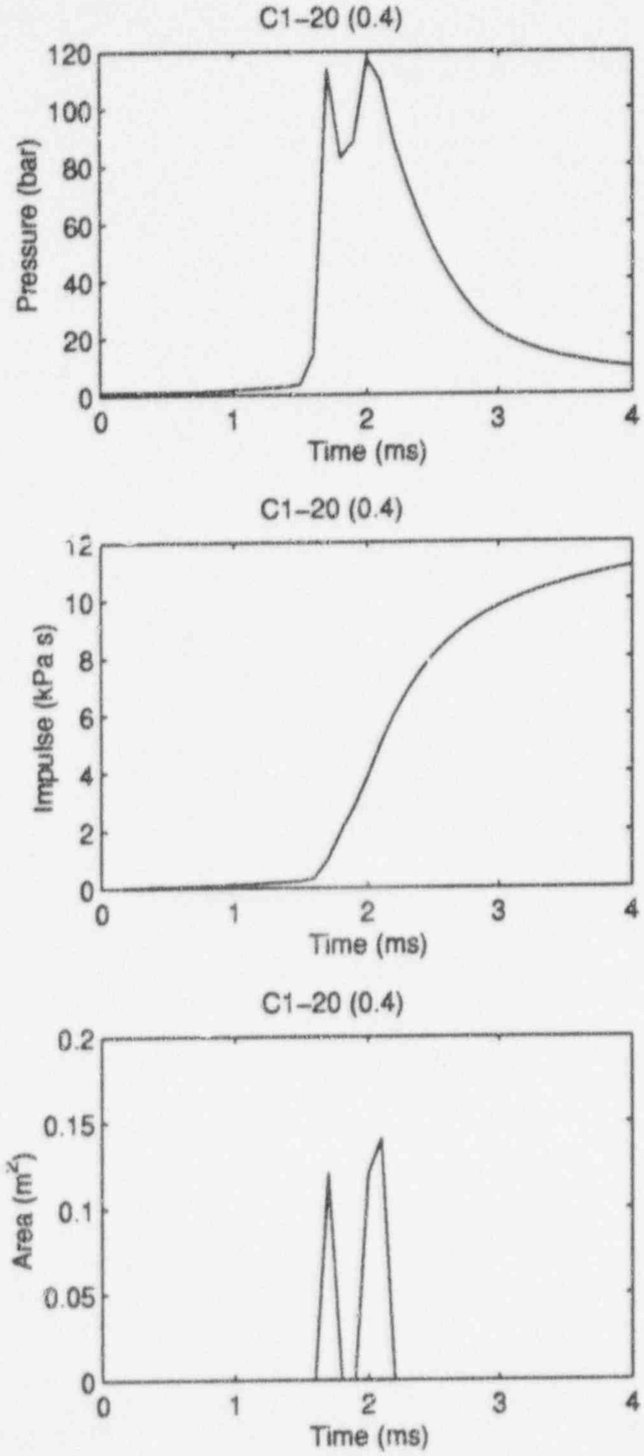


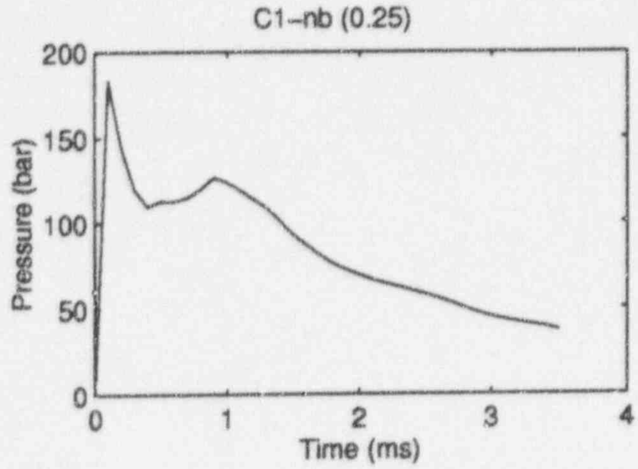

RC1-20 (0.30)
Pressure (bar)
Time (ms)
RC1-20 (0.30)
Impulse (kPa s)
Time (ms)
RC1-20 (0.30)
Area (m^2)
Time (ms)

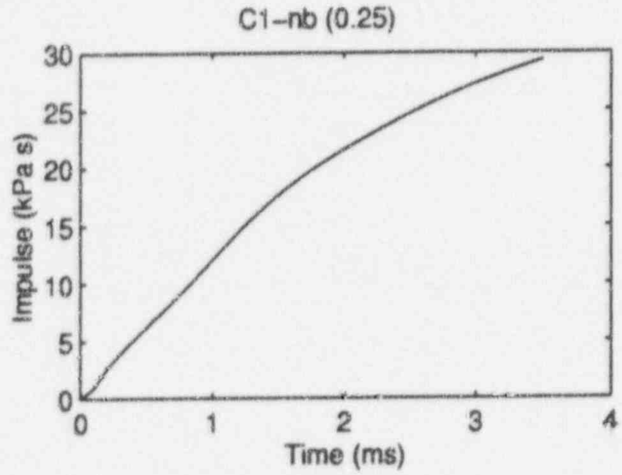

C1-20 (0.35)
Pressure (bar)
Time (ms)

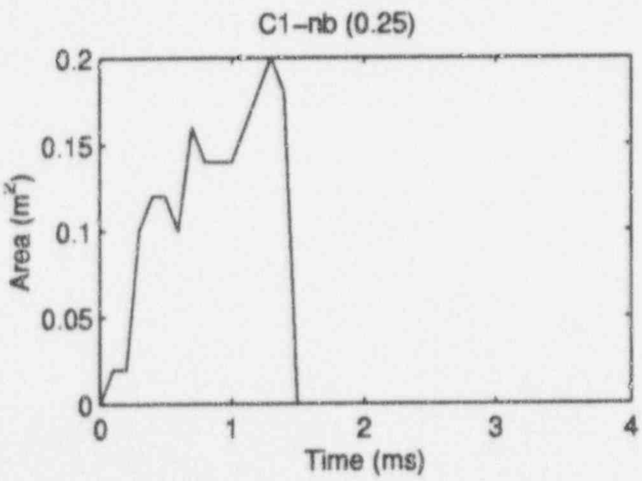

C1-20 (0.35)
Impulse (kPa s)
Time (ms)

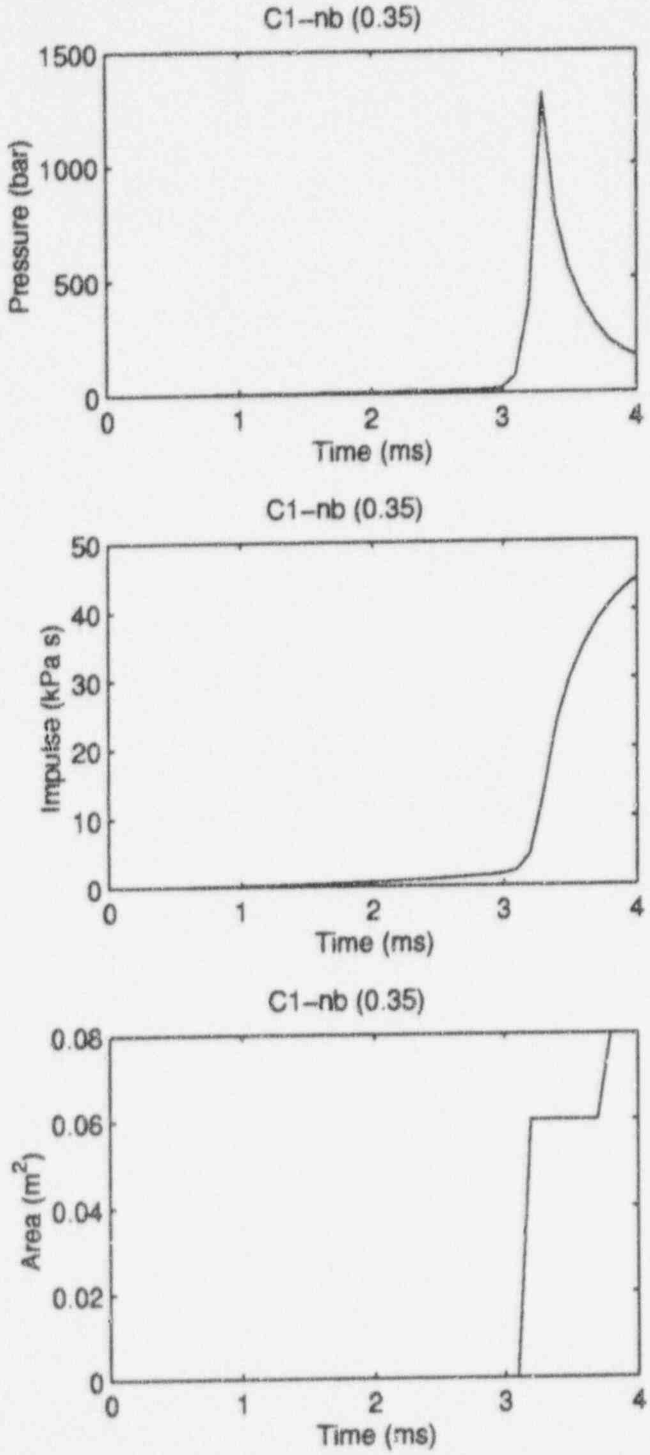

C1-20 (0.35)
Area (m^2)
Time (ms)

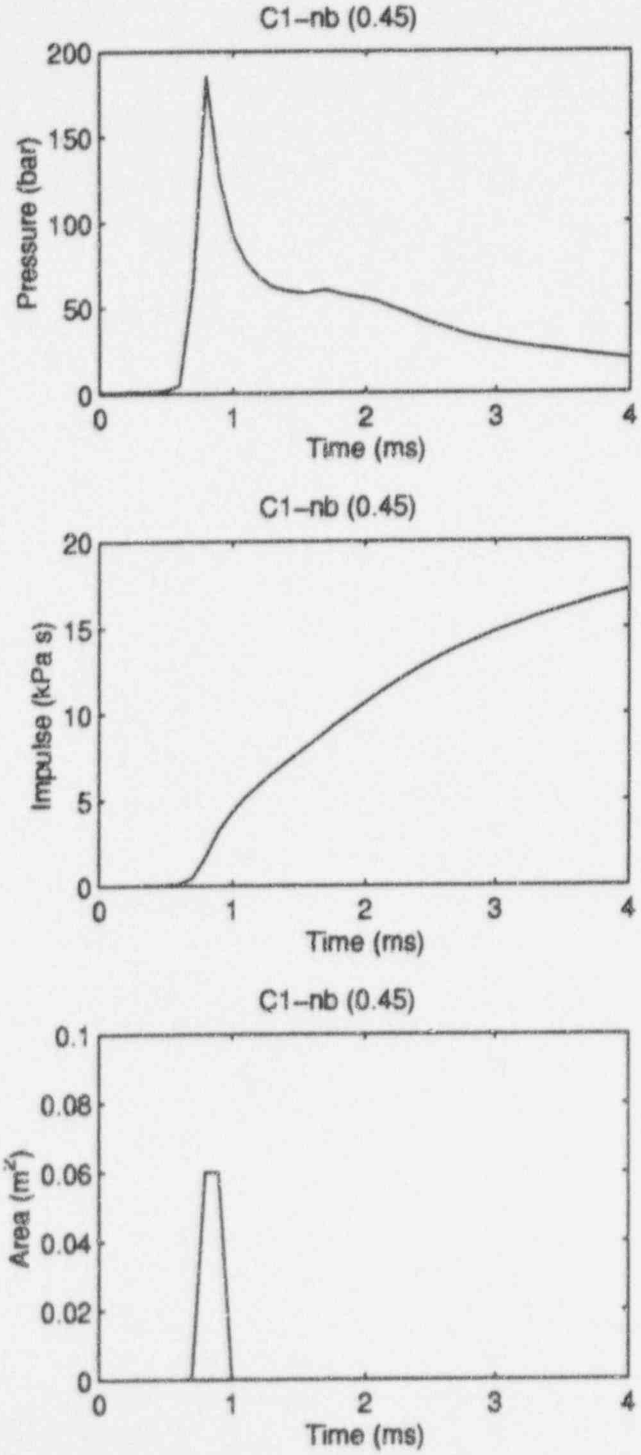

C1-20 (0.4)
Pressure (bar)
Time (ms)
C1-20 (0.4)
Impulse (kPa s)
Time (ms)
C1-20 (0.4)
Area (m²)
Time (ms)

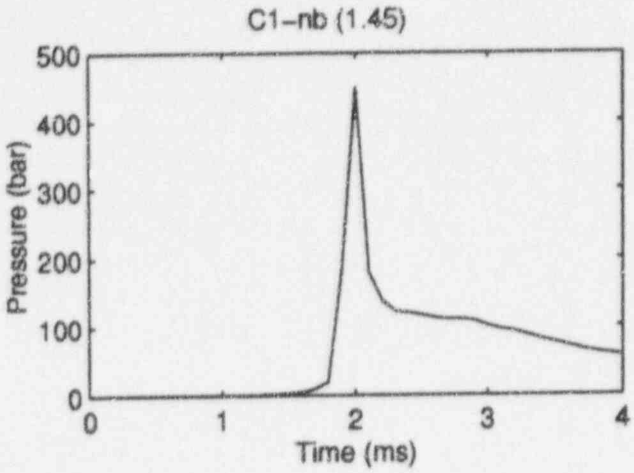

C1–nb (0.25)
Pressure (bar)
Time (ms)

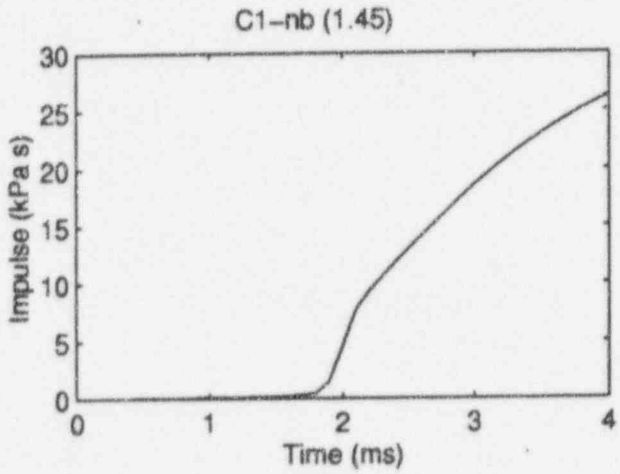

C1–nb (0.25)
Impulse (kPa s)
Time (ms)

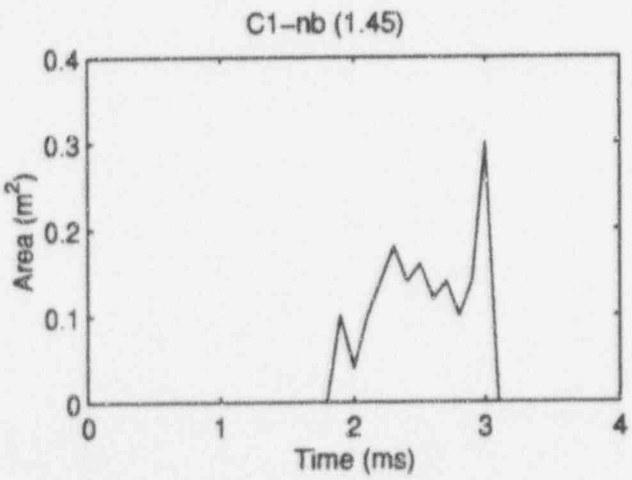

C1–nb (0.25)
Area (m^2)
Time (ms)

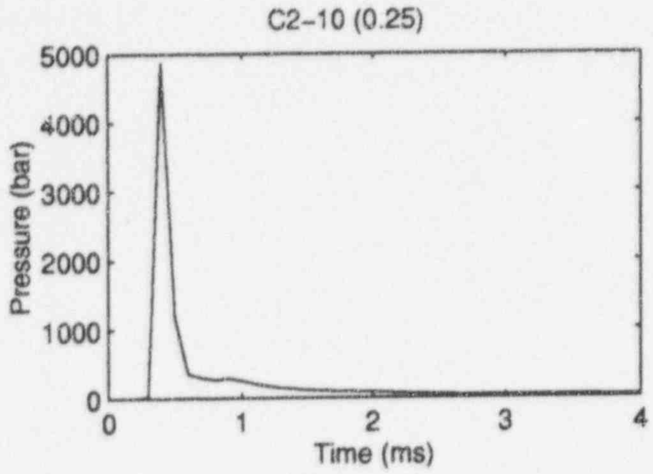

C1–nb (0.35)
Pressure (bar)
Time (ms)
C1–nb (0.35)
Impulse (kPa s)
Time (ms)
C1–nb (0.35)
Area (m2)
Time (ms)

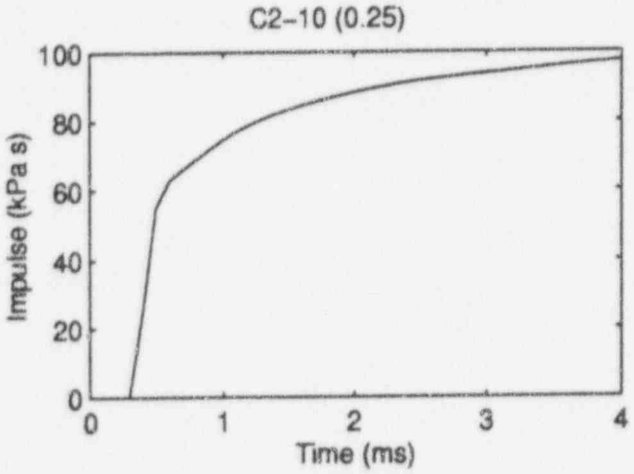

C1–nb (0.45)
Pressure (bar)
Time (ms)
C1–nb (0.45)
Impulse (kPa s)
Time (ms)
C1–nb (0.45)
Area (m²)
Time (ms)

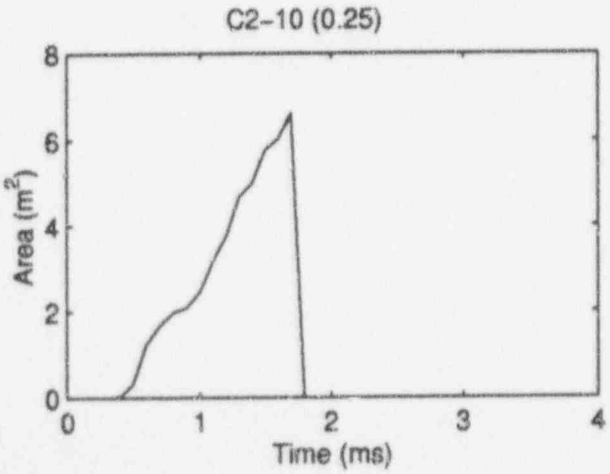

C1-nb (1.45)
Pressure (bar)
Time (ms)
0
100
200
300
400
500
0
1
2
3
4

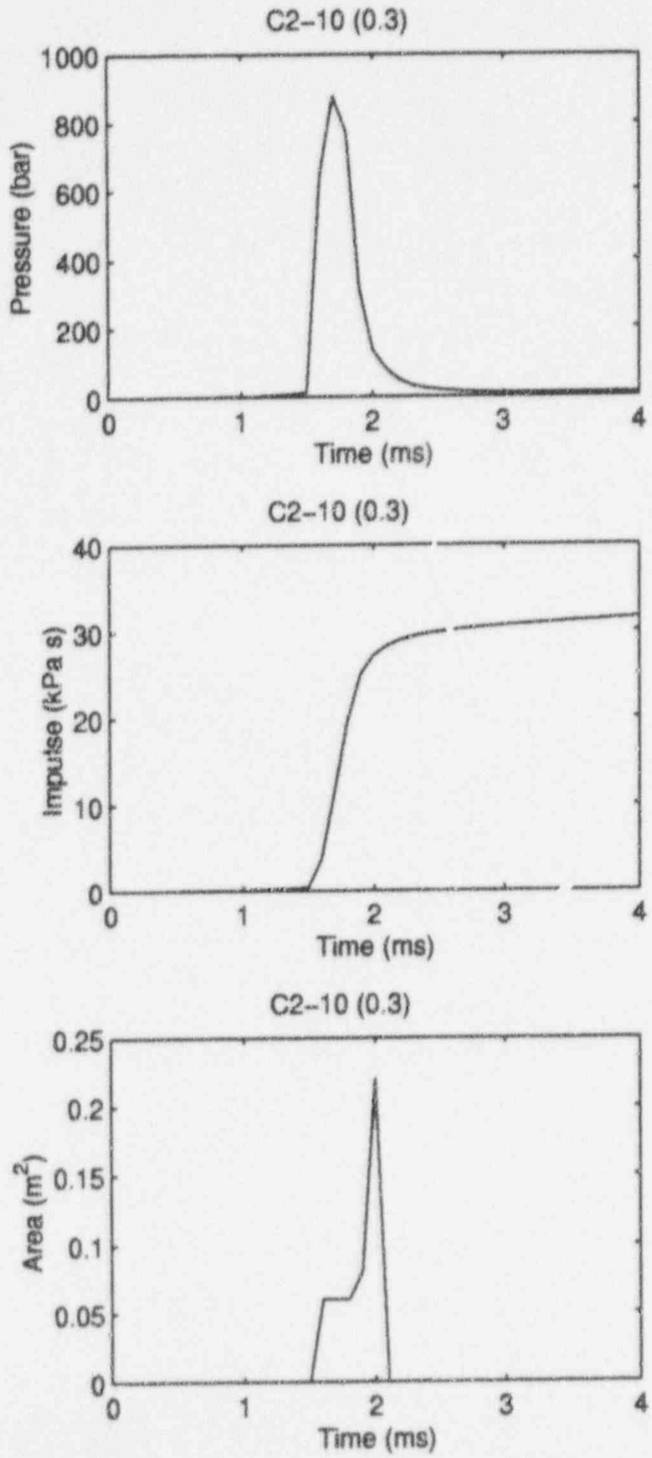

C1-nb (1.45)
Impulse (kPa s)
Time (ms)
0
5
10
15
20
25
30
0
1
2
3
4

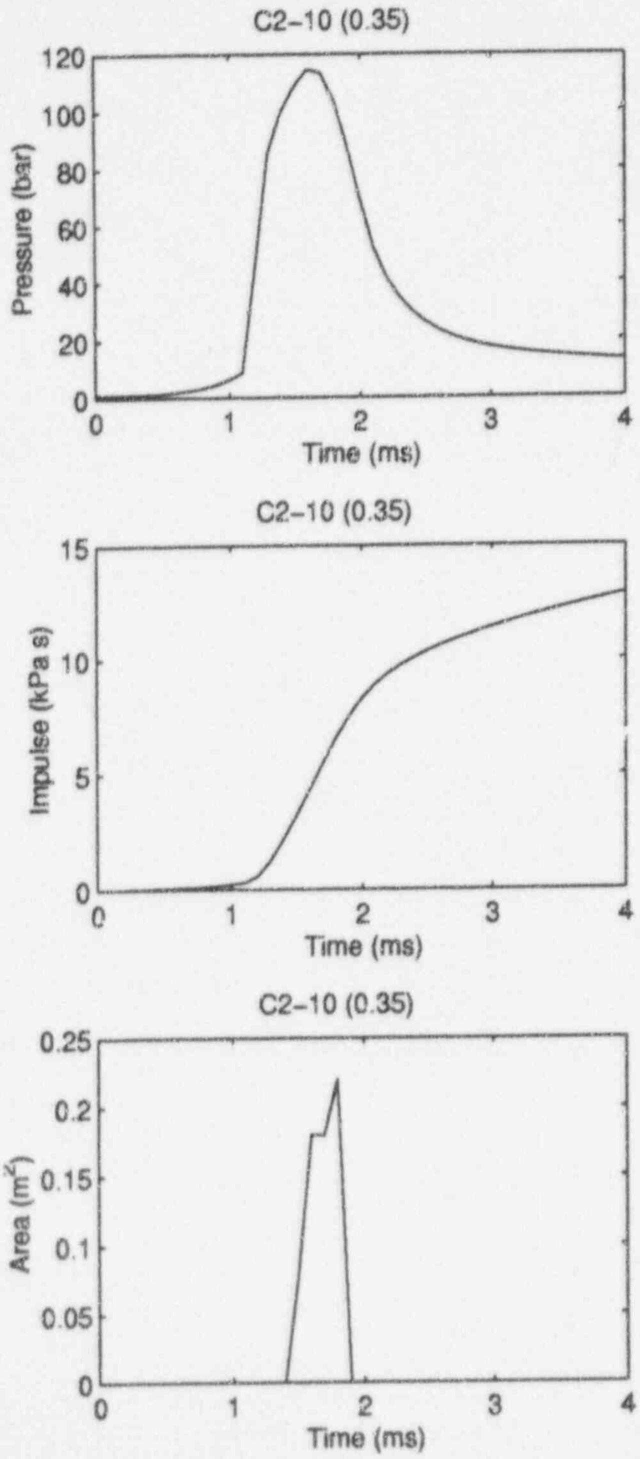

C1-nb (1.45)
Area (m^2)
Time (ms)
0
0.1
0.2
0.3
0.4
0
1
2
3
4

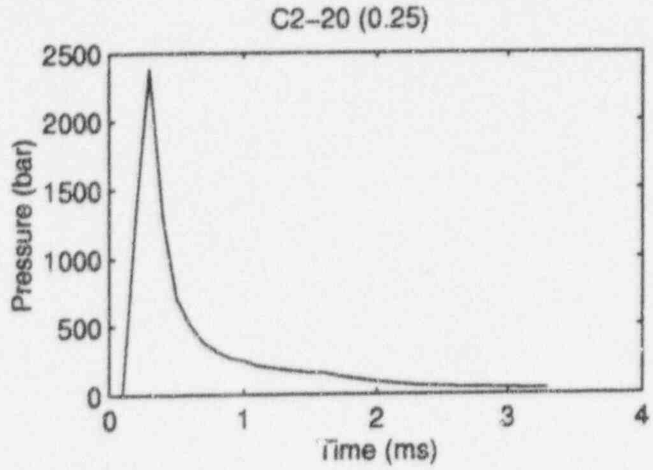

C2-10 (0.25)
Pressure (bar)
Time (ms)

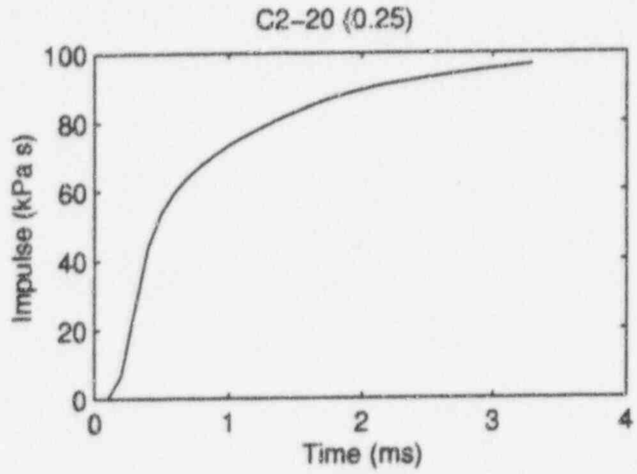

C2-10 (0.25)
Impulse (kPa s)
Time (ms)

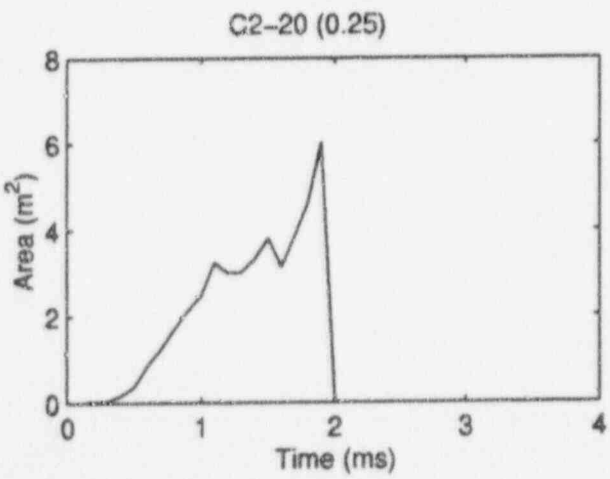

C2-10 (0.25)
Area (m2)
Time (ms)

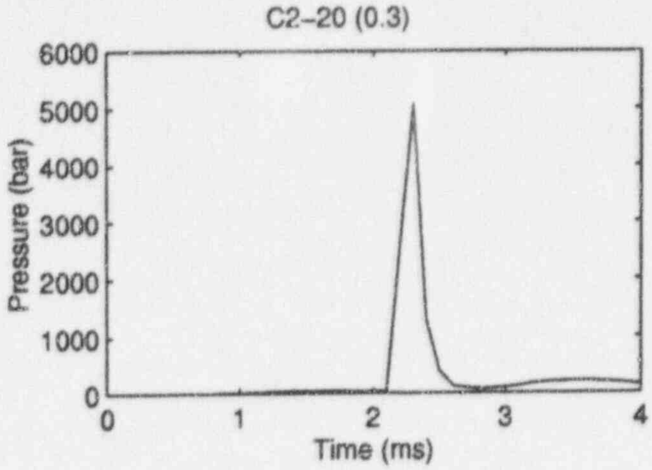

C2-10 (0.3)
Pressure (bar)
Time (ms)
C2-10 (0.3)
Impulse (kPa s)
Time (ms)
C2-10 (0.3)
Area (m^2)
Time (ms)

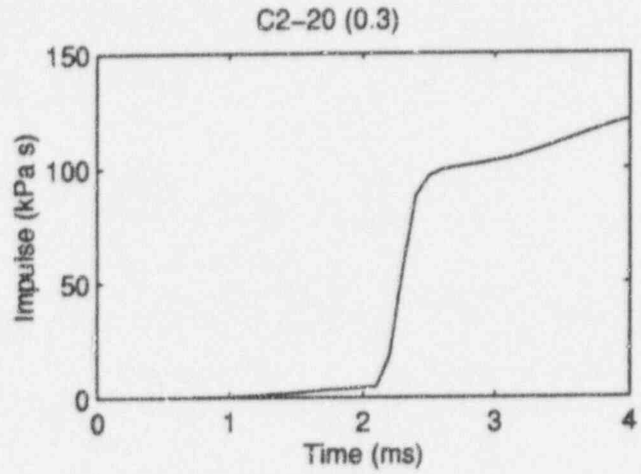

C2-10 (0.35)
Pressure (bar)
Time (ms)
C2-10 (0.35)
Impulse (kPa s)
Time (ms)
C2-10 (0.35)
Area (m²)
Time (ms)

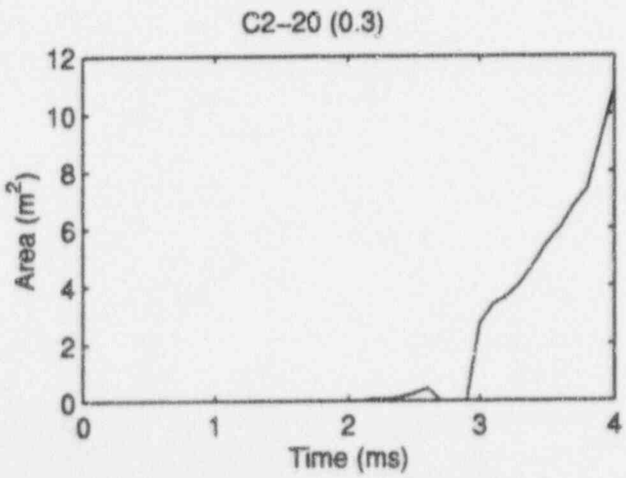

C2-20 (0.25)
Pressure (bar)
Time (ms)

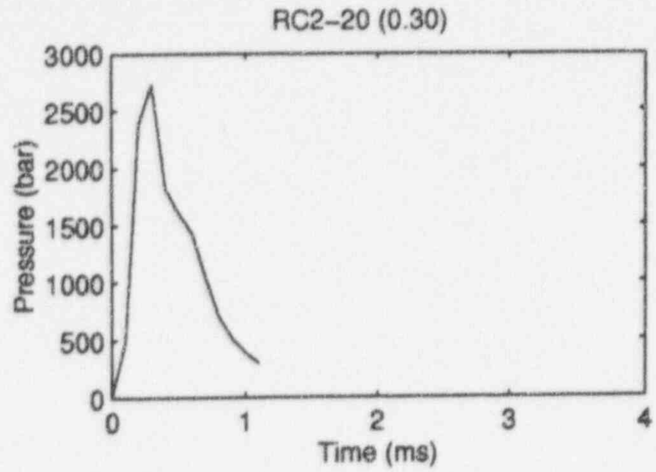

C2-20 (0.25)
Impulse (kPa s)
Time (ms)

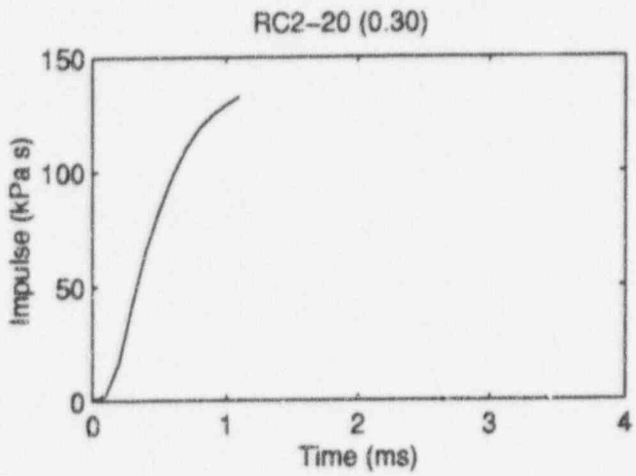

C2-20 (0.25)
Area (m2)
Time (ms)

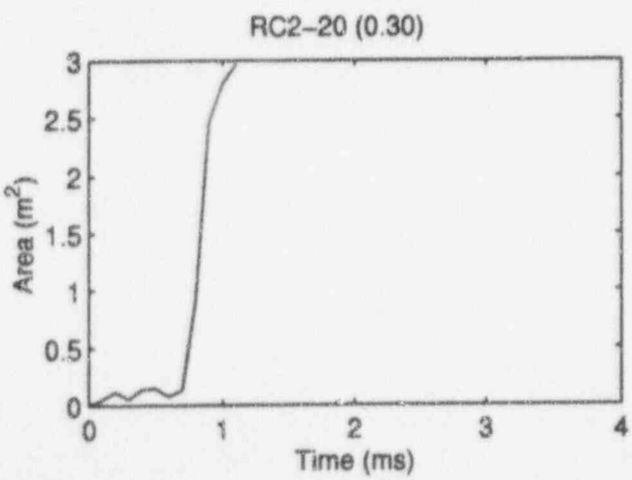

C2-20 (0.3)
Pressure (bar)
6000
5000
4000
3000
2000
1000
0
0
1
2
3
4
Time (ms)


C2-20 (0.3)
Impulse (kPa s)
150
100
50
0
0
1
2
3
4
Time (ms)


C2-20 (0.3)
Area (m^2)
12
10
8
6
4
2
0
0
1
2
3
4
Time (ms)


RC2-20 (0.30)
Pressure (bar)
3000
2500
2000
1500
1000
500
0
0
1
2
3
4
Time (ms)


RC2-20 (0.30)
Impulse (kPa s)
150
100
50
0
0
1
2
3
4
Time (ms)


RC2-20 (0.30)
Area (m²)
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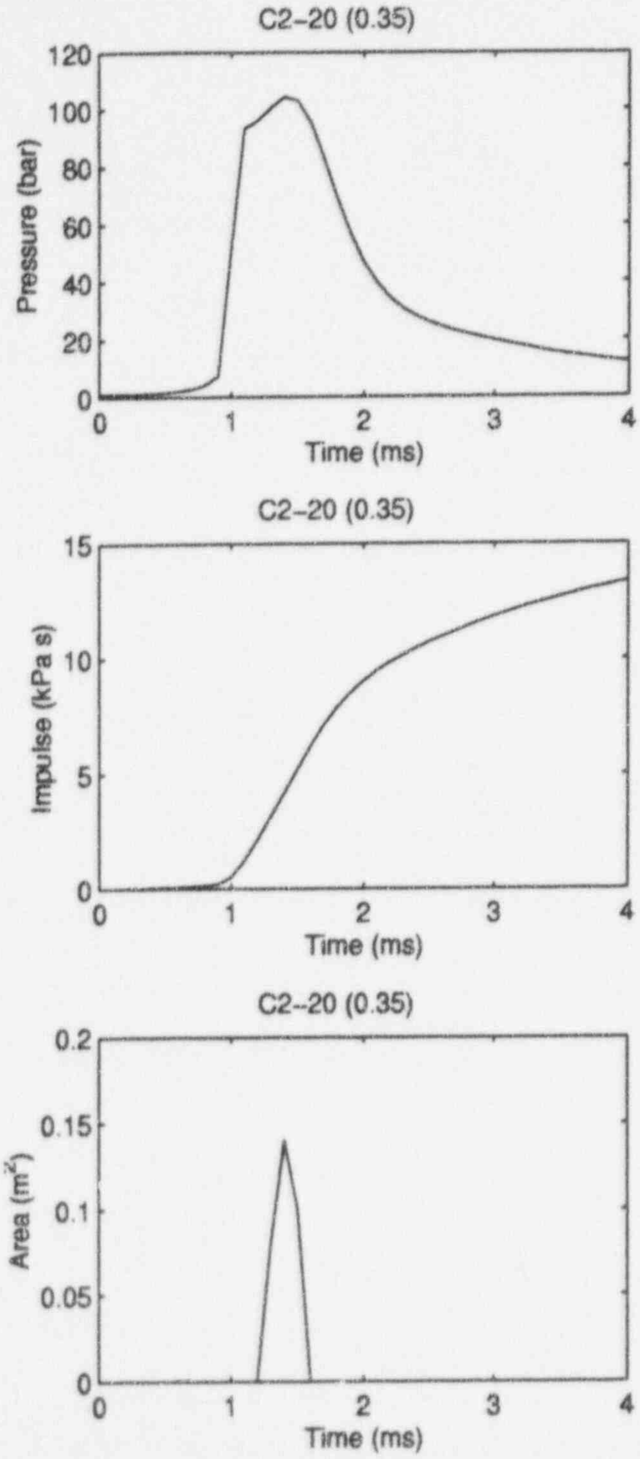
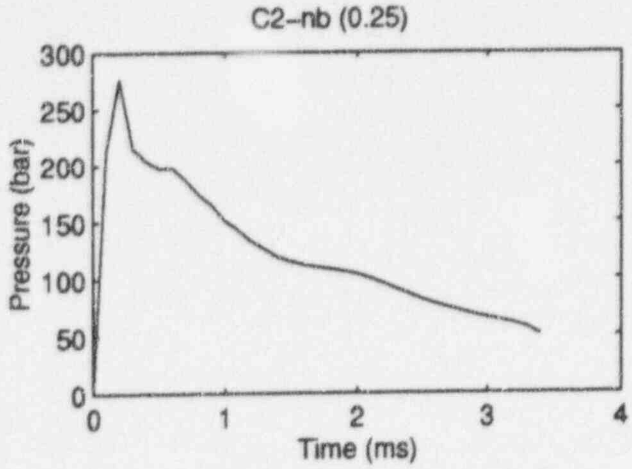
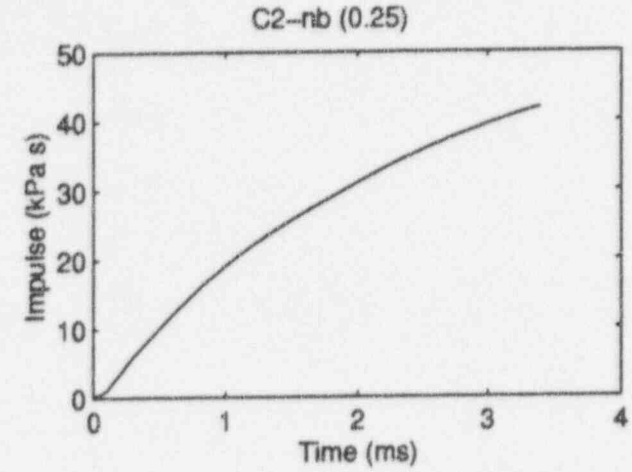
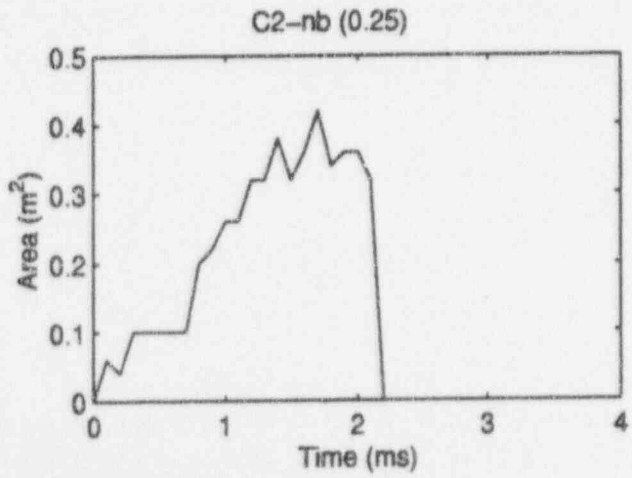
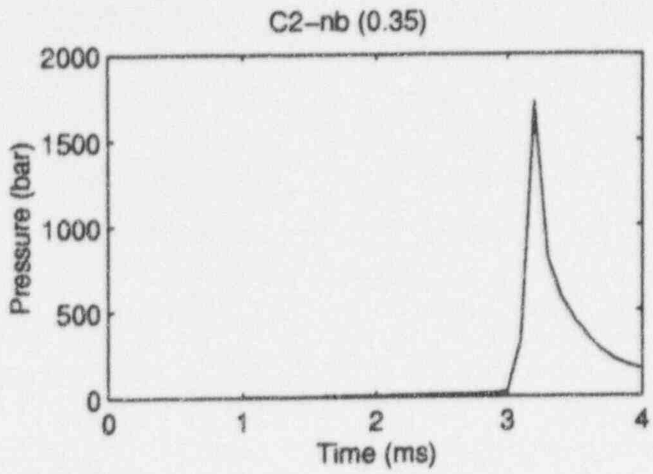
2.5
2
1.5
1
0.5
0
0
1
2
3
4
Time (ms)


C2-20 (0.35)
Pressure (bar)
Time (ms)
C2-20 (0.35)
Impulse (kPa s)
Time (ms)
C2-20 (0.35)
Area (m²)
Time (ms)

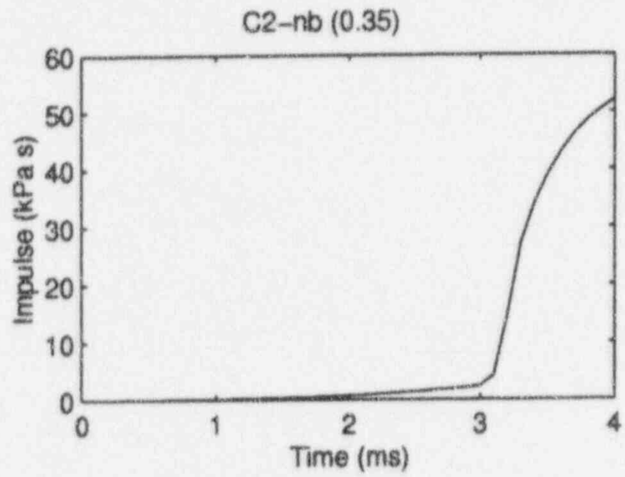

C2–nb (0.25)
Pressure (bar)
Time (ms)


C2–nb (0.25)
Impulse (kPa s)
Time (ms)

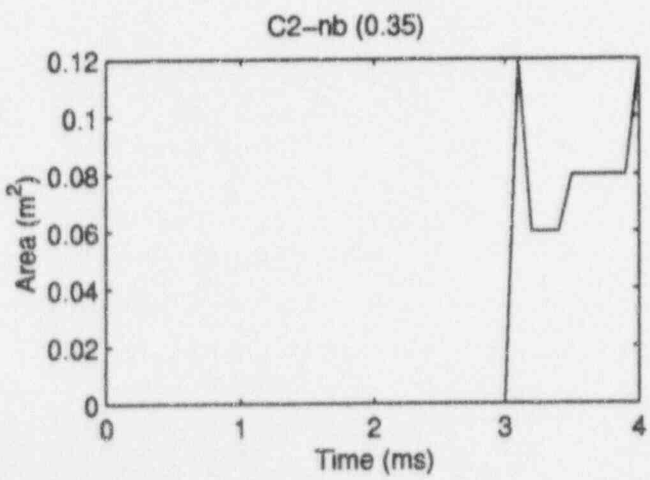

C2–nb (0.25)
Area (m²)
Time (ms)



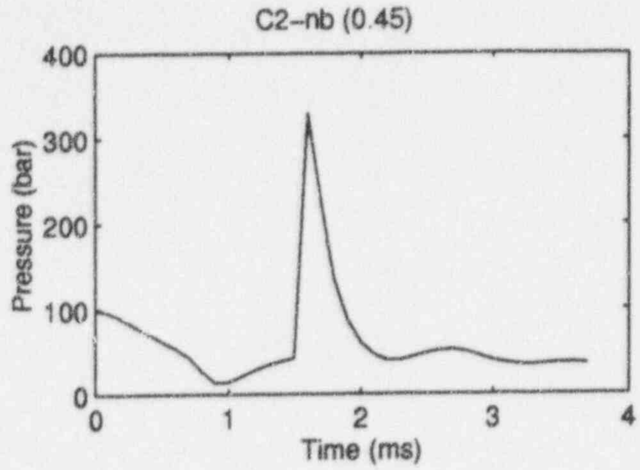
C2-nb (0.35)
Pressure (bar)
Time (ms)
0
500
1000
1500
2000
0
1
2
3
4

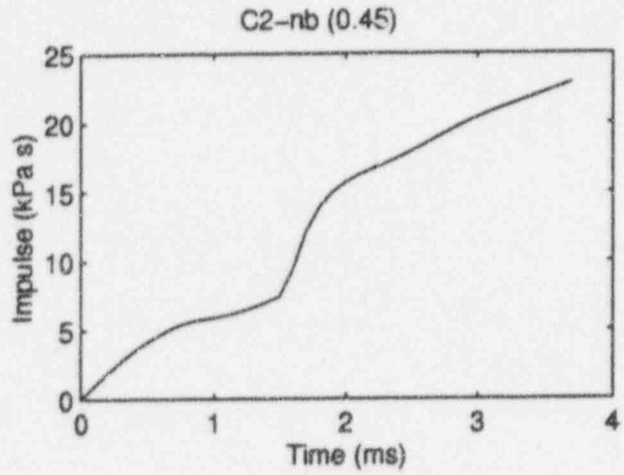


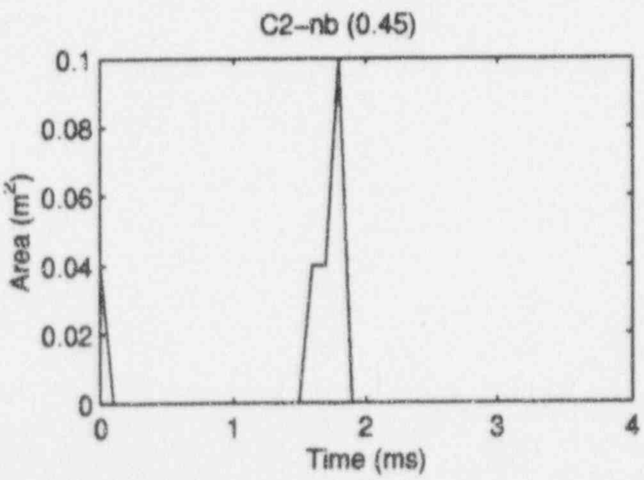
C2-nb (0.35)
Impulse (kPa s)
Time (ms)
0
10
20
30
40
50
60
0
1
2
3
4

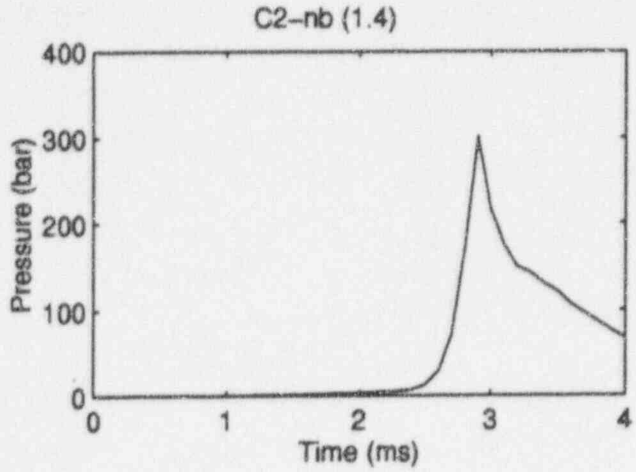


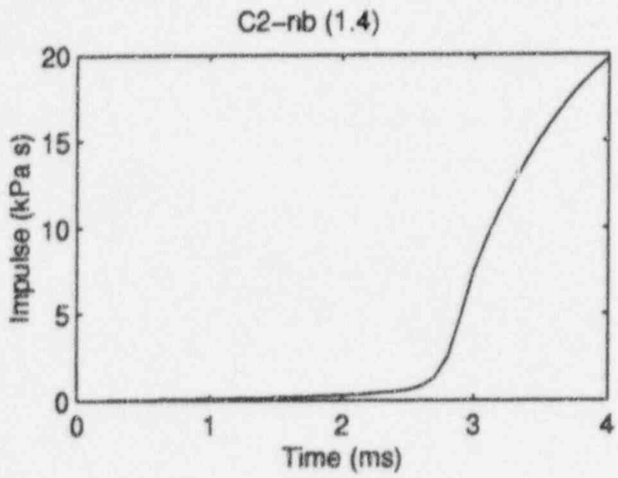
C2-nb (0.35)
Area (m2)
Time (ms)
0
0.02
0.04
0.06
0.08
0.1
0.12
0
1
2
3
4

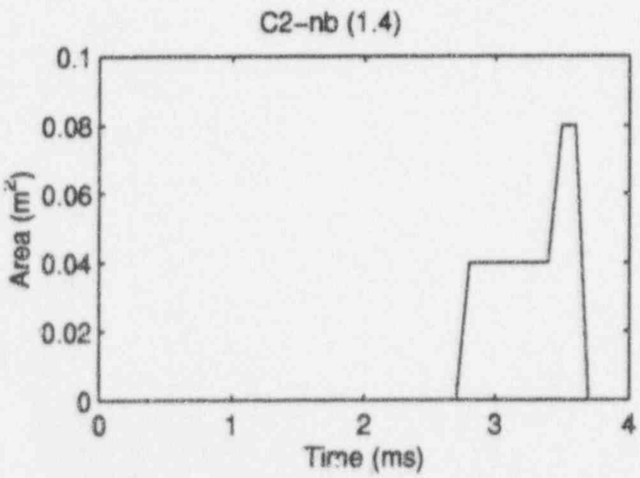

C2-nb (0.45)
Pressure (bar)
Time (ms)


C2-nb (0.45)
Impulse (kPa s)
Time (ms)


C2-nb (0.45)
Area (m²)
Time (ms)


C2–nb (1.4)
Pressure (bar)
400
300
200
100
0
0
1
2
3
4
Time (ms)


C2–nb (1.4)
Impulse (kPa s)
20
15
10
5
0
0
1
2
3
4
Time (ms)


C2–nb (1.4)
Area (m²)
0.1
0.08
0.06
0.04
0.02
0
0
1
2
3
4
Time (ms)

ADDENDUM TO APPENDIX B

ON THE REGIMES OF PREMIXING

by

S. Angelini, T.G. Theofanous and W.W. Yuen

Proceedings, OECD/CSNI Specialist meeting on FCI
JAERI-Tokai Research Establishment, Japan, May 19–21, 1997

On the Regimes of Premixing

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Abstract: The conditions of the MAGICO-2000 experiment are extended to more broadly investigate the regimes of premixing, and the corresponding internal structures of mixing zones. With the help of the data and numerical simulations using the computer code PM-ALPHA, we can distinguish extremes of behavior dominated by inertia and thermal effects—we name these the inertia and thermal regimes, respectively. This is an important distinction that should guide future experiments aimed at code verification in this area. Interesting intermediate behaviors are also delineated and discussed.

Keywords: Premixing, Steam Explosions, Void Fraction, Flash X-ray Radiography.

1. INTRODUCTION

It is now well understood that large-scale steam explosions are highly dynamic events of propagative character. The media that support such propagations are called premixtures, and they are characterized by the volume fractions and length scales of their constituents–melt, liquid coolant, and vapor. Premixing is called the process that leads to the formation of premixtures, and it is a highly transient, generally multidimensional process governed by intense multiphase interactions (although mild by comparison to the propagation itself). As a key step in developing the proper understanding and predictive capability, starting with the MAGICO experiment (Angelini et al., 1992), premixing has been studied with the help of solid particle clouds. This focus on multifield aspects removes a key unknown, the melt length scale, and with well-defined initial and boundary conditions, allows unambiguous comparisons to predictive models. Perhaps more importantly this approach allows for a systematic variation of the experimental condition, so as to exercise the predictive tools, at the fundamental level, over wide ranges of conditions. This approach was continued with the MAGICO-2000 experiments, and more recently with the QUEOS and BILLEAU experiments in Germany and France, respectively. The MIXA experiments, which opened the way, together with MAGICO, employed pre-broken-up thermitic melts, and thus they were also guided to a large extent by similar considerations (Fletcher and Denham, 1993). How to build towards including breakup on this basis has been discussed and demonstrated in connection with specifying and using (for reactor assessment) the PM-ALPHA code (Theofanous et al., 1997a; Theofanous et al., 1997b).

The present work is continuing the efforts on the multifield aspects. **Our main purpose is to introduce the notion of "premixing regimes"**. Specifically, with the help of advanced MAGICO-2000 tests and PM-ALPHA calculations, we distinguish extremes of behavior dominated by inertia and thermal effects. We name these "**inertial**" and "**thermal premixing regimes**," respectively, and expect them to provide an important additional anchor in planning future experiments and assessing completeness of code verification efforts.

2. PRELIMINARY CONSIDERATIONS

As described previously (Angelini et al., 1995), PM-ALPHA calculations under cold MAGICO-2000 conditions revealed a plunger-like action—that is, deep cavities or "holes" in the liquid pool forming just behind the descending cloud and closing up a short time later. The volume fraction of solids in the cloud, before entering the water, was ~10% and MAGICO-2000 experiments confirmed these predictions. This "plunger" effect is even more pronounced in the QUEOS experiments, where the mode of particle delivery produces very dense ("lumps") particle clouds with volume fractions of ~17%. Here, the length of the clouds is rather short (~30 cm), the water pools are saturated at the top and subcooled due to gravity head, and they are subcooled further due to self-pressurization, and the steaming appears as a short burst coincident with the lifetime of the water "hole" (~250 ms). Even with hot runs this is a heavily inertia dominated behavior, not really germane to the premixing process. PM-ALPHA calculations indicate that to avoid the inertia regime, the particle volume fraction must be reduced to just a few percent. In MAGICO-2000, the pours are longer (150 cm), and by comparison to QUEOS rather dilute (~ 2%), and this places them outside of the range of inertia dominated regimes. However, the corresponding pour duration is only ~0.33 s, and a purely thermally-dominated regime would have even lower particle volume fractions and, particularly, prolonged pour durations. These considerations lead to a special adaptor in the MAGICO-2000 facility and the experiments reported here. In these experiments, we have achieved pours of ~0.5% particle volume fraction, ~6.5 m length and ~1.5 s pouring time, for a thermally-dominated behavior.

3. EXPERIMENTAL APPARATUS AND MEASUREMENT TECHNIQUES

The MAGICO-2000 experiment has been described previously (Angelini et al., 1995). Its central component is the graphite heating element, illustrated in Figure 1. It can deliver up to ~5 kg of ZrO_2 particles at temperatures up to ~2000 °C. For the present experiment, this element was "fitted" with a special device, such as to deliver the

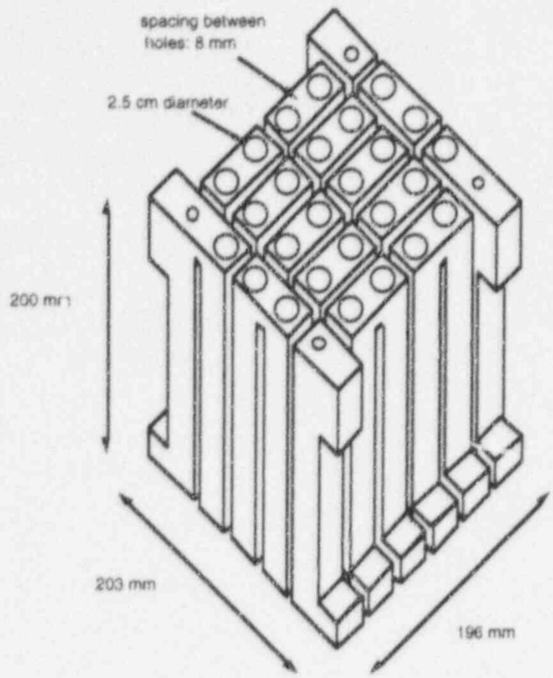

Figure 1. The heating element in MAGICO-2000.

cloud at a much "diluted" condition--volume fraction of 0.5%, pour length 6.5 m, pour duration 1.5 s. An overall view of the experimental stand, including key dimensions, is shown in Figure 2. In the present experiments, for the interaction tank we employ a two-dimensional slab geometry, as illustrated in Figure 3. The principal emphasis in measurements is placed in visualizing the internal structure of the whole premixing zone, and in obtaining respective composition maps. This is done by radiography, employing flash X-rays, as explained in detail by Angelini and Theofanous (1997). With two film cassettes placed one on top of the other, we can map a 35 x 73 cm region, as illustrated in Figure 3. Particle volume fractions are obtained by counting. In all regions of the film not occupied by particles the steam volume fractions are obtained from the film density and appropriate calibrations. The resolution is 200 dots per inch and the void fraction accuracy is estimated at ± 5% (relative error).

In addition, we obtain photographic records with a 35 mm still camera and two videocameras. During the pour the particle temperature is read using a two-color pyrometer.

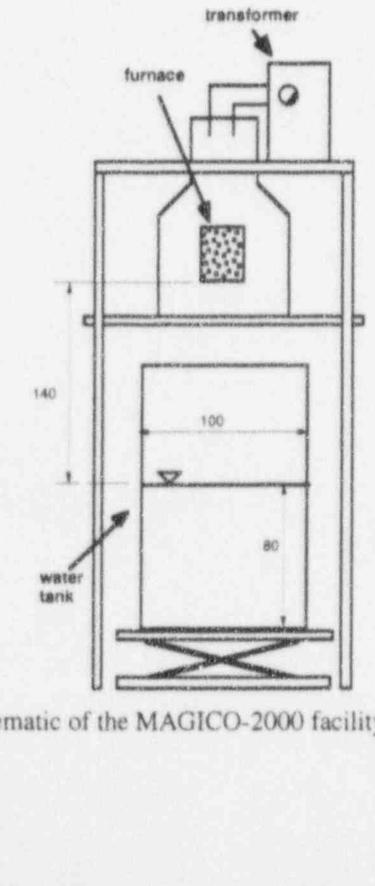

Figure 2. Schematic of the MAGICO-2000 facility. All dimensions are in cm.

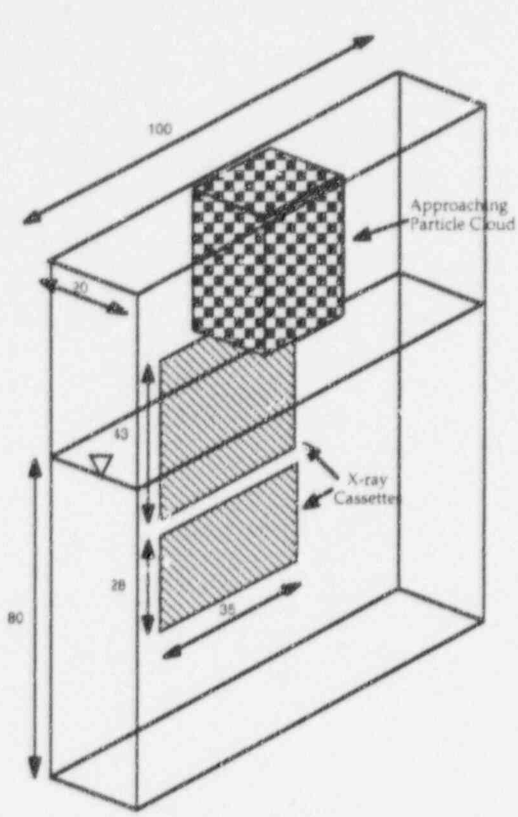

Figure 3. Interaction tank and positions of X-ray cassettes. All dimensions are in cm.

4. EXPERIMENTAL RESULTS AND INTERPRETATIONS

A total of eight experimental conditions were investigated, as summarized in Tables 1 and 2. The key point on these is that the ZrO_2 particulate volume fractions varied by about one order of magnitude. For the zero subcooling runs, saturation throughout was assured by boiling the water pool from below. In the following, code predictions will be shown alongside the presentation of experimental results. The code is the version documented by Theofanous et al., 1996, and the inputs for each run are according to the specifications given in Tables 1 and 2. Only the following additional clarifications are necessary:

1. The Cartesian geometry was modeled by matching exactly the dimensions of the tank. The pour area was modeled by matching its size in the direction of the tank width (the large dimension of the tank), and by spreading it over the entire depth of the 2D field (the small dimension of the tank). A 2×2cm (runs in Table 1) and 2.5×2.5cm (runs in Table 2) numerical grid was used.

2. To determine more precisely the position of the fronts in the Eulerian calculation, we superposed Lagrangian tracer particles, made to move with the local (cell) velocity of the particulate field.

Table 1. Conditions for the Cold MAGICO-2000 Runs in a 2D Slab Geometry

Run	Particle Size [mm]	Total Mass [kg]	Particle Volume Fraction [%]	Impact Velocity [m/s]	X-Ray Time [s]
ZCN	2	5.5	~6.5	~5.2	0.2, 0.25, 0.3, 0.35, 0.4
ZCT	7	5.0	~0.5	~4.4	0.1, 0.8

Table 2. Conditions for the Hot MAGICO-2000 Runs in a 2D Slab Geometry							
Run	Particle Size [mm]	Total Mass [kg]	Volume Fraction [%]	Impact Velocity [m/s]	Particle Temperature [°C]	Water Subcooling [°C]	X-Ray Time [s]
Z11	2	5.5	4.2	5.2	1550	0	0.3
Z12	2	5.7	4.2	5.2	1400	10	0.3
Zb13	7	4.8	5.5	5.2	1600	0	0.3
ZT14	7	5.1	0.5	4.4	1650	0	0.8
ZT15	7	4.4	0.5	4.4	1800	0	0.8
ZT16	7	2.6	0.5	4.4	2000	0	0.8

The five radiographic images taken for the conditions of run ZCN (this means five repeated runs under identical conditions) are shown in Figures 5(a) through 5(e). The void fraction distributions deduced from the first three radiographs are shown in Figures 6(a) through 6(c). We can clearly see the "hole" described above and its closing at ~0.35 s. Notice the visualization detail as, for example, the "clean" shapes of the upper cavity boundary, the highly structured lower portion of the cavity, and the fine detail on the hydrodynamic jet and its disintegration into spray, following collapse. PM-ALPHA predictions are shown in superposition to these radiographic images in Figures 7(a) through 7(e). Note the "wing" pattern of the particle distribution and the accurate depiction of the key void characteristics in the calculation. Also interesting to note is the exact matching of closing and timing and even shape of the emerging jet. Some numerical diffusion is present, as seen for the particles, and a small amount of void (~10%) retained below the liquid surface after closing (Figure 7(c)).

The aim of the dilute pour runs was to avoid this inertia-dominated plunging regime, and this aim was achieved as illustrated with the results of runs ZCT in Figures 8(a) through 8(c). Note in these figures the uniform particle cloud distribution in the air and the deceleration (increase in concentration) obtained in the water pool. We also see that a small amount of air, entrained with the particles, produces void fractions in the 10 to 20% range in the central portion of the mixing zone. Such small amounts of air are expected to be negligible under the strong steaming at hot run conditions.

The radiographs of the six hot runs conducted, under Table 2, are shown in Figure 9(a) through 9(f). It is clear from Figures 9(a) and 9(b) that the small particle runs produce highly voided regions (90 to 100%) even under 10°C subcooling, although in the latter case we see a more limited, in size, voided region. The void fraction maps for the other four, large particle, runs are shown in Figures 10(a) through 10(d). In the three dilute runs, the particle volume fractions in the mixing zone were found to be in the 0.6 to 1.1% range. It is interesting to note here the essential difference in regimes between Figures 9(c) and 9(d). In Figures 10(a) and 10(b), we read corresponding void fractions in the 90 to 100% and 60 to 70% ranges. Also, it is interesting to note from Figures 10(b), 10(c), and 10(d) that a change in particle temperature from 1650 to 2000°C does not appear to have a significant effect on the resulting premixtures. Numerical interpretations of these data are shown in Figures 11(a) through 11(f). We find all the essential qualitative and quantitative features to be in excellent agreement. These include the location of the particle lower fronts and upper boundaries, the void fraction levels and the locations of sharp gradients, and the voided region pinch-off in the subcooled run. The effect of the temperature and particle size seems to be captured very well also in the calculations.

The last three runs in this series are idealized representations of what we wish to call the "thermal regime" of premixing. As shown above, the behavior is not only quantitatively but also qualitatively different, and this difference becomes more vivid if the internal structures discussed above are viewed in the context of video records of the whole interaction vessel. Due to space and time limitations, this additional information will be presented in a future paper.

5. CONCLUSIONS

The conditions of the MAGICO-2000 experiment were extended to more broadly investigate the regimes of premixing and the corresponding internal structures of the mixing zones. With the help of the data and numerical simulations using the computer code PM-ALPHA, we could distinguish extremes of behavior dominated by inertial or thermal effects—we name these the inertial and thermal regimes of premixing, respectively. This is an important distinction that should guide future experiments aimed at code verification in this area.

ACKNOWLEDGEMENTS

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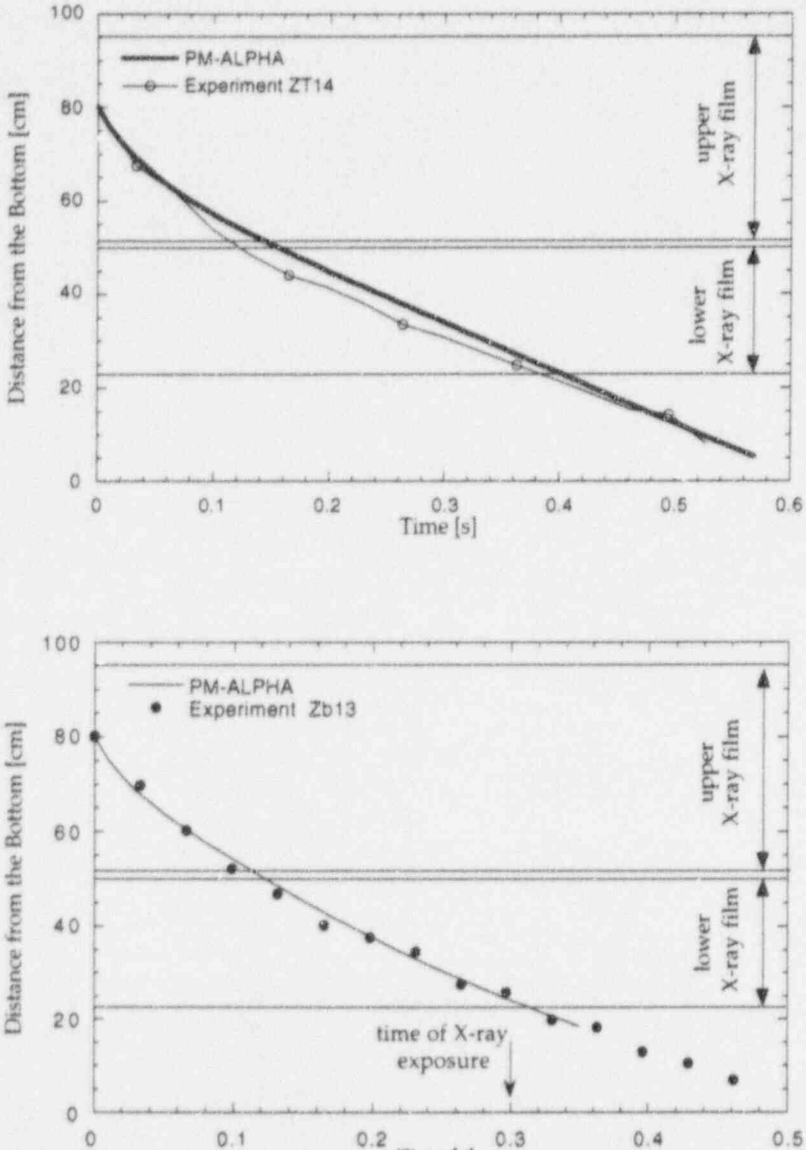


Figure 4. Front advancement in two hot runs at 0.5% (top) and 5.5% (bottom) inlet particle volume fractions, and typical comparisons with PM-ALPHA predictions. Also shown are the regions covered by the two X-ray cassettes.

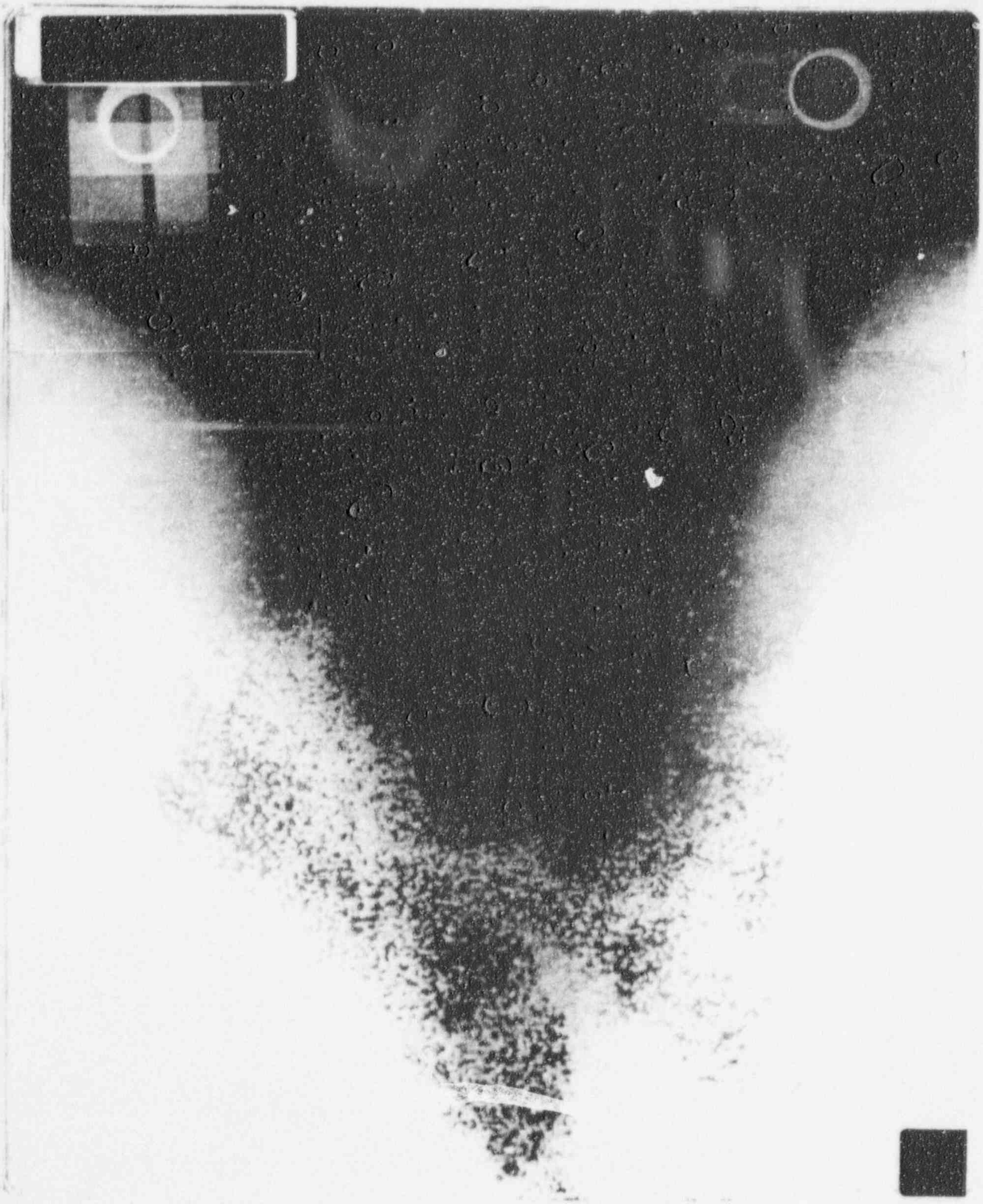


Figure 5(a). Radiographs for runs ZCN. These are repeat runs and the five images (a)–(e) were obtained at times 0.2, 0.25, 0.3, 0.35 and 0.4 s following initial contact of the cloud with the water.

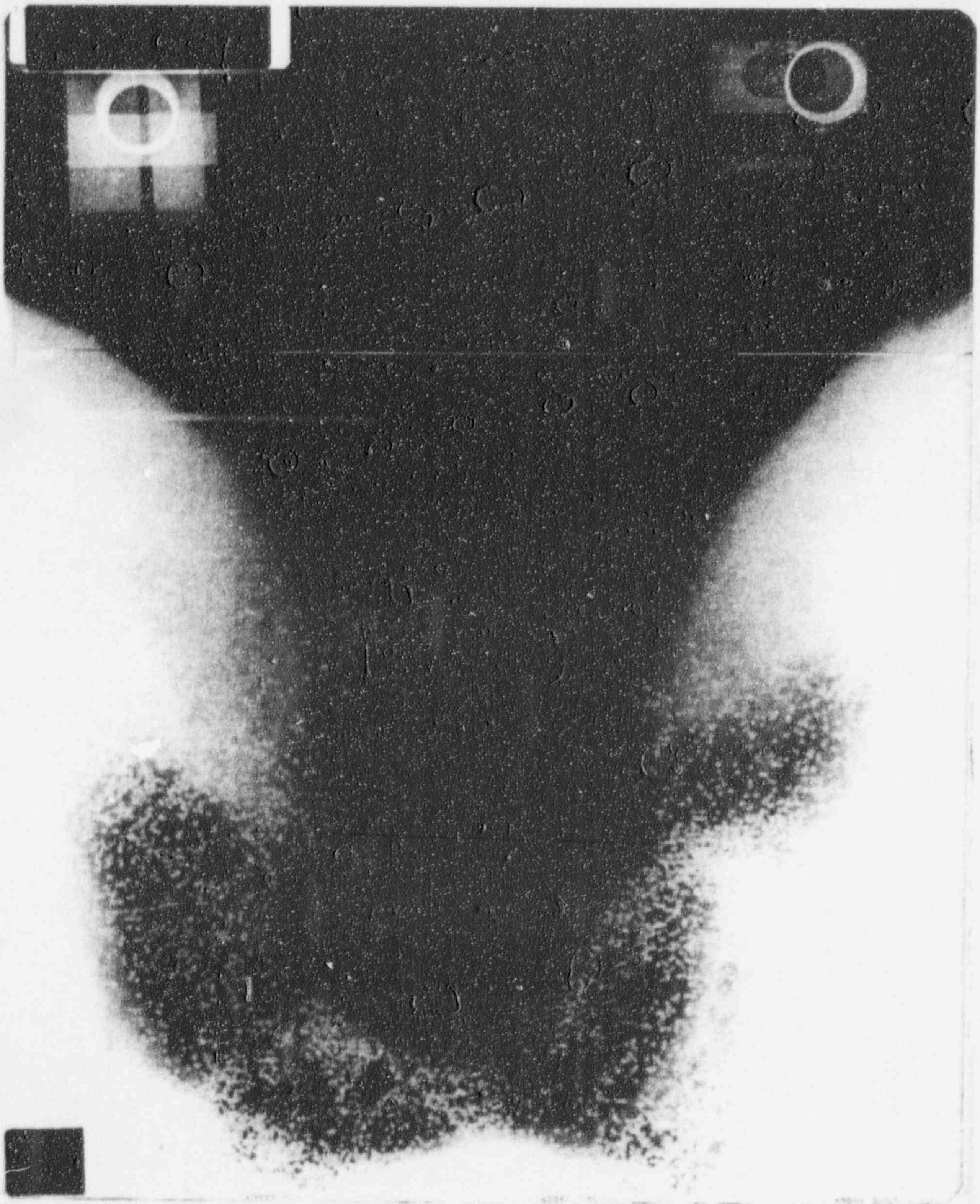


Figure 5(b).

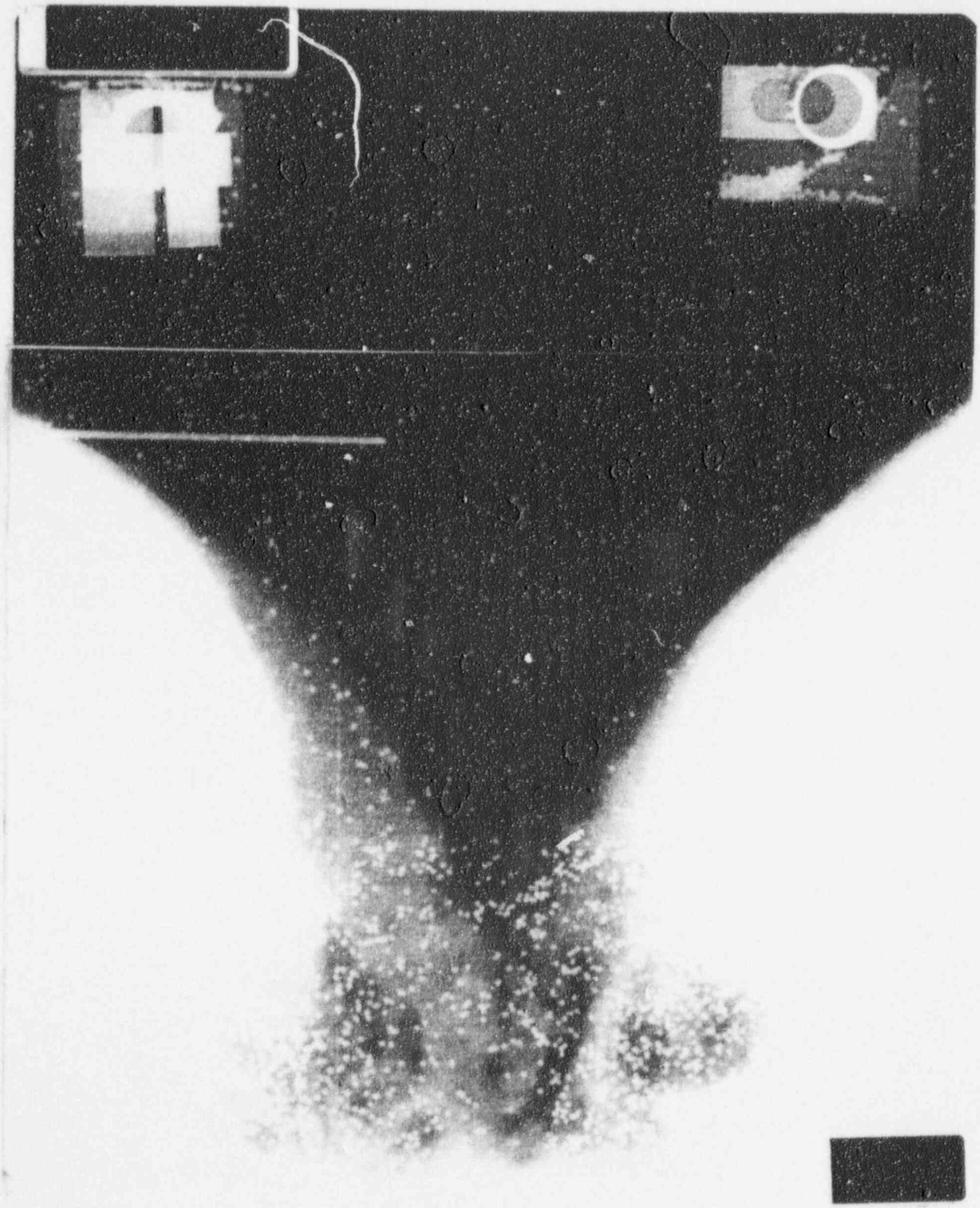


Figure 5(c).

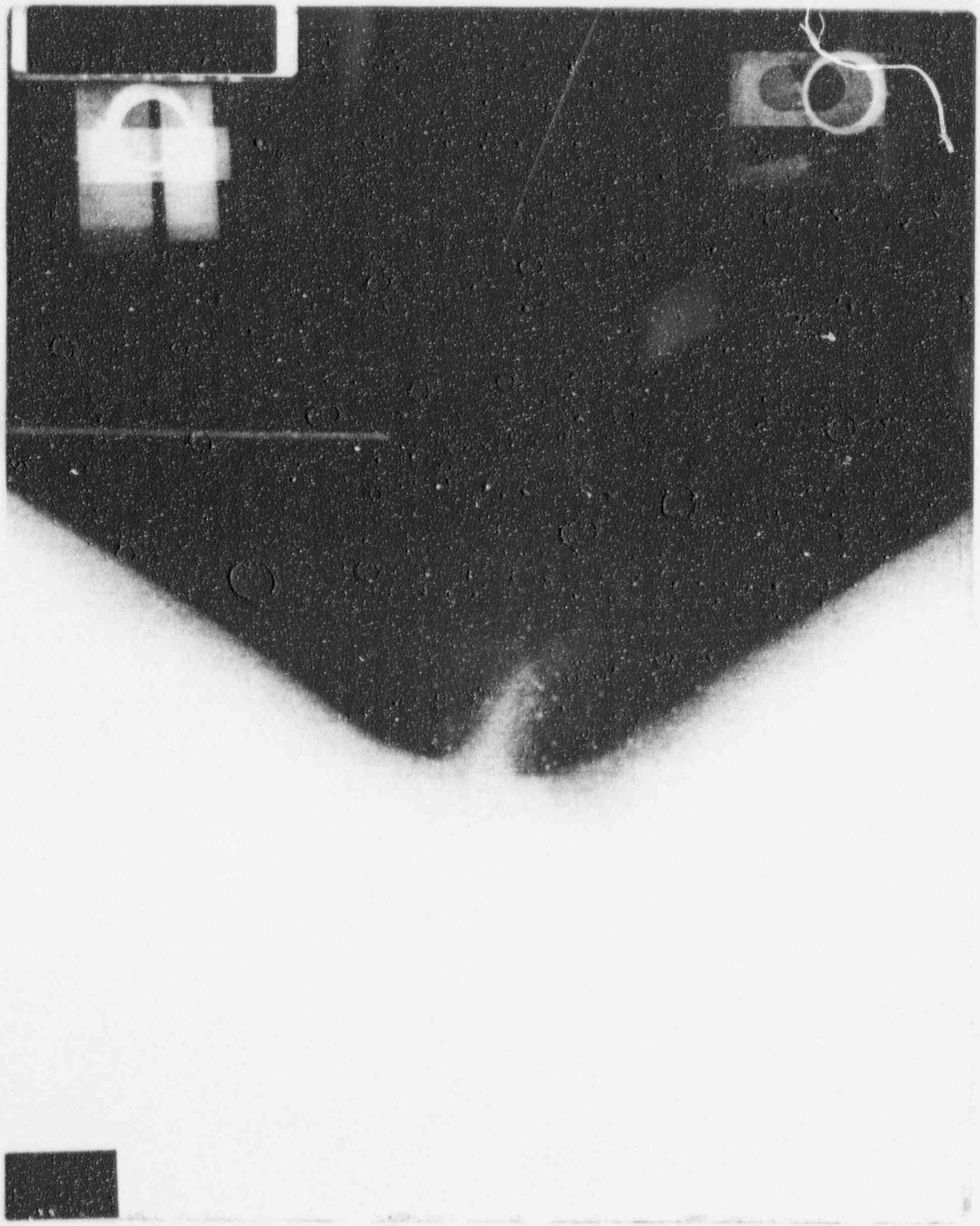


Figure 5(d).

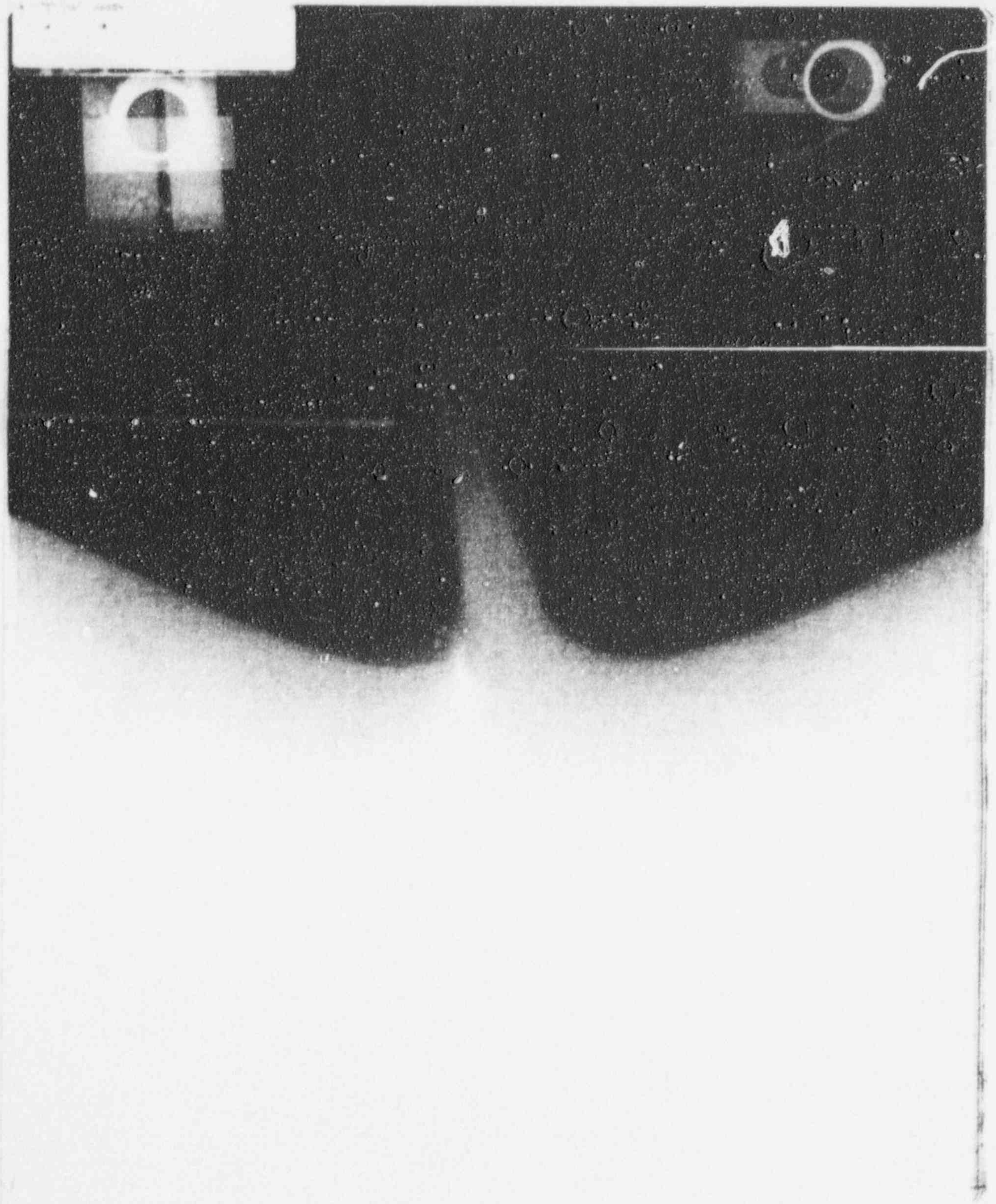


Figure 5(e).

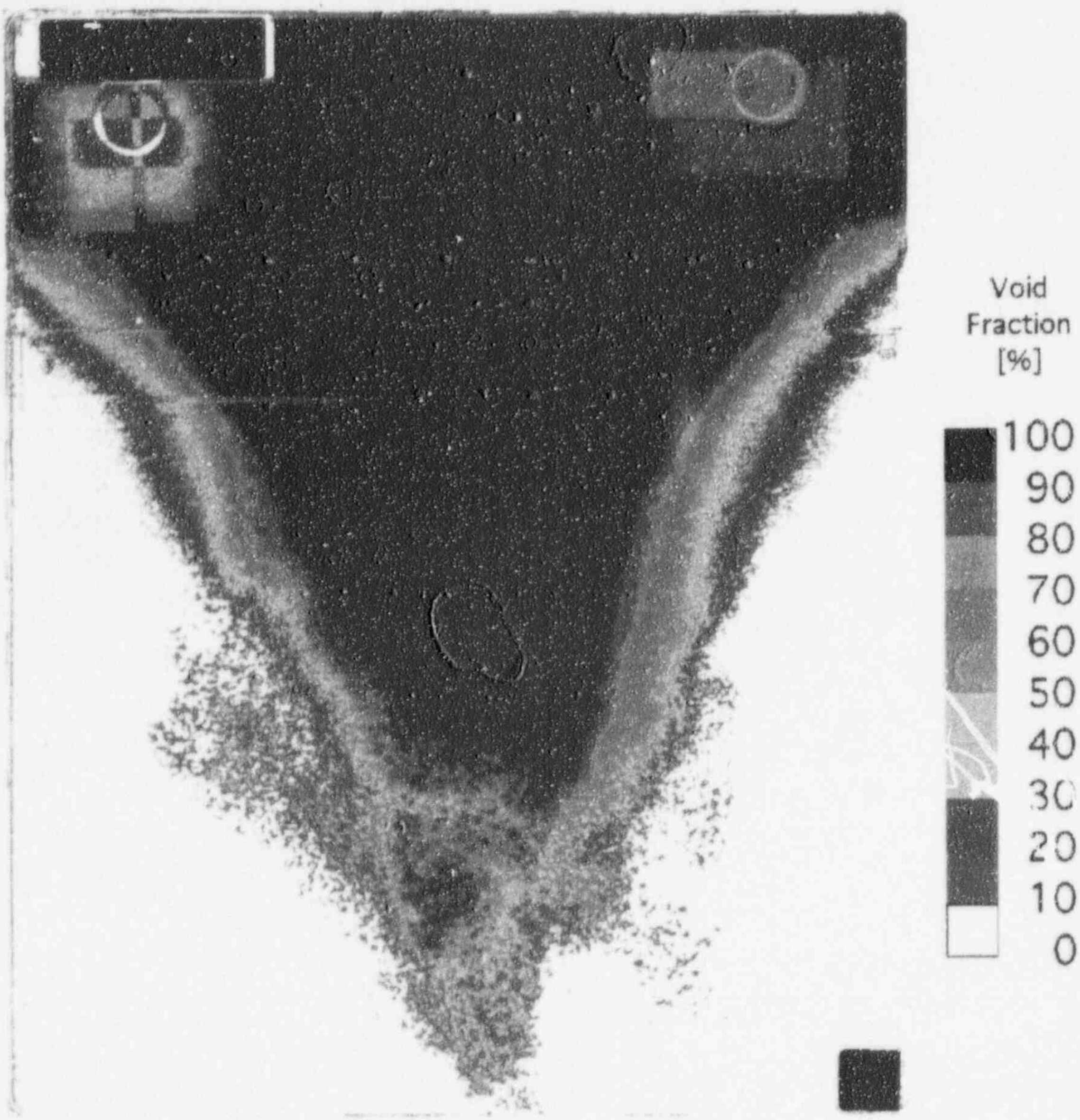


Figure 6(a). Void fraction distributions deduced from the radiographs of Figure 5. The (a) through (c) correspond to the (a) through (c) of Figure 5.

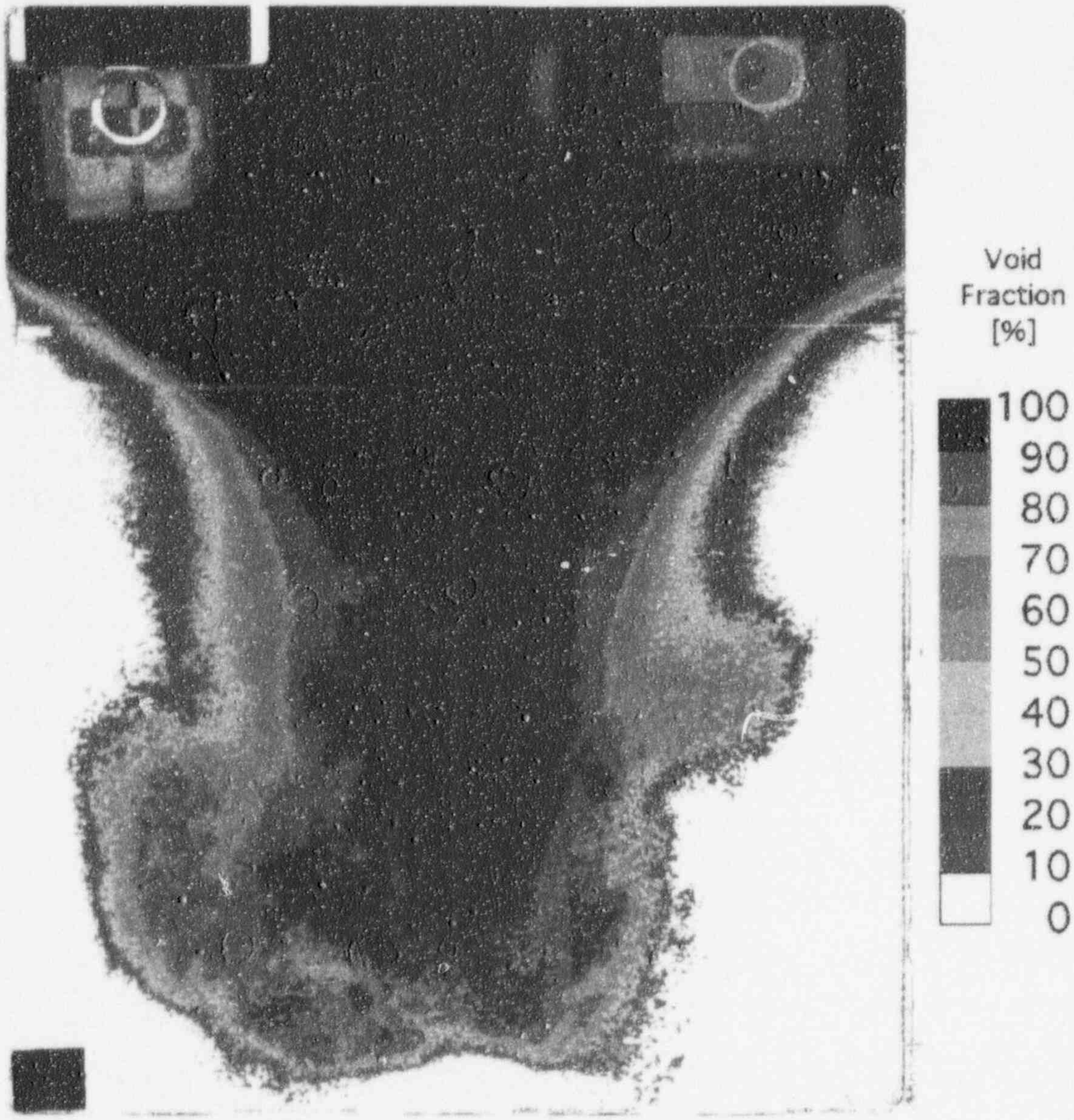


Figure 6(b).

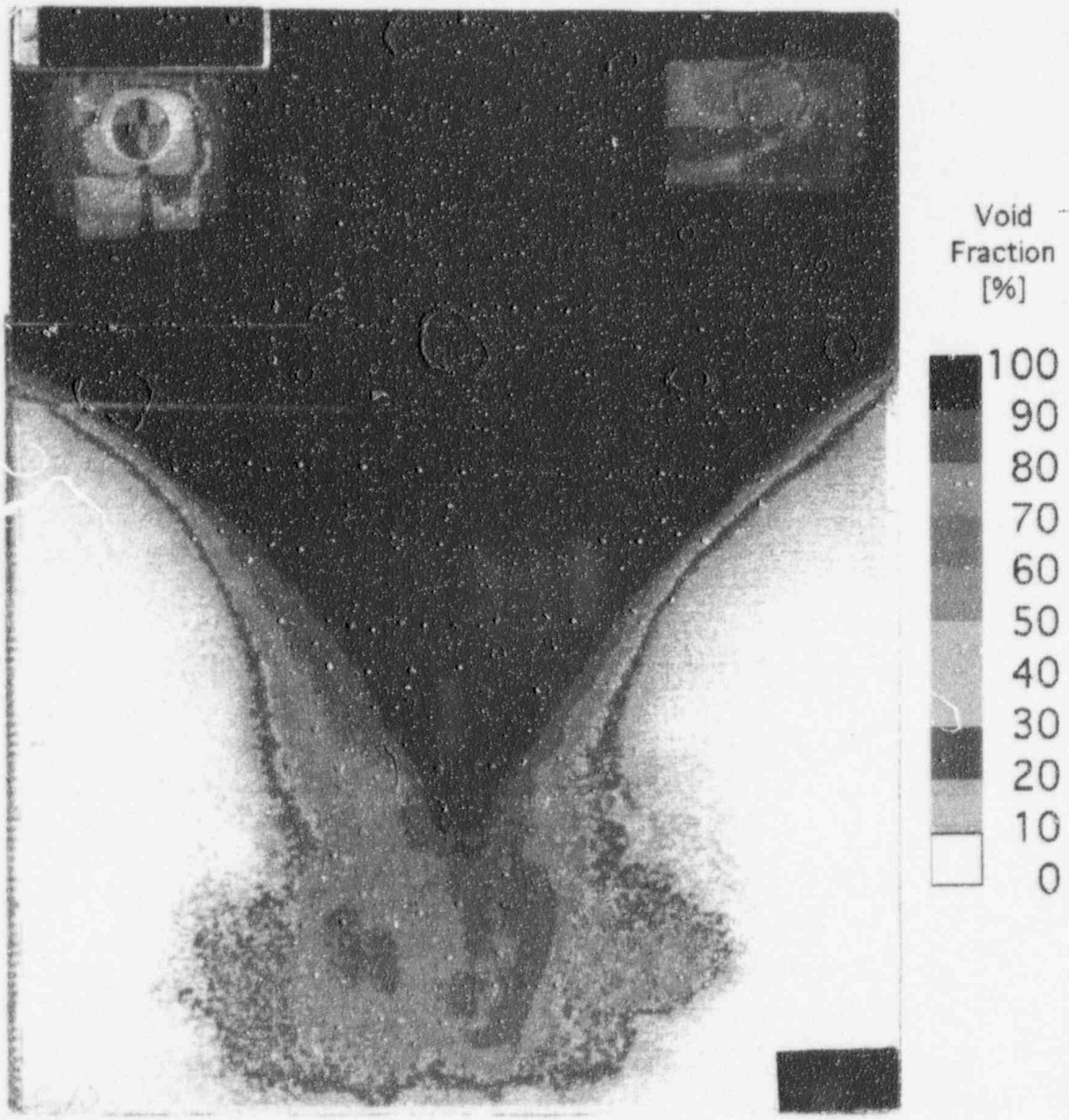
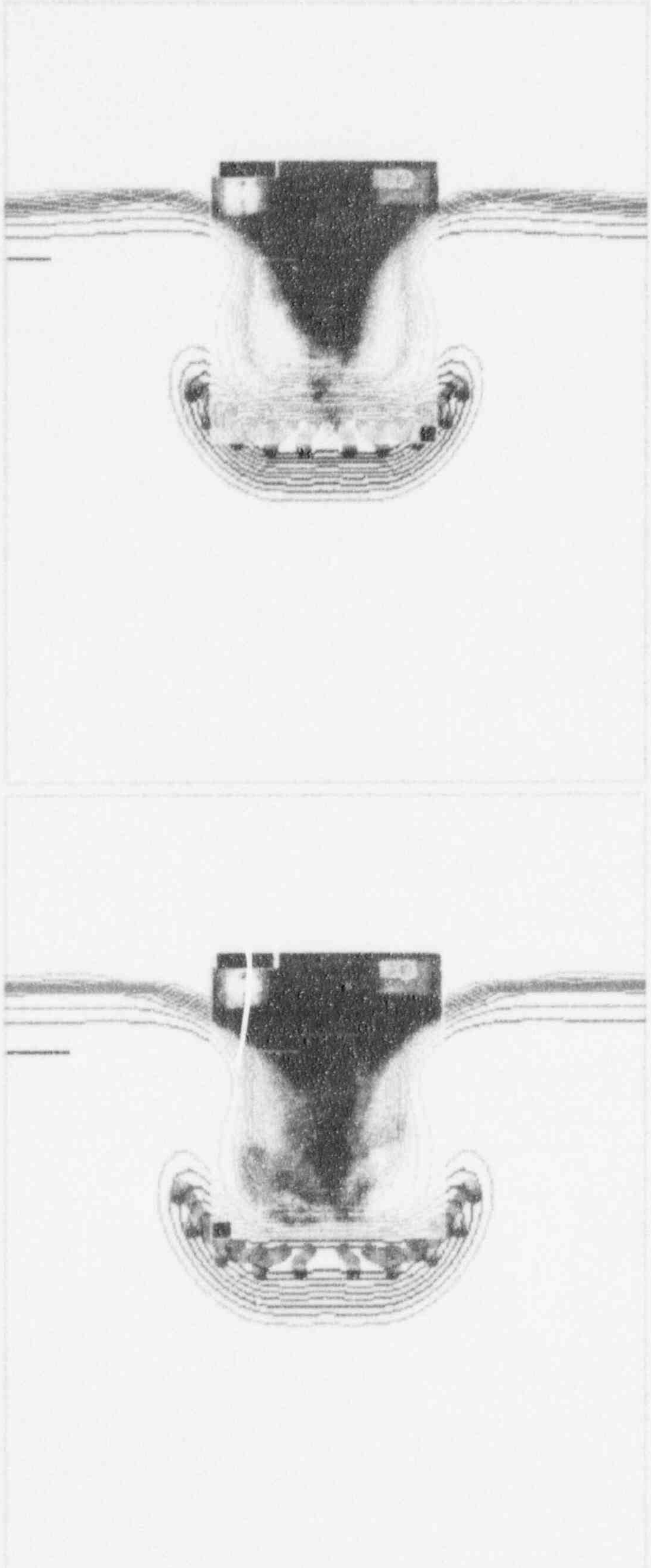
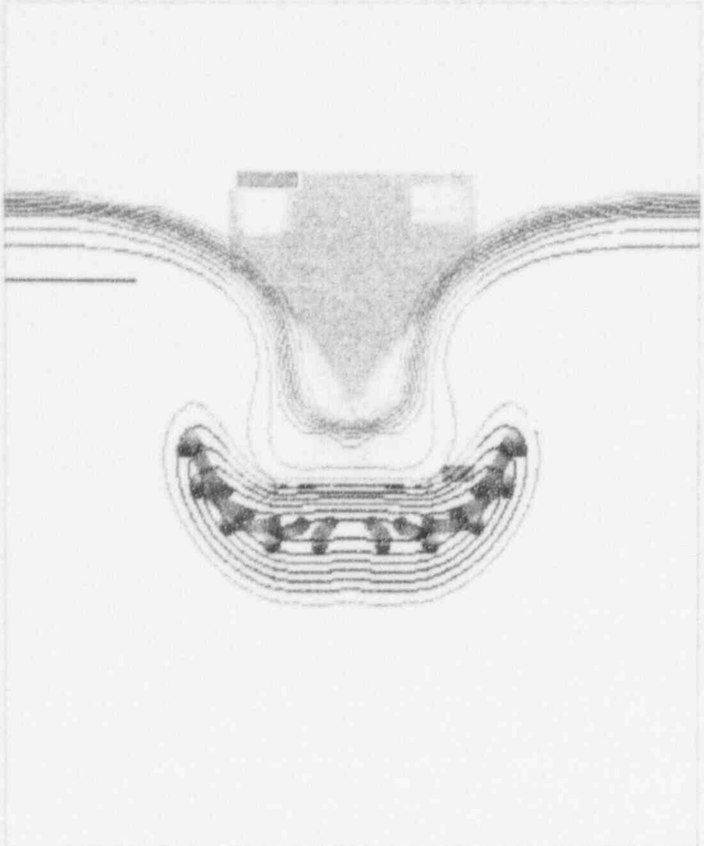
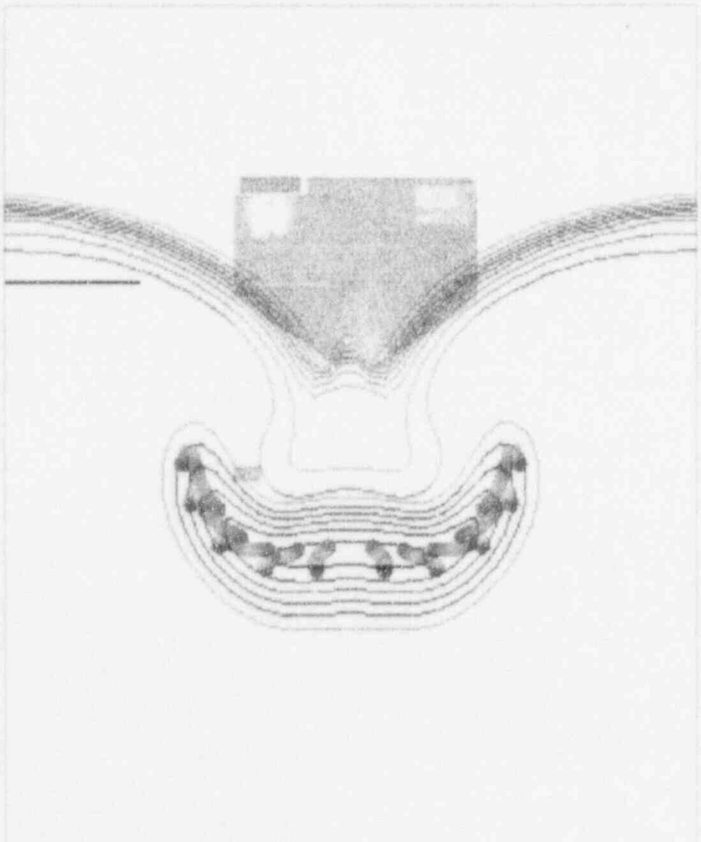


Figure 6(c).



Figures 7(a) and 7(b). PM-ALPHA predictions of the void fraction and particle distributions for the five ZCN runs. The (a) through (e) correspond to (a) through (e) of Figure 5. Superposed also are the radiographic images.





Figures 7(c) and 7(d).

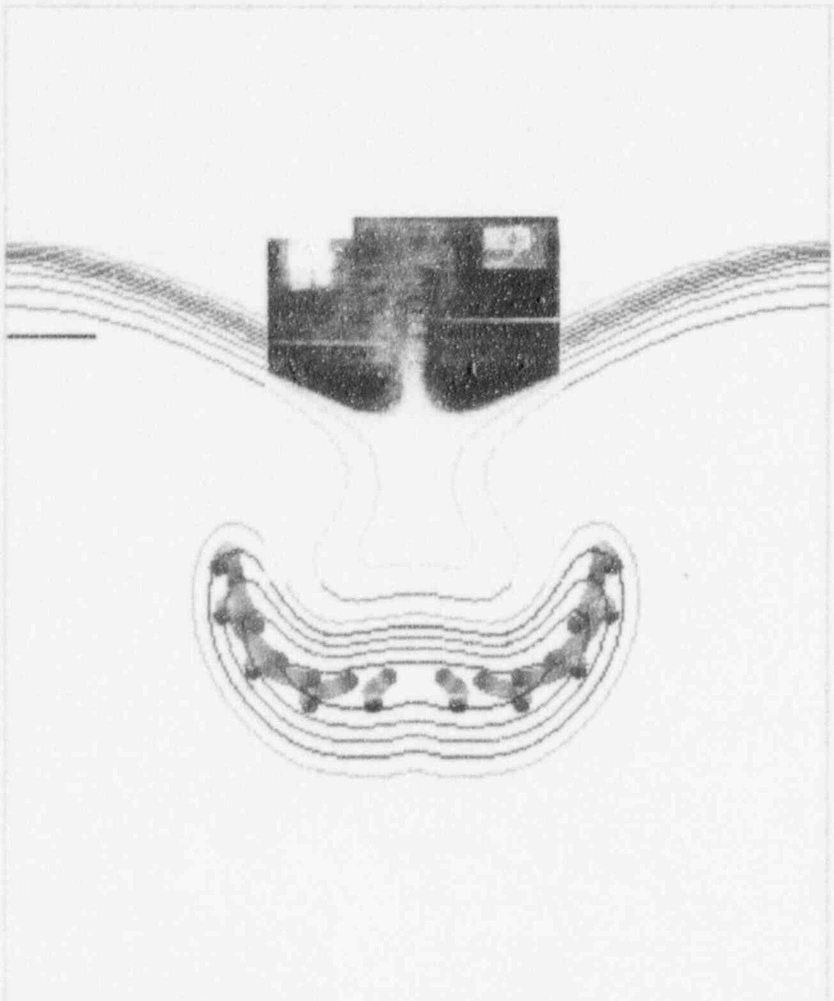


Figure 7(e).

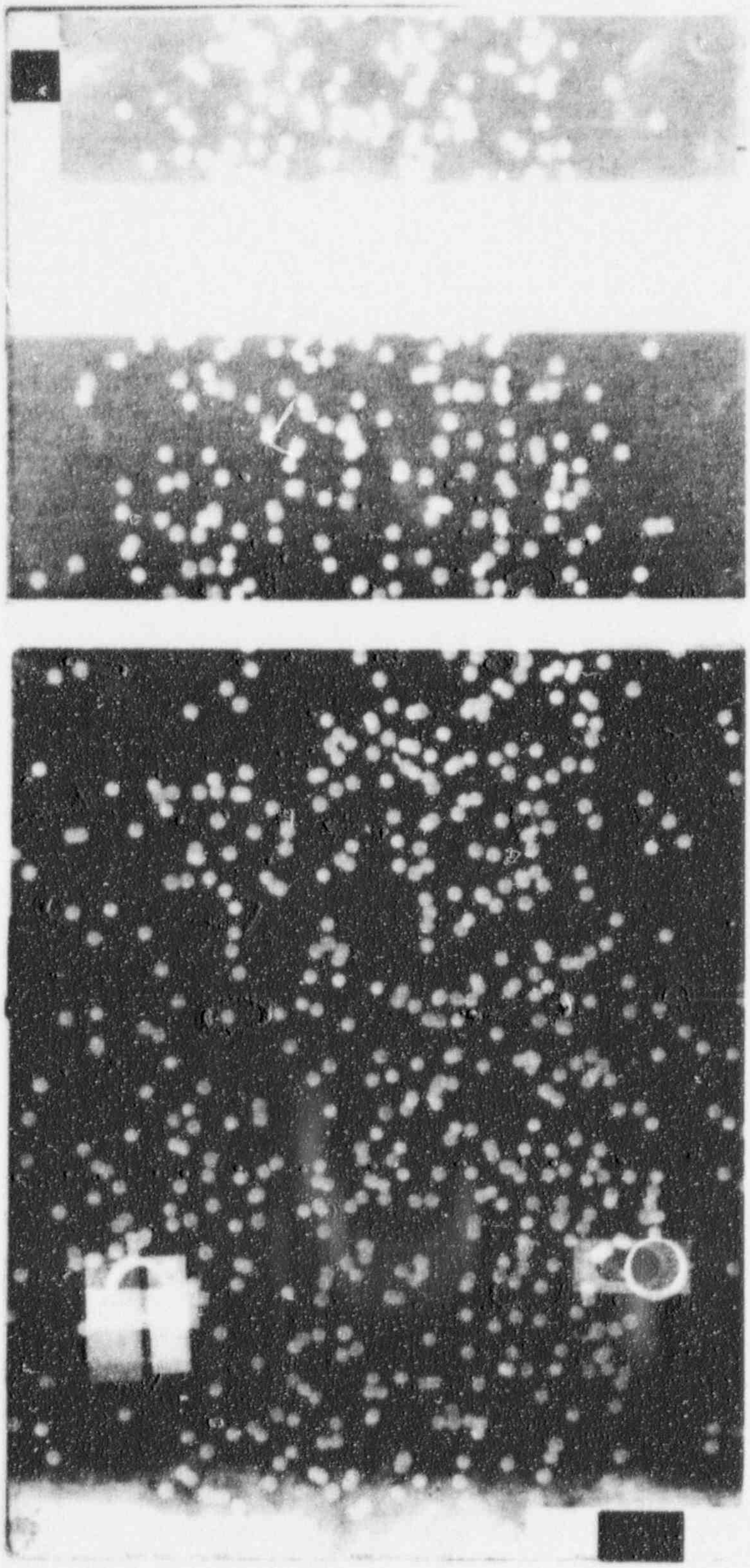


Figure 8(a). Radiographs and deduced volume fraction distributions from runs ZCT: (a) image of a portion of the particle cloud in the air, (b) radiograph of the mixing region at 0.8 s following contact with water, and (c) quantified version of the radiograph.

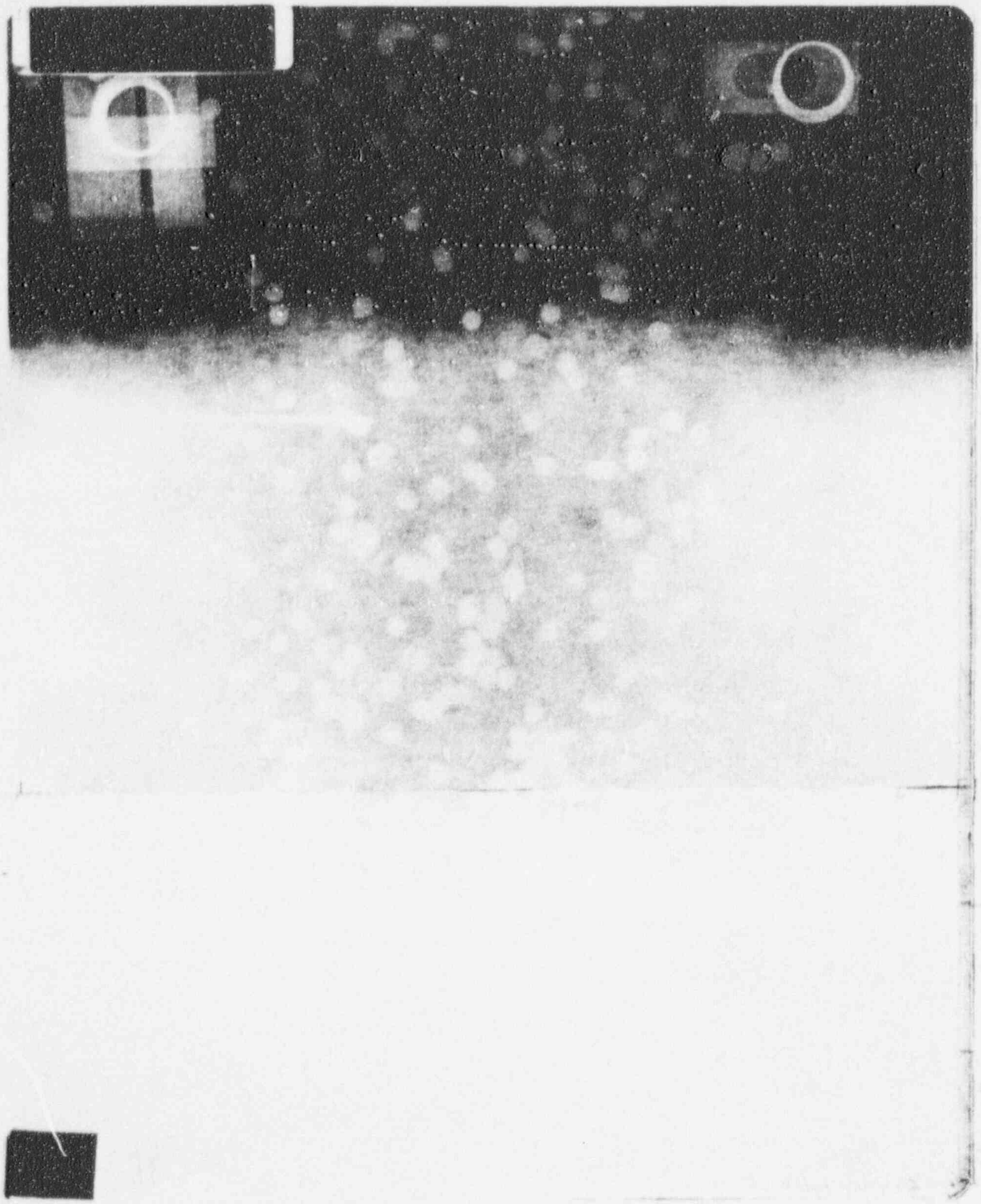


Figure 8(b).

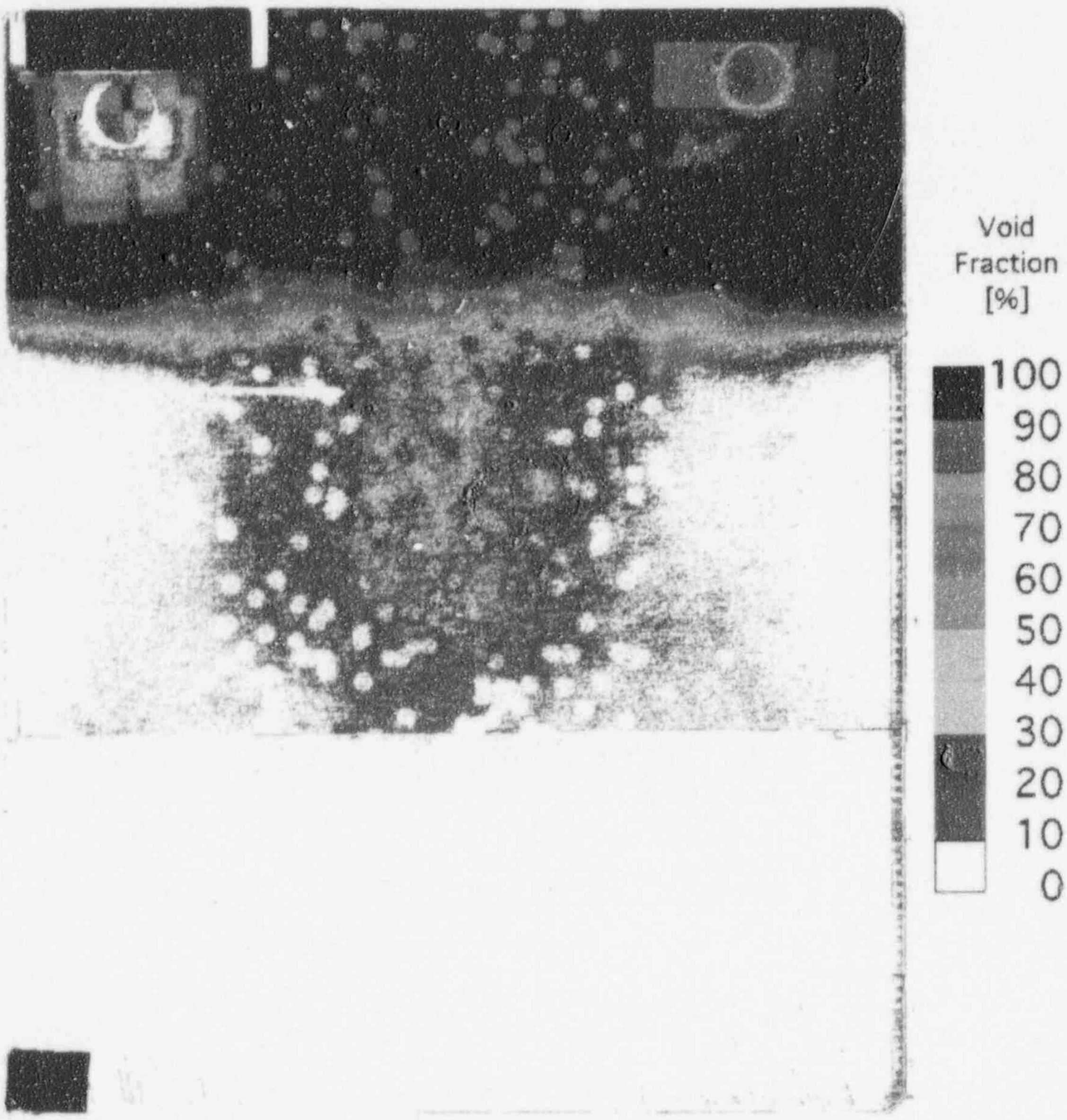


Figure 8(c).

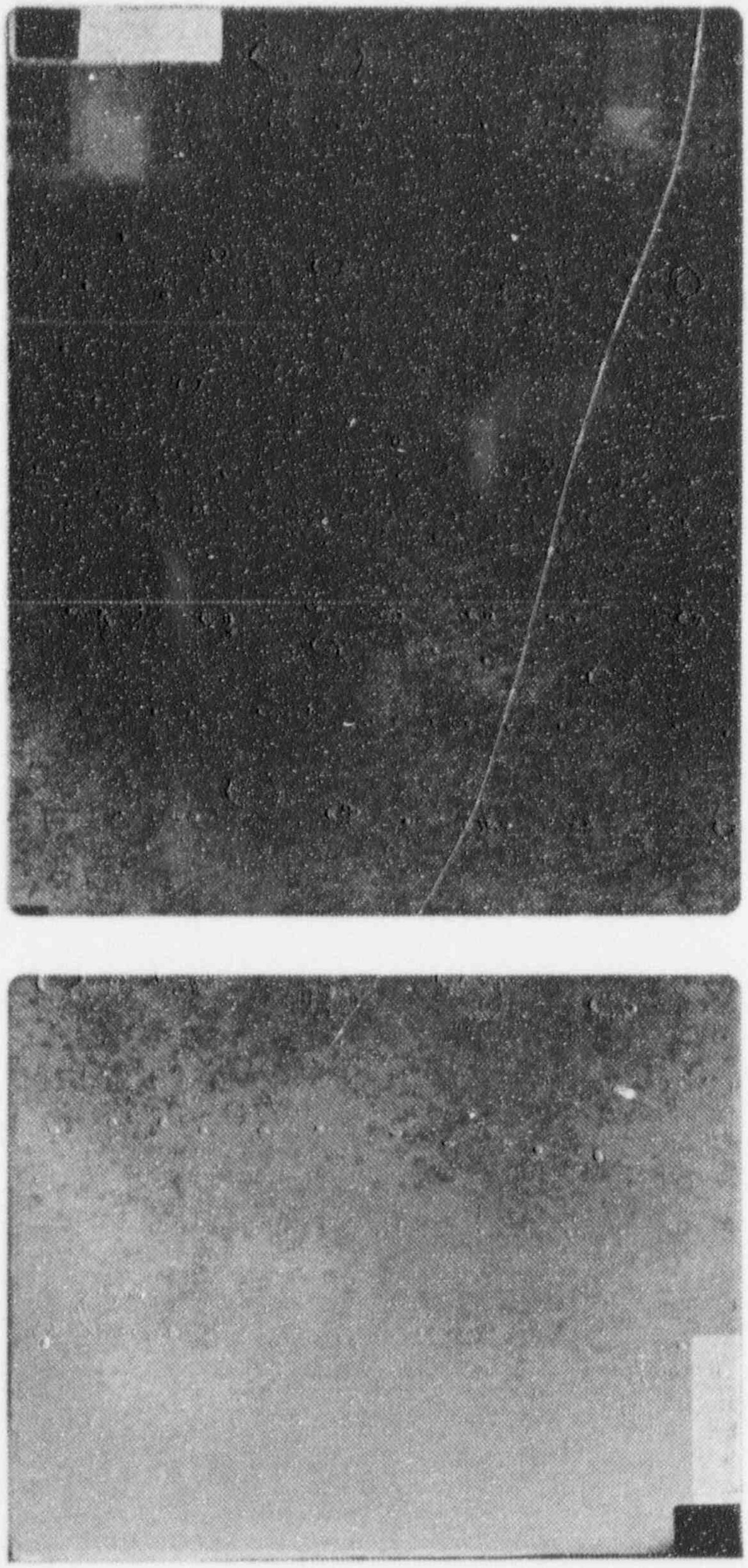


Figure 9(a). The radiographs obtained in the runs of Table 2. The (a) through (f) correspond to runs 11 through 16 in the table.

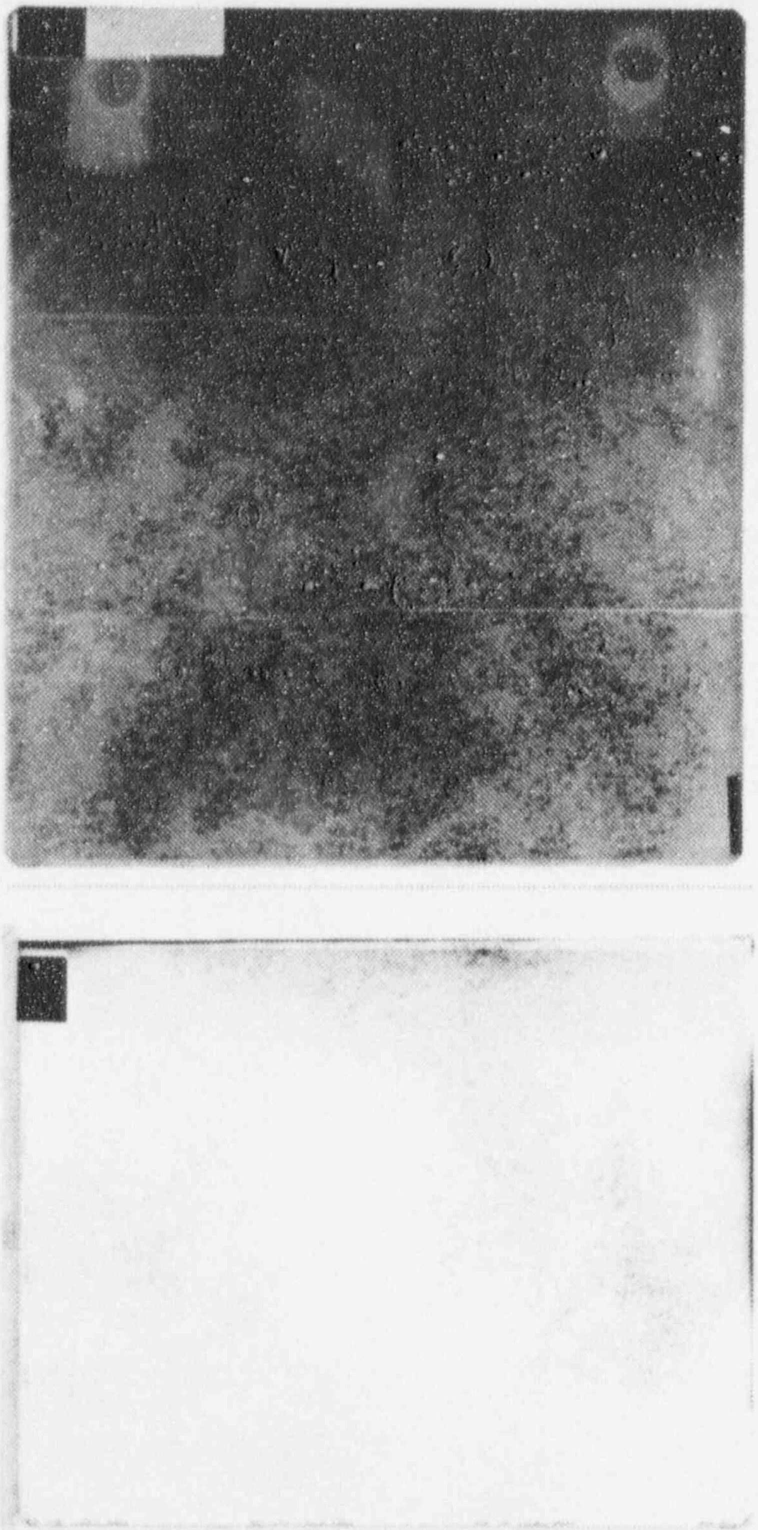


Figure 9(b).

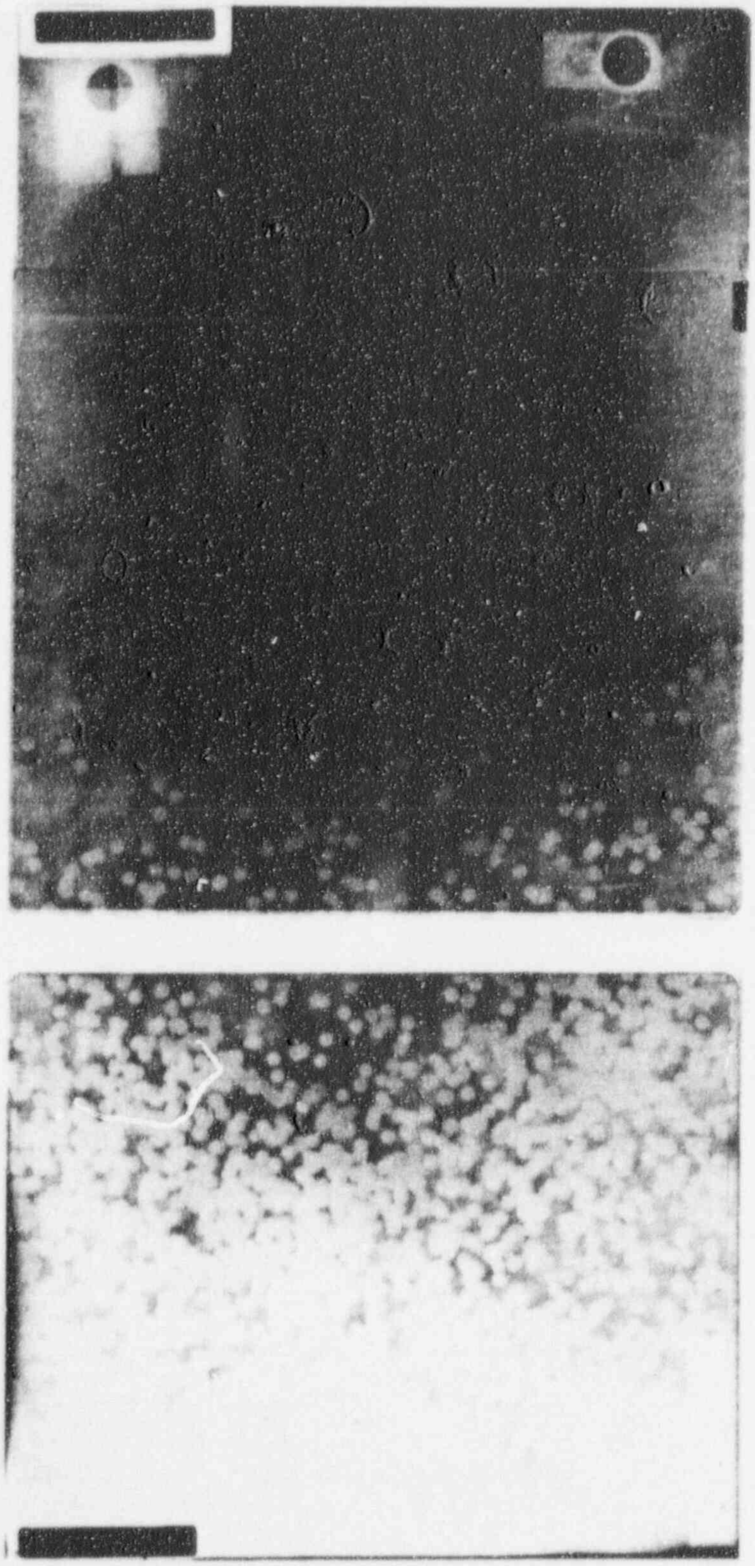


Figure 9(c).

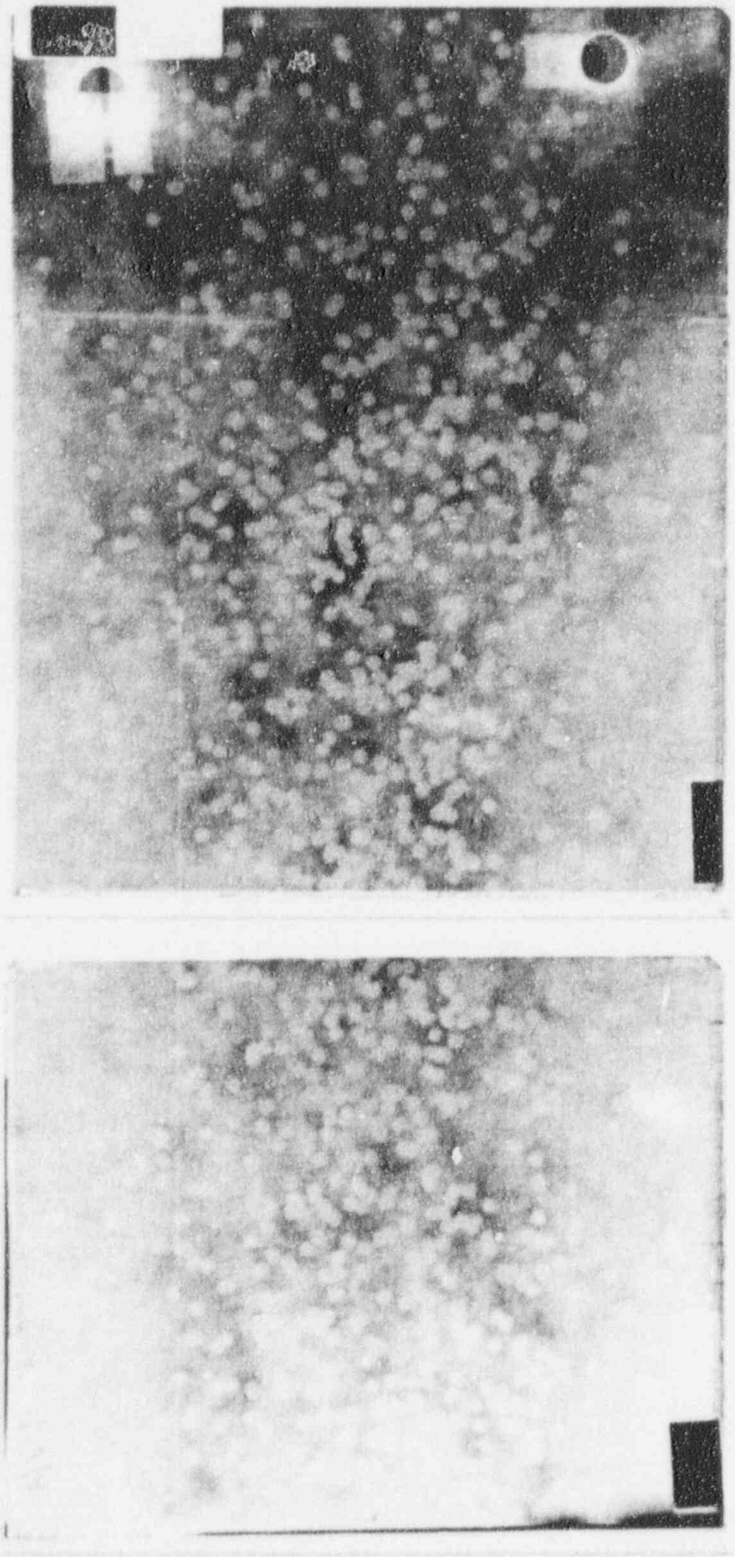


Figure 9(d).

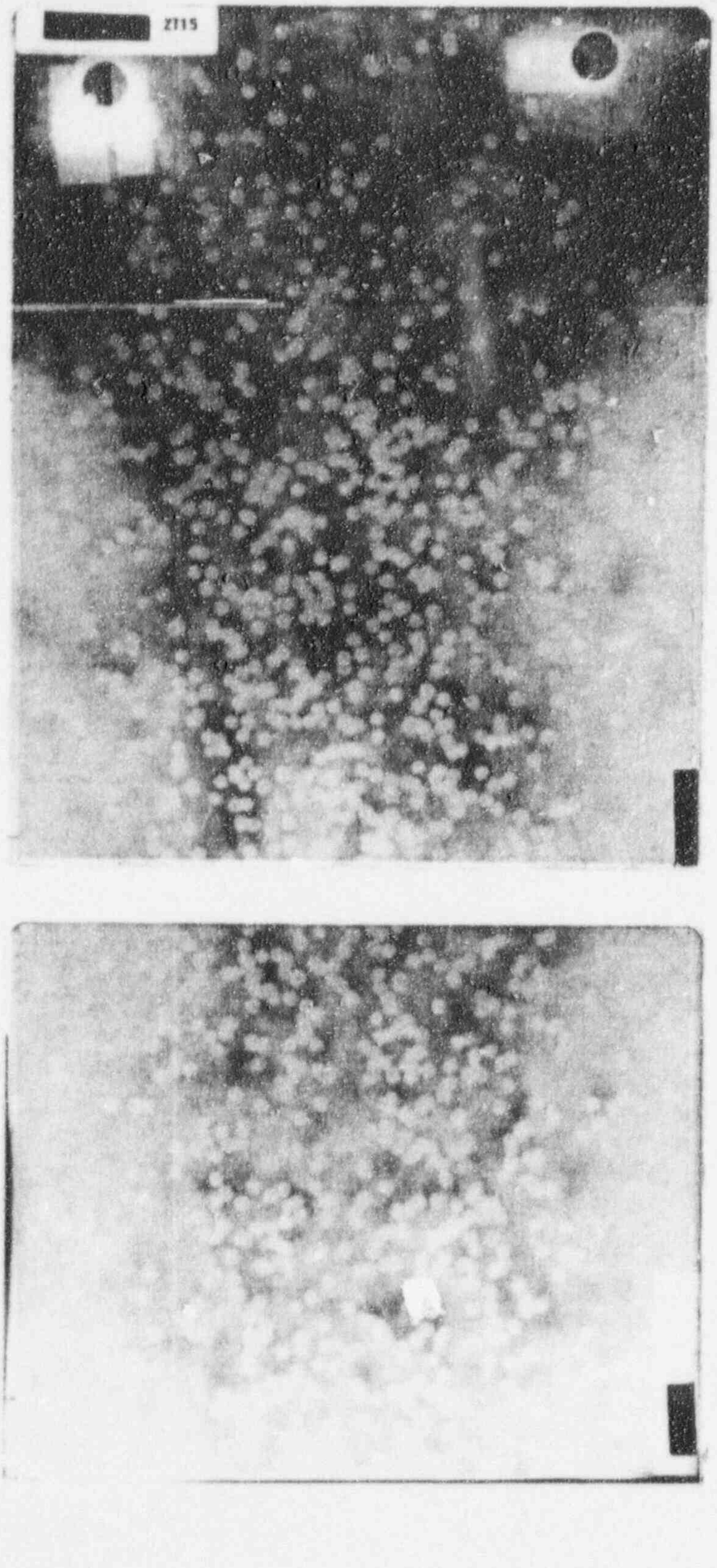


Figure 9(e).

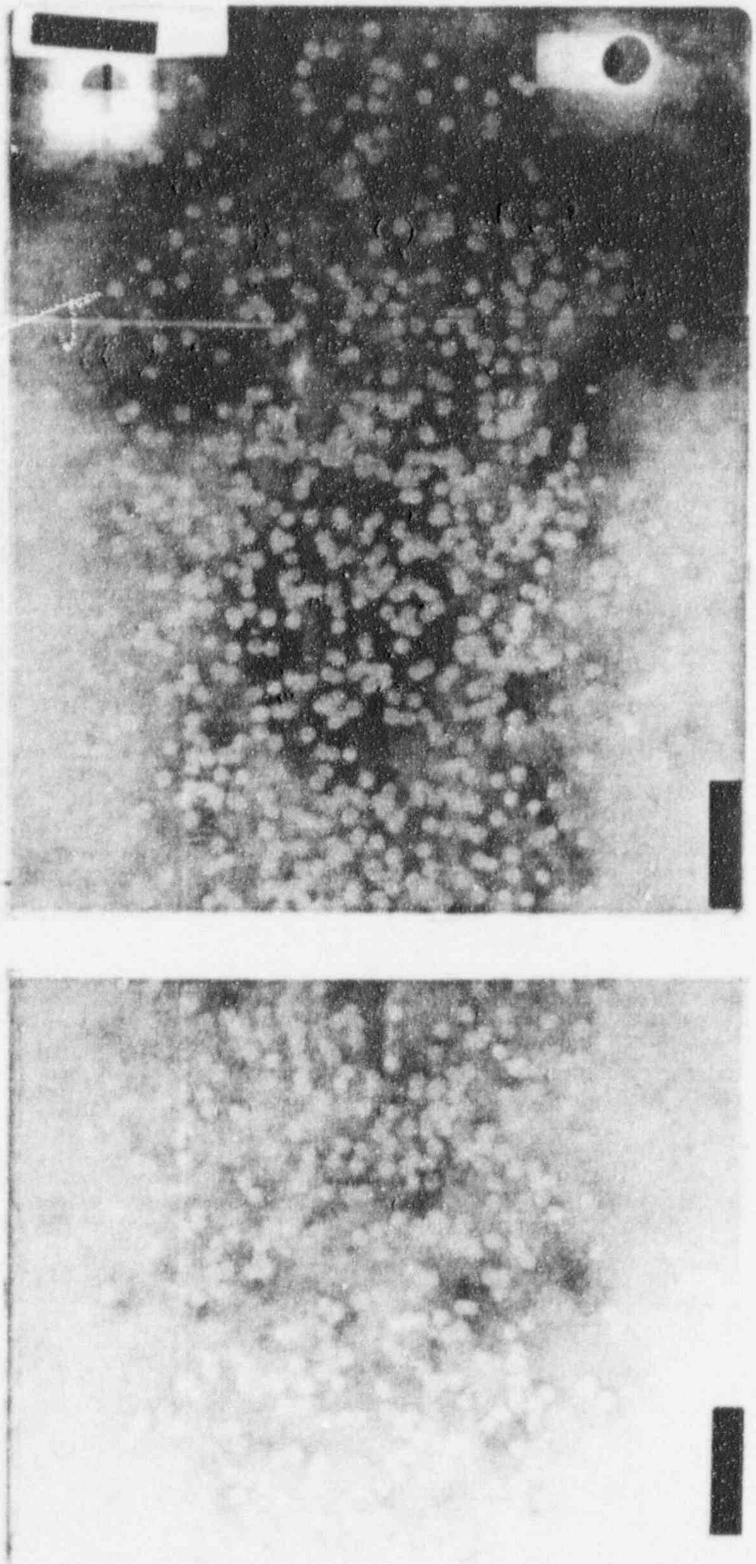


Figure 9(f).

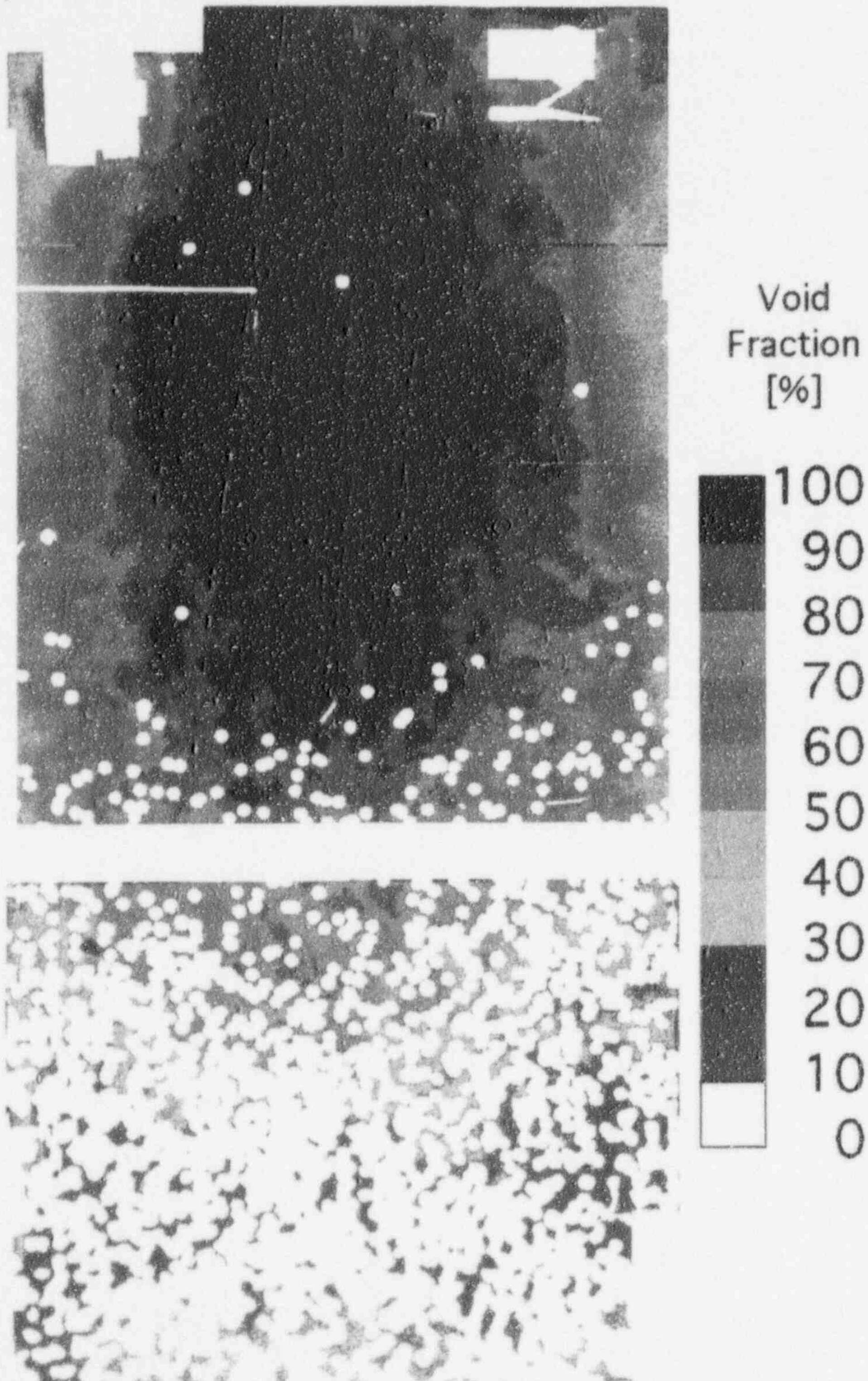


Figure 10(a). Quantified images of the radiographs in Figure 9. The (a) through (d) correspond to the (c) through (f) in Figure 9. Figure 10(a') represents the upper film shown in Figure 10(a), but scanned so as to emphasize the region of very high void fractions.

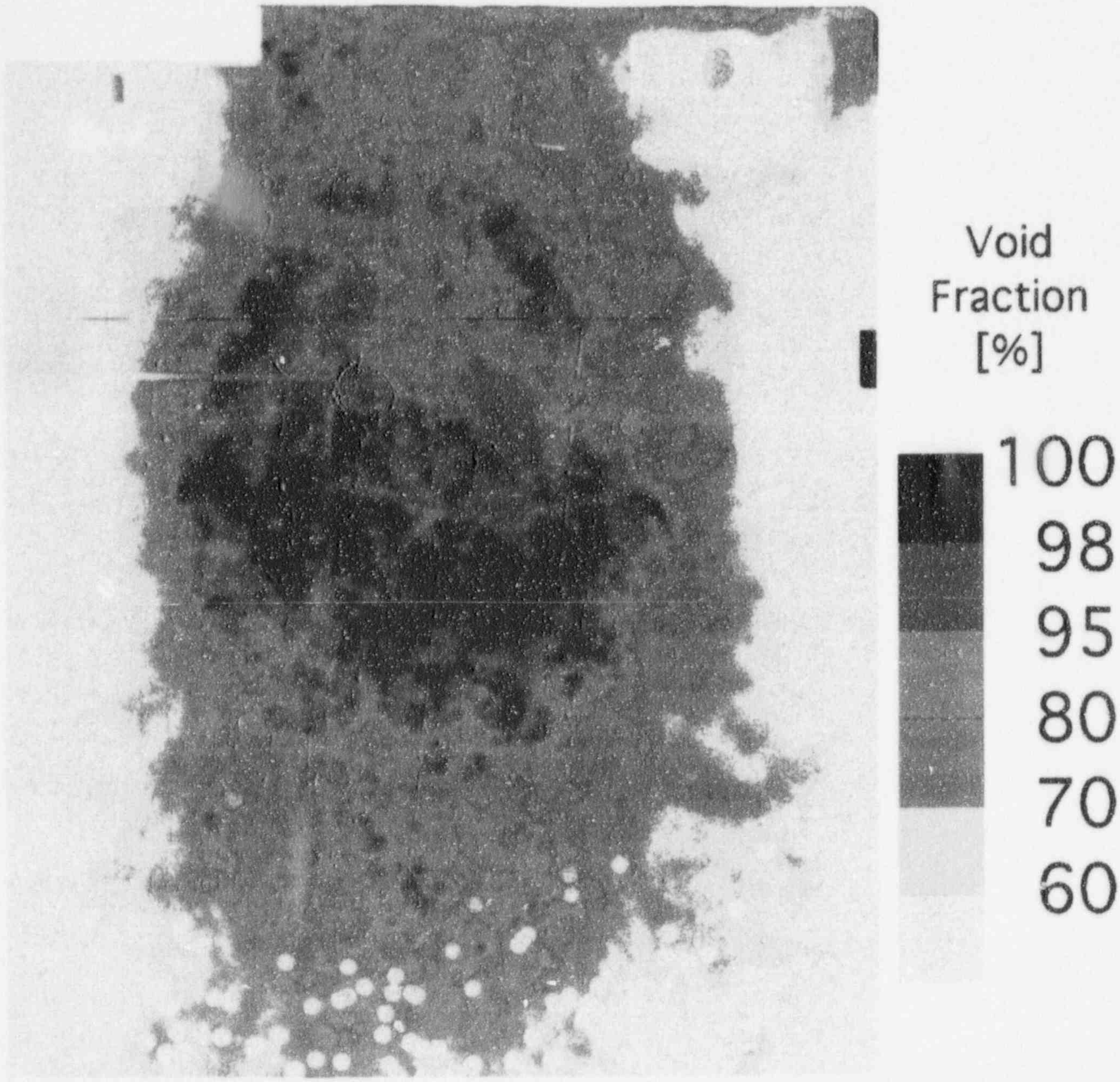


Figure 10(a')

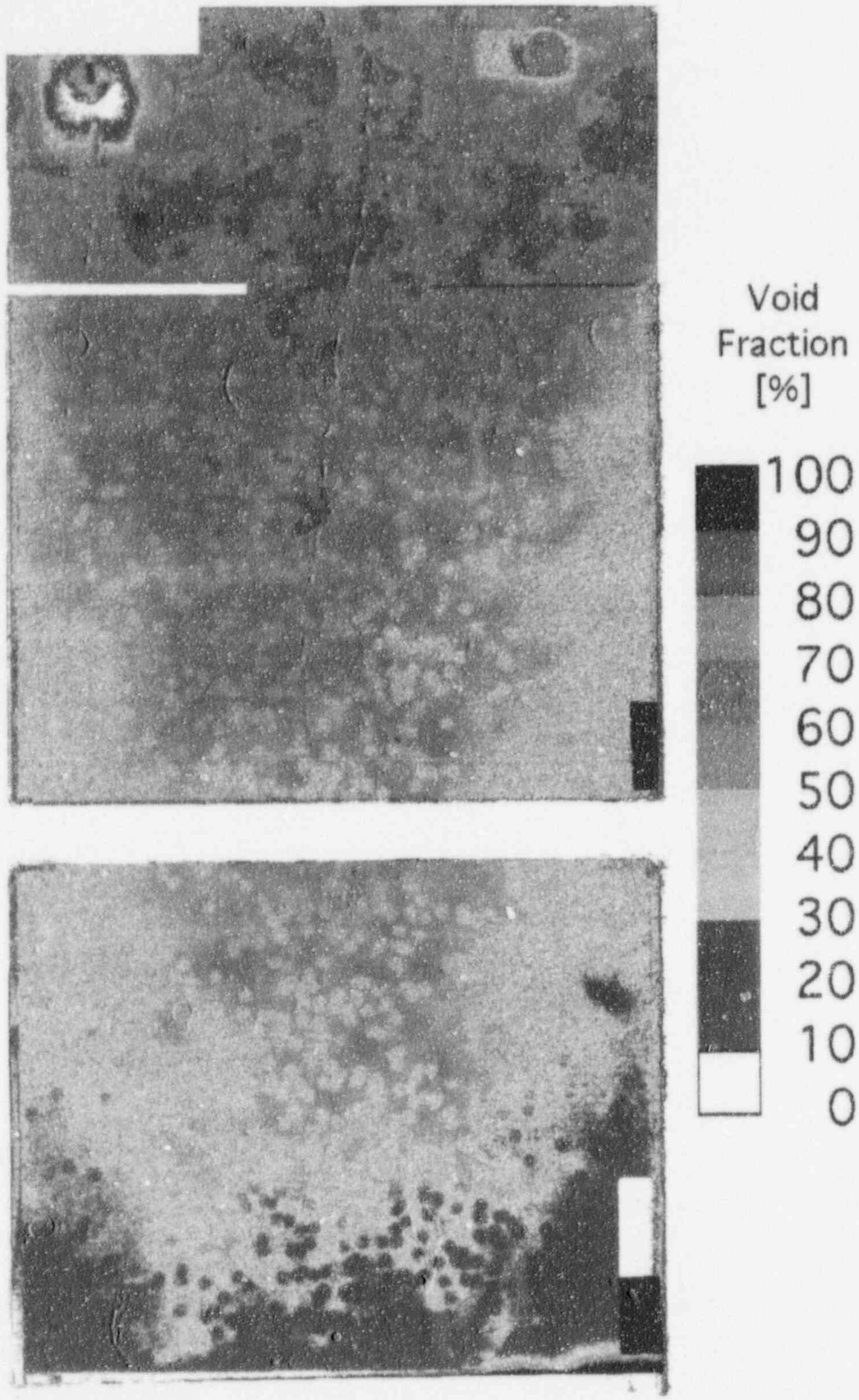


Figure 10(b).

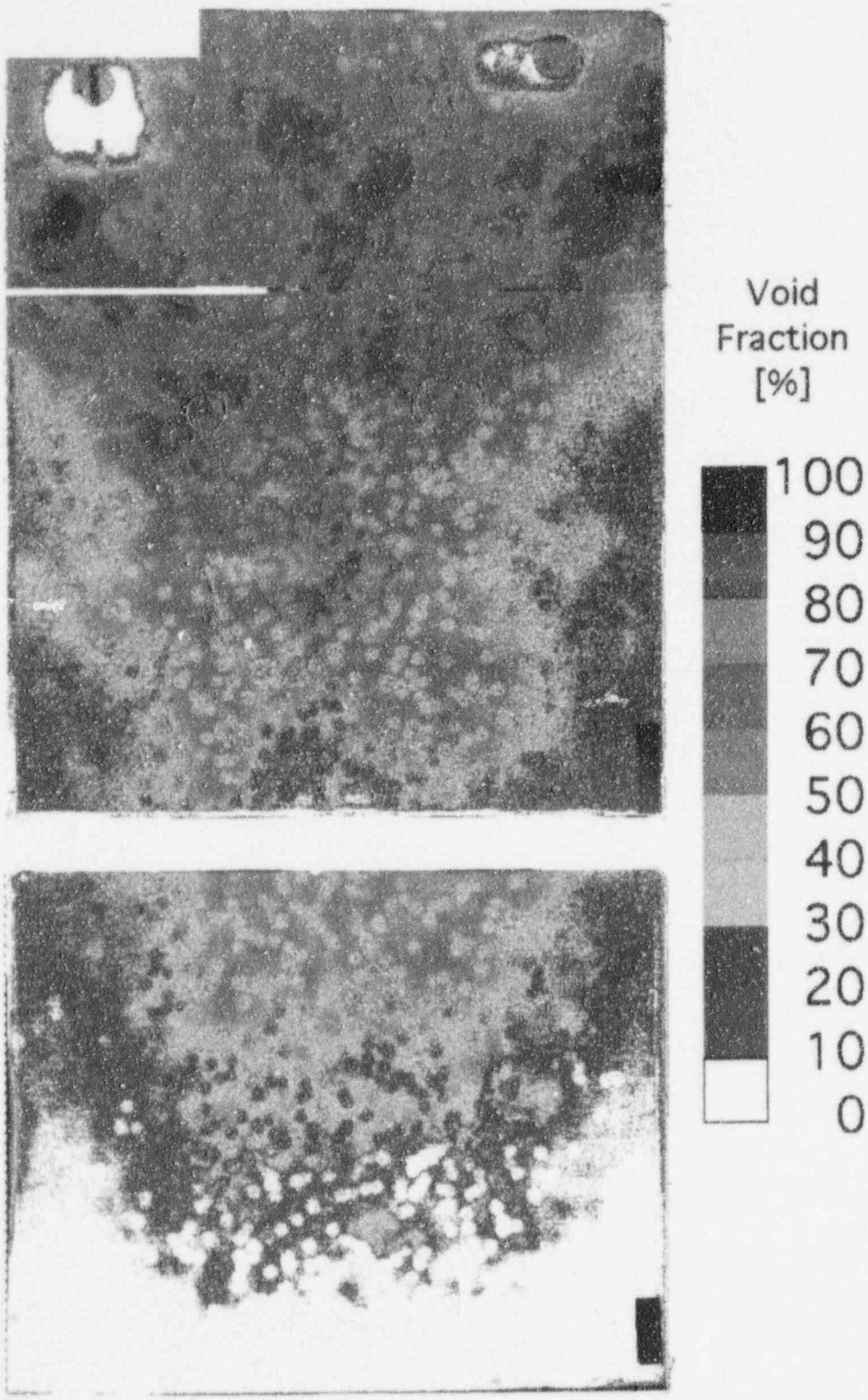


Figure 10(c).

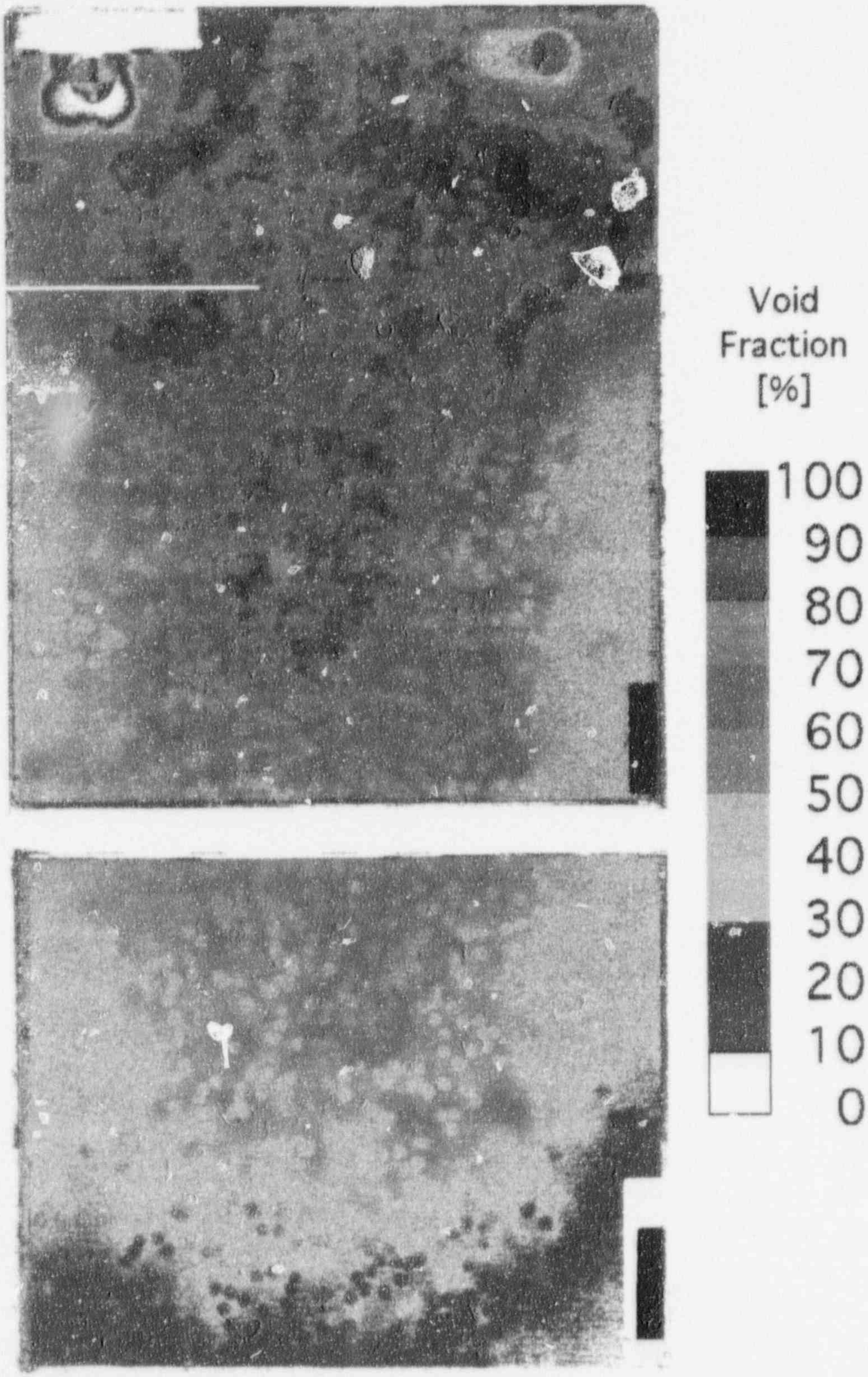


Figure 10(d).

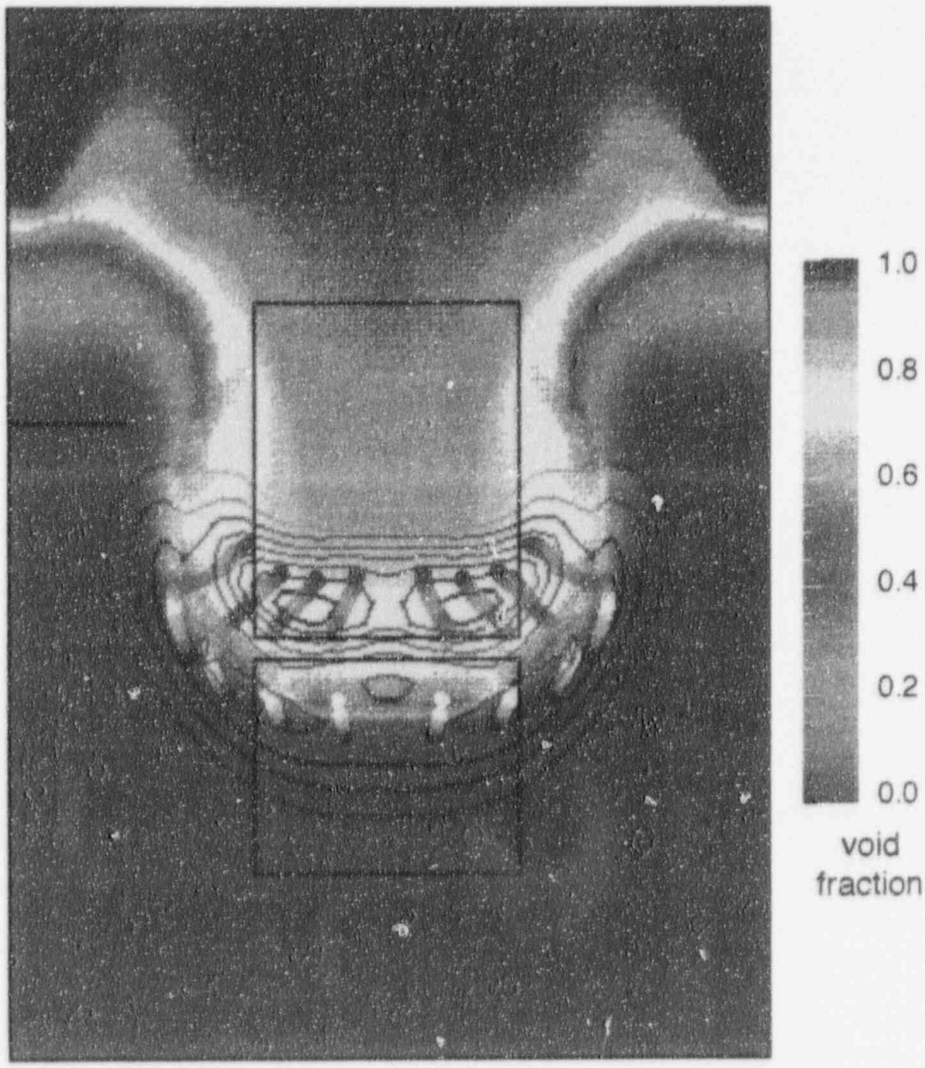


Figure 11(a). PM-ALPHA predictions of void fraction and particle volume fraction distributions at the time and conditions corresponding to radiographs of Figure 8. Predicted particle volume fraction contour lines are from 0.5% to 4% in 0.5% interval.

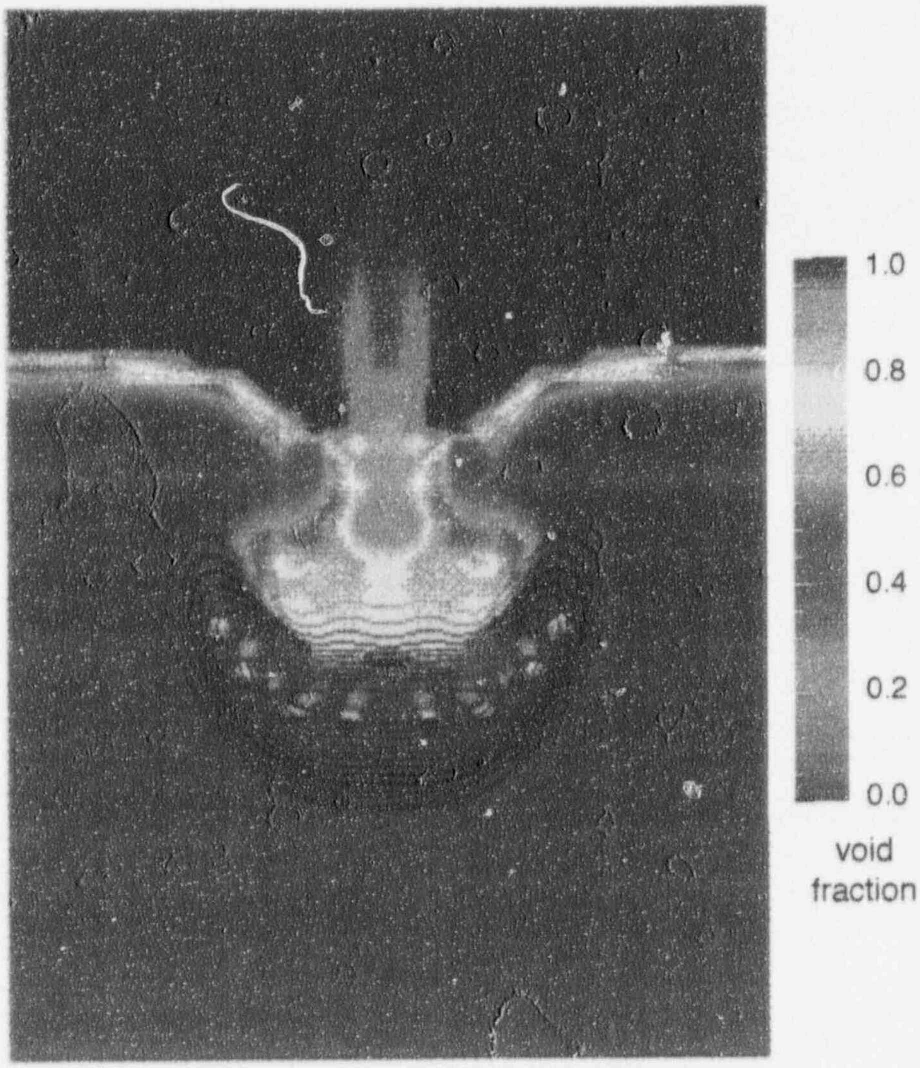


Figure 11(b). Predicted particle volume fraction contour lines are from 1% to 6% in 0.5% interval.

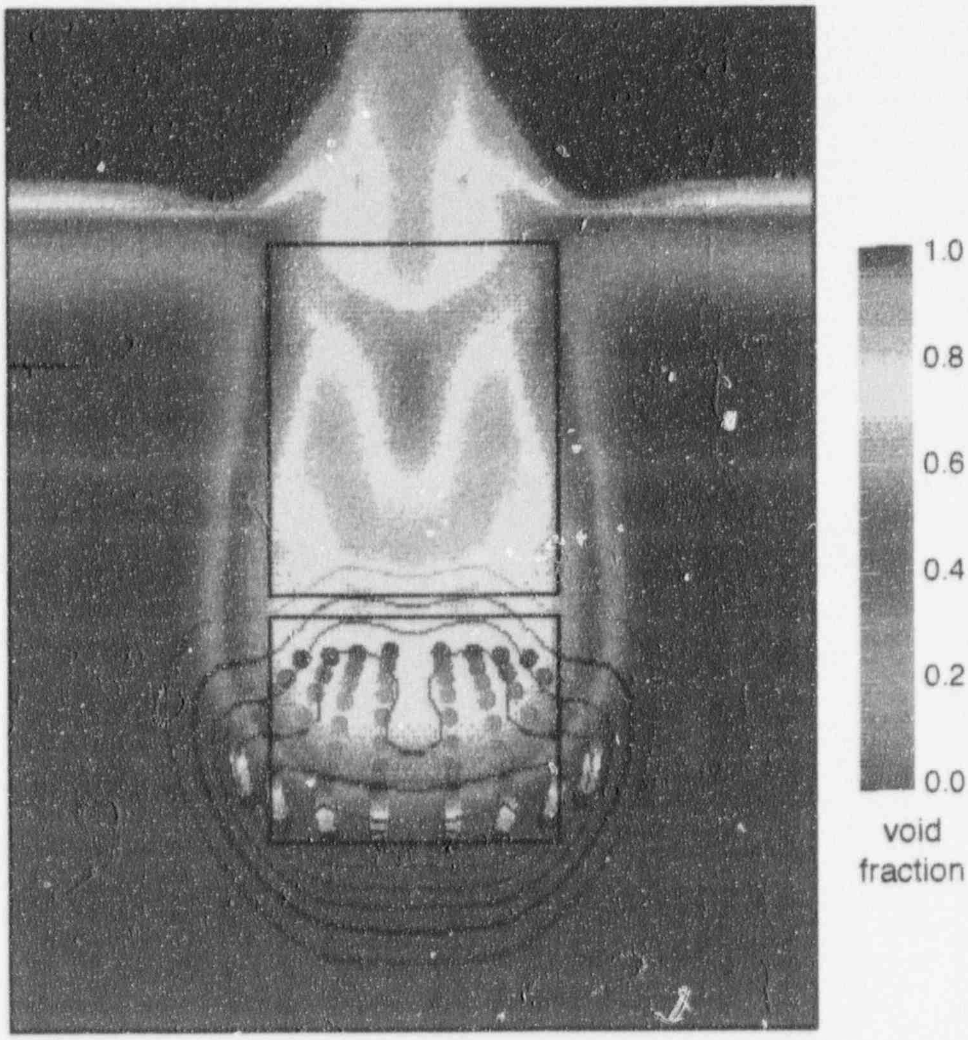


Figure 11(c). Predicted particle volume fraction contour lines are from 0.5% to 3% in 0.5% interval.

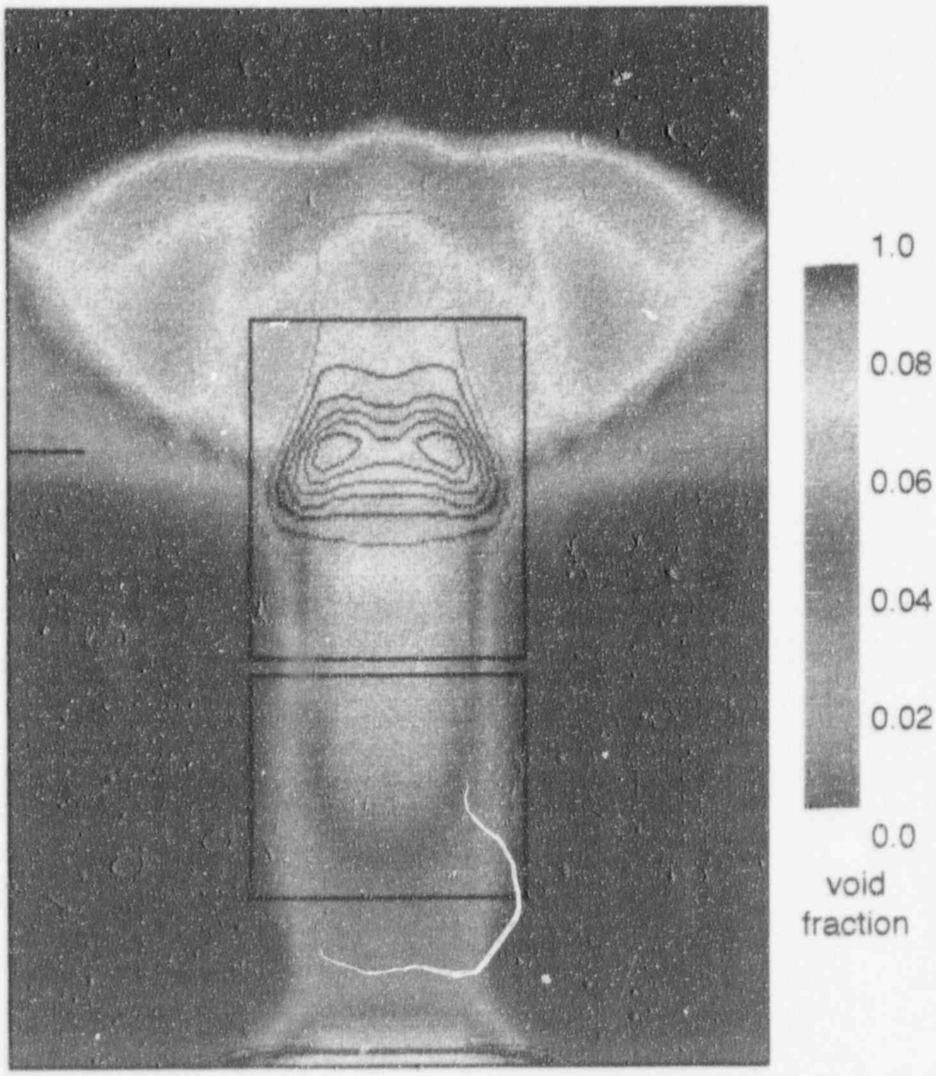


Figure 11(d). The measured (from the radiographs) particle volume fraction distributions were in the 0.6 to 1.1% range. Predicted particle volume fraction contour lines are from 0.6% to 1.8% in 0.2% interval.

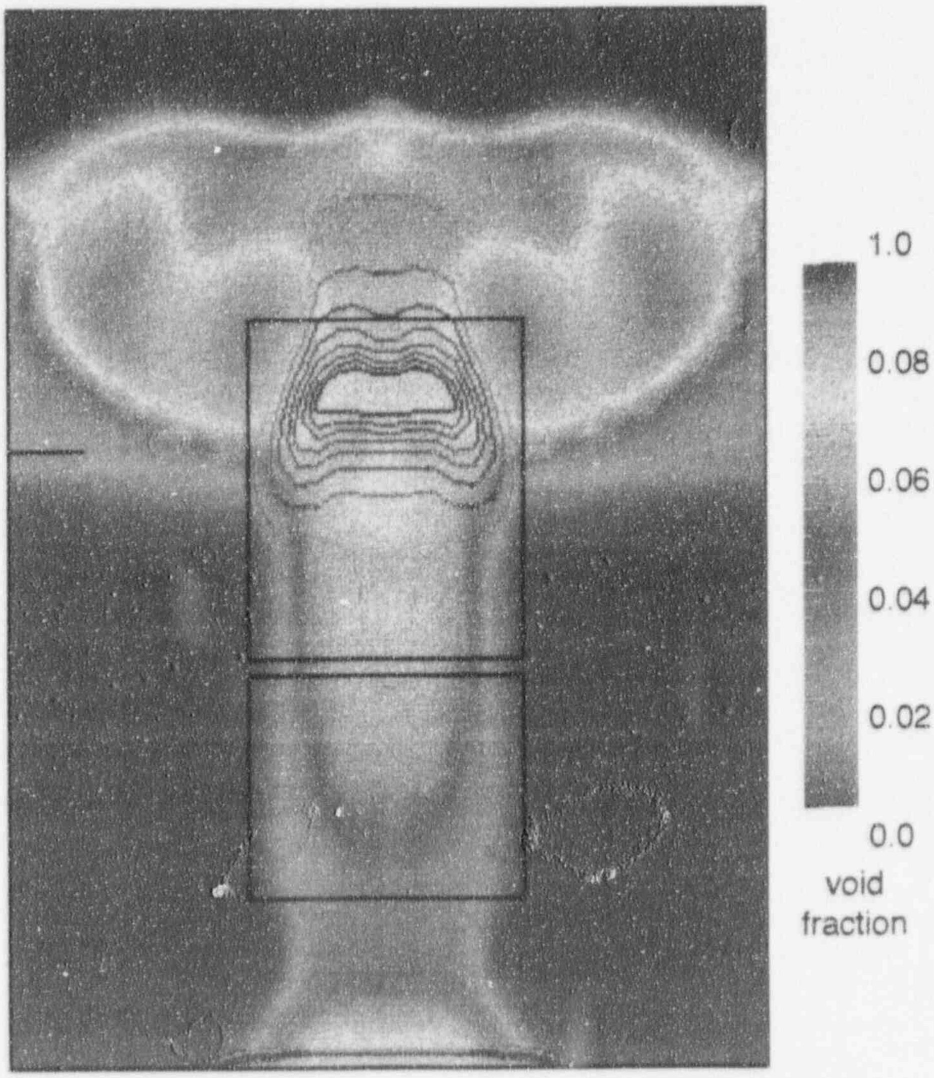


Figure 11(e). The measured (from the radiographs) particle volume fraction distributions were in the 0.6 to 1.1% range. Predicted particle volume fraction contour lines are from 0.6% to 2% in 0.2% interval.

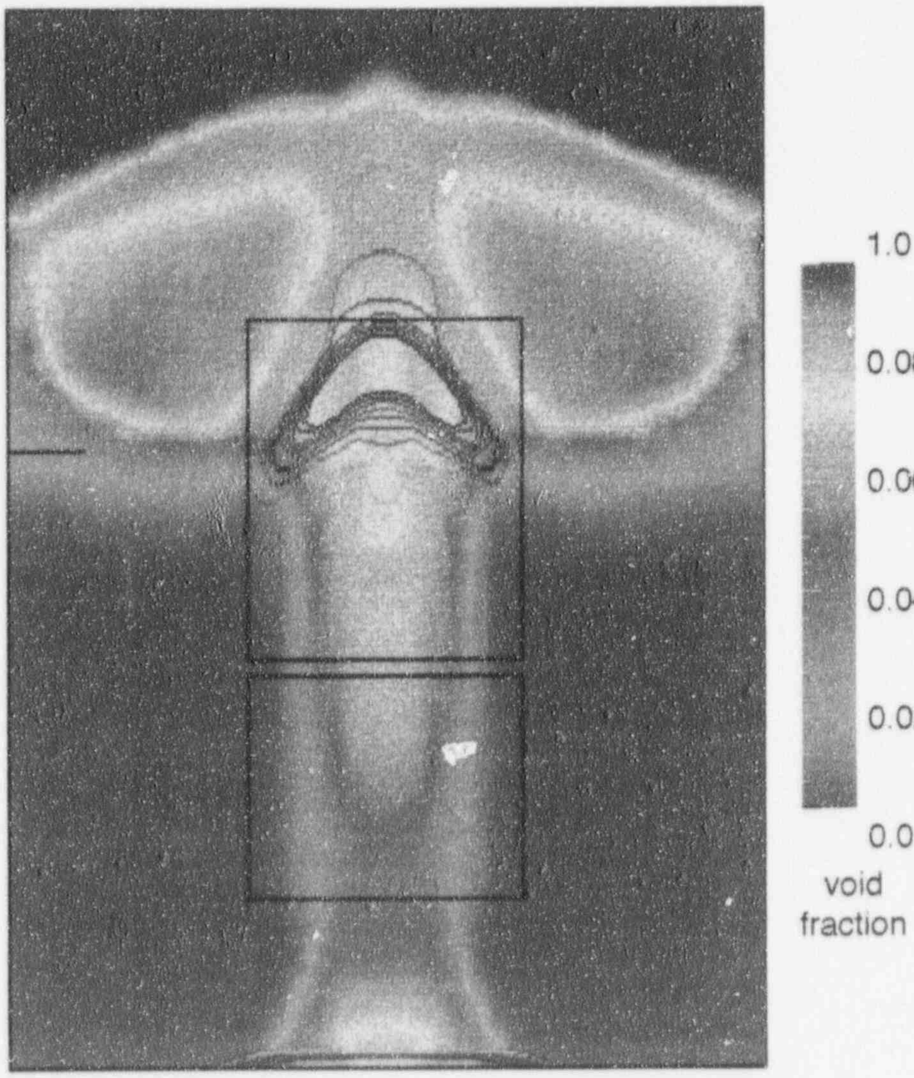


Figure 11(f). The measured (from the radiographs) particle volume fraction distributions were in the 0.6 to 1.1% range. Predicted particle volume fraction contour lines are from 0.6% to 2% in 0.2% interval.

ADDENDUM TO APPENDIX C

EXPERIMENTAL SIMULATION OF MICROINTERACTIONS IN LARGE SCALE EXPLOSIONS

by X. Chen, R. Luo, W.W. Yuen and T.G. Theofanous

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JAERI-Rokai Research Establishment, Japan, May 19–21, 1997

Experimental Simulation of Microinteractions in Large Scale Explosions

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Abstract: This paper presents data and analysis of recent experiments conducted in the SIGMA-2000 facility to simulate microinteractions in large scale explosions. Specifically, the fragmentation behavior of a high temperature molten steel drop under high pressure (beyond critical) conditions are investigated. The current data demonstrate, for the first time, the effect of high pressure in suppressing the thermal effect of fragmentation under supercritical conditions. The results support the microinteractions idea, and the ESPROSE.m prediction of fragmentation rate.

Keywords: Fragmentation, Molten Drops, Microinteractions

1. INTRODUCTION

Over the last few years, a systematic research program has been conducted at the Center of Risk Studies and Safety (CRSS) at the University of California at Santa Barbara (UCSB) to understand the fragmentation kinetics of molten drops under a sustained high pressure environment expected in a large scale explosion (Patel and Theofanous, 1981; Yuen et al., 1994; Chen et al., 1995) . Utilizing the SIGMA facilities (both the original SIGMA and the current SIGMA-2000), fragmentation behavior of gallium and mercury drop (under isothermal condition) and high temperature molten tin drops (between 670 and 1800 °C) at various shock pressures (between 68 and 200 bar) were observed. These experiments led to the microinteractions concept and the development of a computer code ESPROSE.m (Yuen and Theofanous, 1995). The computer code was successful in interpreting the steam explosion data generated at the KROTOS facility (Theofanous, et.al. 1995).

While the existing tin data were useful in illustrating the relative importance between thermal and hydrodynamic effects on fragmentation in a sustained high pressure environment, they are still short of the range of pressures and temperatures of interest (supercritical pressure and fuel temperature in excess of 2000 °C). An important objective of the current research program is thus to expand the data base to higher pressures and temperatures so that the physics of molten drop fragmentation and microinteractions can be further understood. The data can also be used to further develop the constitutive laws for microinteractions. Steel is selected for the present experiment because while it has about the same density as tin, it has a much higher thermal energy content (twice the heat capacity and four times the latent heat). Steel is thus a good material to examine the importance of thermal effects. To gain a perspective of the present experiment in relation to previous one, a summary of the thermal properties of steel, tin and gallium is presented in Table 1.

Table 1. Physical Properties of Steel, Tin and Gallium

Material	Melting point (°C)	Density (g/cm^3)	Surface tension (mN/m)	Specific heat (J/g· k)	Latent heat (J/g)
Steel	1450	6.87 (1700 °C)	1550	0.449	247
Tin	232	6.10 (1700 °C)	500	0.220	60.7
Gallium	29.8	6.07	660	0.371	29.7

2. EXPERIMENT

A schematic of the SIGMA-2000 facility is shown in Figure 1. A detailed description of the facility and the experimental procedure were given in previous publications and will not be repeated here. In the present set of experiments, a one-gram molten steel drop is released in subcooled water (20 °C). Six experiments, conducted at two shock pressures (68 bar and 265 bar), are presented. Two of the six experiments are conducted with a 6 % void fraction section both ahead of and behind the drop (by distributing plastic air bubbles in the water, the detail of this procedure is described in Chen (1996)) as shown in Figure 2. The collapsing of the void by the shock wave creates a high coolant velocity at the point of fuel-coolant interaction thus allowing to obtain considerably higher velocity for the same pressure behind the shock. Three drop temperatures (1550, 1620 and 1650 °C) are used to determine

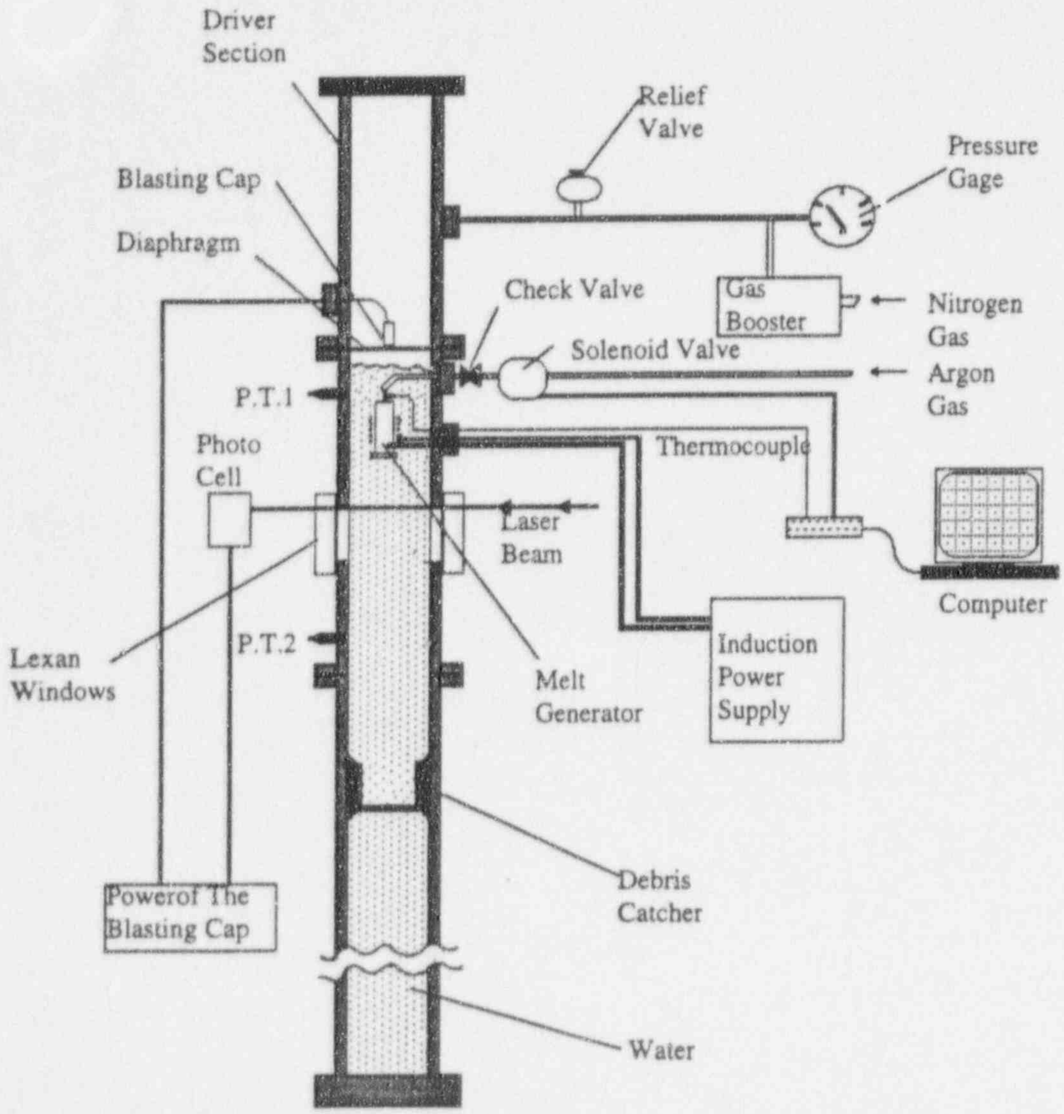

Figure 1. Schematic of SIGMA-2000 (not to scale).

the effect of melt superheat. Since the melting temperature of steel is high (1450°C), the current data illustrate both the effect of high temperature and low superheat (100, 170 and 200°C, respectively). This is in contrast to the previous high temperature tin data which were obtained with a superheat of over 1000 °C (Chen, et.al. 1995). The initial conditions for the tests are summarized in Table 2. Each run is labelled with an identification code. S-4-16.5 and S-4-16.5(0.06), for example, stand for runs with a shock pressure of 265 bar (4000 psi), steel temperature of 1650 C, fully liquid and two-phase coolant ahead of the drop respectively. The two similar tests S-4-16.5a and S-4-16.5b were conducted to demonstrate the repeatability of the experiment.

Table 2. Test Conditions of the Six Runs

Run ID	P(bar)/T(°C)	Void Fraction	Bo	X-ray Time (ms)
S-1-16.2	68/1620	0	34	
S-4-15.5	265/1550	0	300	0.32
S-4-16.5a	265/1650	0	300	
S-4-16.5b	265/1650	0	300	
S-4-15.5(0.06)	265/1550	0.06	1500	0.12
S-4-16.5(0.06)	265/1650	0.06	1500	0.22

The amplitude of the shock wave is measured by the pressure transducer of location PT2. Data from test S-4-16.5a are shown in Figure 3a and the expanded early transient is shown in Figure 3b. The transient data show that the pressure rose quickly to about 250 bar and remains relatively constant until the wave was reflected back from the bottom of the shock tube. There is a period of about 2 ms in which the pressure around the molten drop remained constant. The corresponding pressure data for a two-phase run (S-4-16.5(0.06)) are shown in Figures 4a and 4b. The shock pressure is oscillatory (due to collapsing of the void) and has a lower average pressure of $\sim$ 200 bar. The time period of constant pressure is reduced to about 1.5 ms due to reflection from the two-phase liquid interface below the observation widow.

The molten steel drop was released at 5 cm above the observation window prior to the arrival of the shock. As described previously, the timing of the drop release and the initiation of the shock was synchronized such that the interaction between the shock and the drop occurred at the observation window. The fragmentation and subsequent entrainment were then recorded by a high speed movie camera at 50,000 frames per second. To gain a better perspective of the initial condition of the molten drop, a drop test with the same drop temperature and water temperature was conducted inside the shock tube without the shock. The image of the drop and the surrounding vapor film was recorded by a video camera and is shown in Figure 5. The color image clearly shows that the ellipsoidal red-hot drop was surrounded by a large vapor bubble as it fell through the subcooled water. Since flash X-ray is used to capture the fragmentation behavior during the fuel coolant interaction, an X-ray image of this unbroken molten drop, together with a contour map of the 2-D projected mass distribution, are also shown in Figure 5. The drop falling speed, measured from the video tape, was estimated to be about 0.7 m/s at the observation window. For the shock amplitude of 68 and 265 bar, the induced water velocities were 4.5 and 15 m/s respectively. For the two-phase run at 265 bar, the water velocity is about 38 m/s. The corresponding Bond numbers for all experiments are also shown in Table 2.

The high speed movie images for all six experiments are presented in Figures 6a through 6f. In run S-4-16.5a (Figure 6c), for example, the first image shows a free falling molten steel drop surrounded by a bubble before the arrival of shock wave. The white spot at the center of the image was caused by the flashlight as it was projected in the normal direction to the vapor bubble surface and passed straight through. The shrinking of the bubble which is apparent in the second frame indicates the arrival of the shock wave. The bubble was completely collapsed 40 μs after the passage of the shock wave, and left a tiny non-condensible gas bubble on top. At 80 μs (frame 5), the initial ellipsoid drop expanded both in and transverse to the flow direction and formed a much bigger mass cloud. As time passed, this cloud continued to asymmetrically expand. Crests appeared at the upper right corner of the cloud at 0.22 ms. After 0.3 ms, the cloud elongated faster than it expanded in the horizontal direction and eventually formed a mushroom shape cloud. The cap of the cloud had a wavy surface and continued to expand in all directions, while the diameter of the stem became smaller as it moved with water. At 1.32 ms, the lower part of the cloud moved out of the window. Except for the difference in timing and the detail of the "cloud" generated by the interaction, all experiments follow the same qualitative behavior. A direct comparison between runs with different steel temperatures (for example, S-4-15.5 and S-4-16.5) shows that even with a temperature difference of only 100 °C, the volume of the "cloud" is larger for the high temperature case. The remarkable similarity between Figures 6c and 6d confirms the repeatability of the experiment.

To gain a perspective directly on the fragmentation kinetics, flash X-ray images at selected time of the interaction are generated for three runs. The timing of these X-rays is shown in Table 2. Since water and steam are essentially

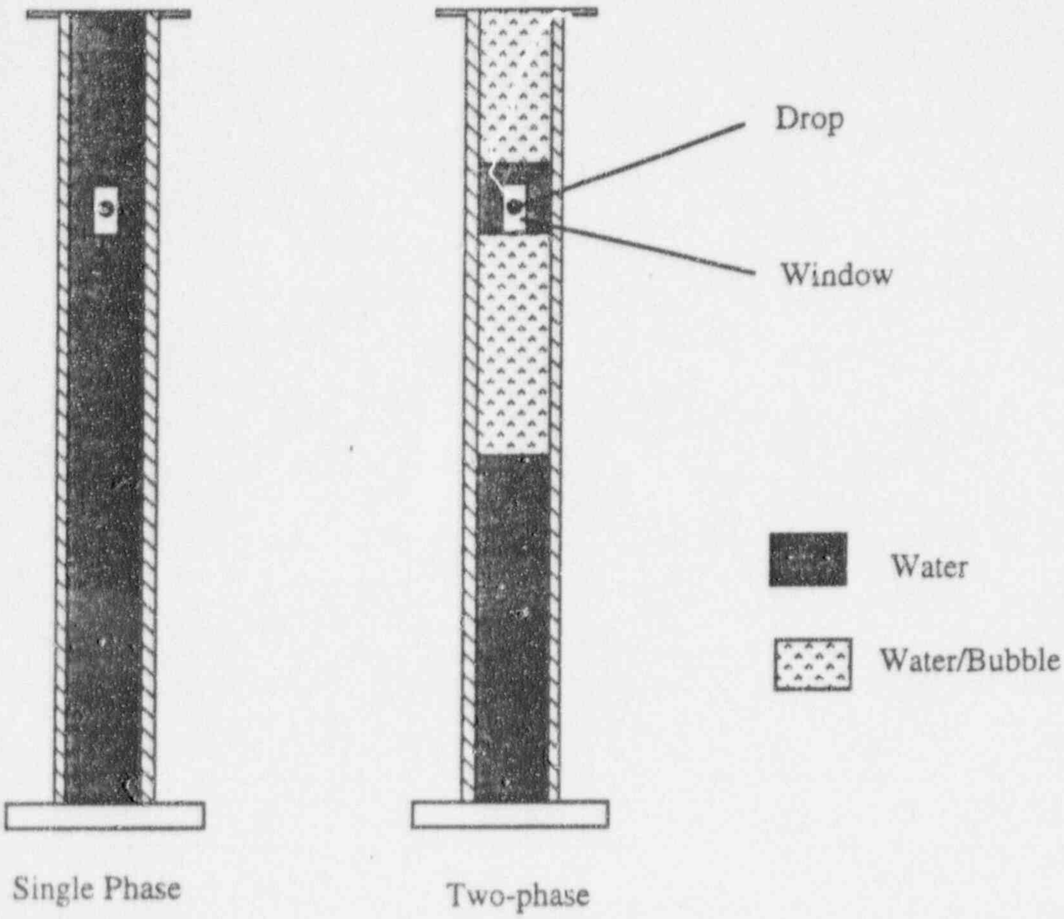

Figure 2. Schematic of the void fraction distribution for the single-phase and two-phase runs (the figure is in scale in the vertical direction only).

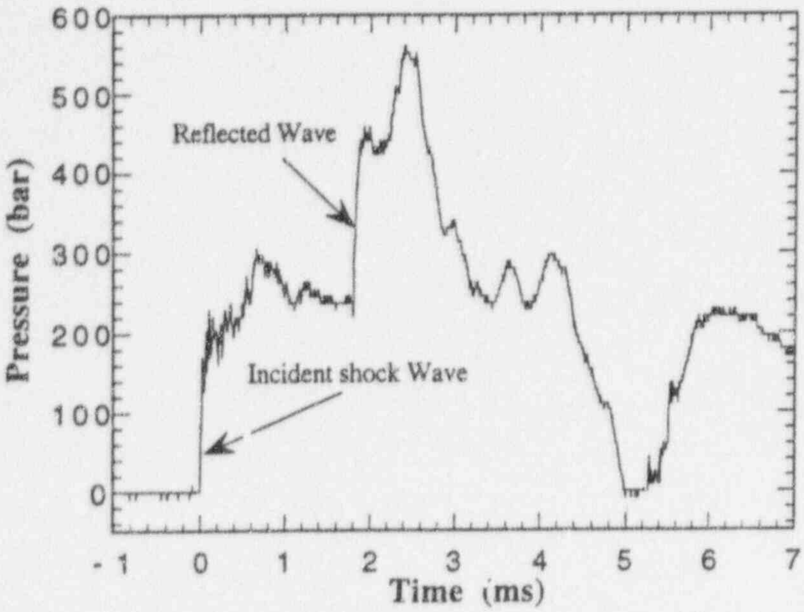

Figure 3a. Pressure transient as recorded by PT-2 in run S-4-16.5a.

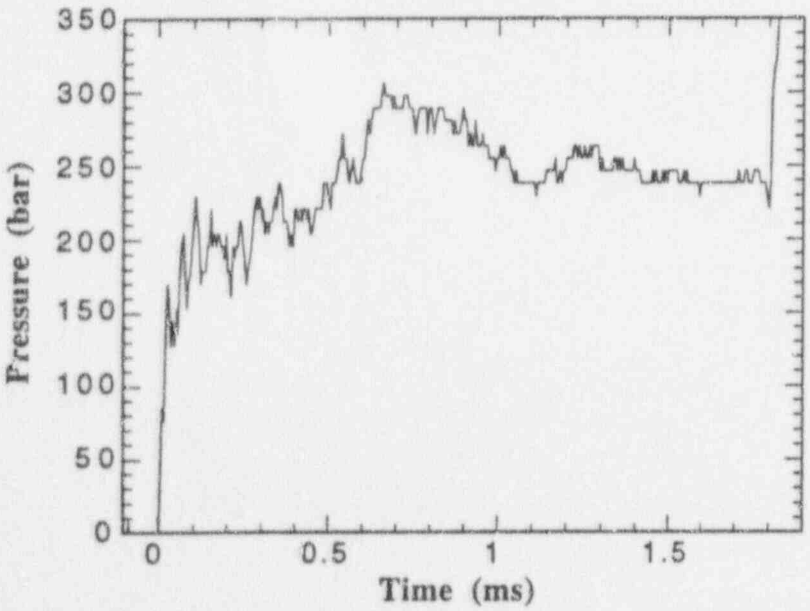

Figure 3b. Expanded pressure transient during the first two milliseconds of interactions in run S-4-16.5a.

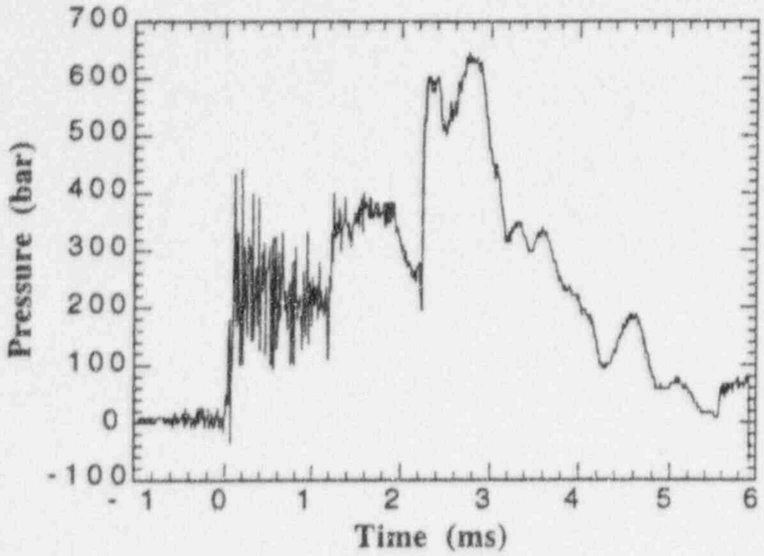

Figure 4a. Pressure transient as recorded by PT-2 in run S-4-16.5(0.06)..

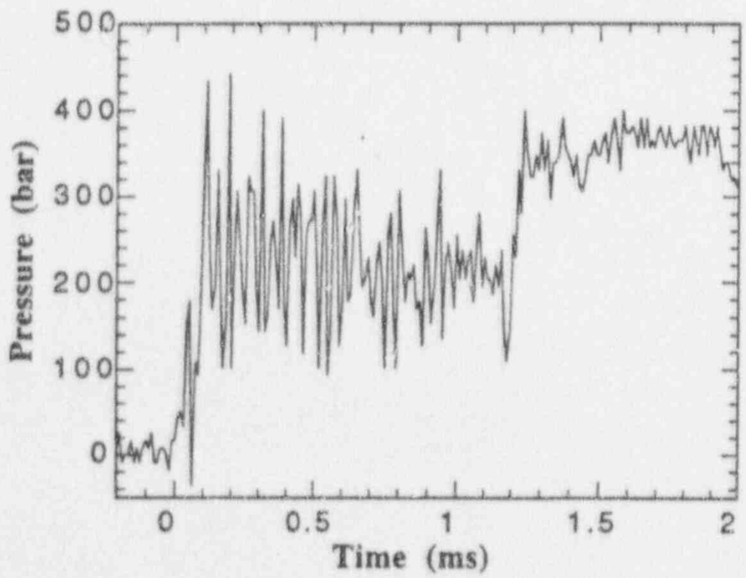

Figure 4b. Expanded pressure transient during the first two milliseconds of interactions in run S-4-16.5(0.06).

transparent to the X-ray (relative to the high absorptivity of steel), these images give quantitative information about the fragmented steel. These images and the deduced mass fraction contours, together with the corresponding movie image are shown in Figures 7a through 7c. In contrast to the original symmetrical and smooth drop images as shown in Figure 5, the region of high mass concentration is highly distorted and "flattened" due to its interaction with the shock. An interesting feature of the fragmentation, relative to the volume of the "cloud" deduced from the movie image, can be observed from the high temperature two-phase run, S-4-16.5(0.06), as shown in Figure 7c. The region of high mass concentration (the drop) is much smaller than the observed "cloud". The debris seem to be concentrated along narrow strips appearing to shed off the edge of the parent drop in a manner reminiscent of the previous mercury runs.

3. DATA ANALYSIS

Debris are collected from each experiment and analyzed. Data for the four high pressure tests are shown in Figure 8. The corresponding size distributions and the size distribution of debris collected for the low pressure run are shown in Figures 9. For the two single phase runs, 70% (in mass) had sizes greater than 1 mm (in fact, for S-14-16.5, this mass was a big porous particle with diameter of about 1.5 cm), 20% belonged to a group of millimeter-size debris, fine debris with diameter less than 300 μm was 3.5%, and the remaining 6.5% of the mass was lost during retrieving and handling which was believed to be the fine debris. The two phase runs, on the other hand, had relatively uniform debris size distributions. Significant fractions of the debris ($\sim$ 30% and 50%) had particle sizes of less than 0.1 mm. These results give clear evidence of a stronger fuel coolant interaction for the two phase runs. Some typical SEM pictures of the debris are presented in Figures 10a to 10d. In each figure, SEM pictures of a typical "large" particle and "small" particle are shown to illustrate the debries morphology and its dependence on the test parameters. While all debris show a highly convoluted and porous structure with drawn, sharp edges, a comparison between Figures 10a and 10b shows that debris from the high temperature run (S-4-16.5a) has a "finer" structure. This can be attributed to the faster freezing in the low temperature run (S-4-15.5) which preserves more the "larger" debris strtucture. A comparison between Figures 10b and 10c shows the effect of the high liquid velocity associated with the two-phase run. The stronger fuel coolant interaction leads to a much more convoluted and porous structure than the single phase case.

The debris collected from the low pressure test (S-1-16) shows a striking dissimilarity from that of the high pressure tests. Approximately 70% of the mass was retrieved and as shown in Figure 9, all debris have size less than 1.0 mm with 12% less than 300 μm. The SEM images in Figure 10d show millimeter-size debris consisted of highly porous particles and flakes.

To separate debris mass from the drop mass in the X-ray images, we first use a procedure developed in a previous work (Chen et.al., 1995) which identifies a "boundary" between the still-coherent drop mass and the surrounding debris by a sudden drop-off in the mass thickness. For the two-phase runs, this leads to a "cutoff" thickness of 0.2 mm for the drop. The accuracy of this "cutoff" thickness is consistent with the mass distribution curves generated from the contour plots shown in Figure 11. The mass thickness of 0.2 mm corresponds approximately to a point of inflection which implies a change in the mass concentration. Based on this "cutoff" thickness, the debris mass fraction for the two phase runs are determined to be 22 % (for S-4-15.5(0.06)) and 48 % (for S-4-16.5(0.06)). The same procedure, however, yields inconsistent cutoff mass thickness for the single phase run. The determination of debris mass for S-4-15.5 is therefore not presented in view of this uncertainty.

To understand the implication of the two-phase fragmentation data on the microinteration model, we ran the ESPROSE.m code to simulate the two phase run using the same fragmentation constant ($\beta_f = 9$) and entrainment factor ($f_e = 7$) determined from the fragmentation data of mercury, gallium and tin (Chen et.al., 1995). A thermal exhancement factor (γ_t) of 3 is required to match the fragmentation data. The transient debris mass fraction and mixing volume predicted by ESPROSE.m, together with the two data points, are shown in Figures 12.

4. CONCLUSION

This paper presents data and analysis which confirm results from our previous investigation that at supercritical condition, the thermal effect on fragmentation is highly suppressed. A hydrodynamic-based fragmentation model is sufficient to characterize the fragmentation behavior. The microinteractions idea is qualitatively confirmed, but more data and analysis are needed to make the next quantitative step.

5. ACKNOWLEDGMENTS

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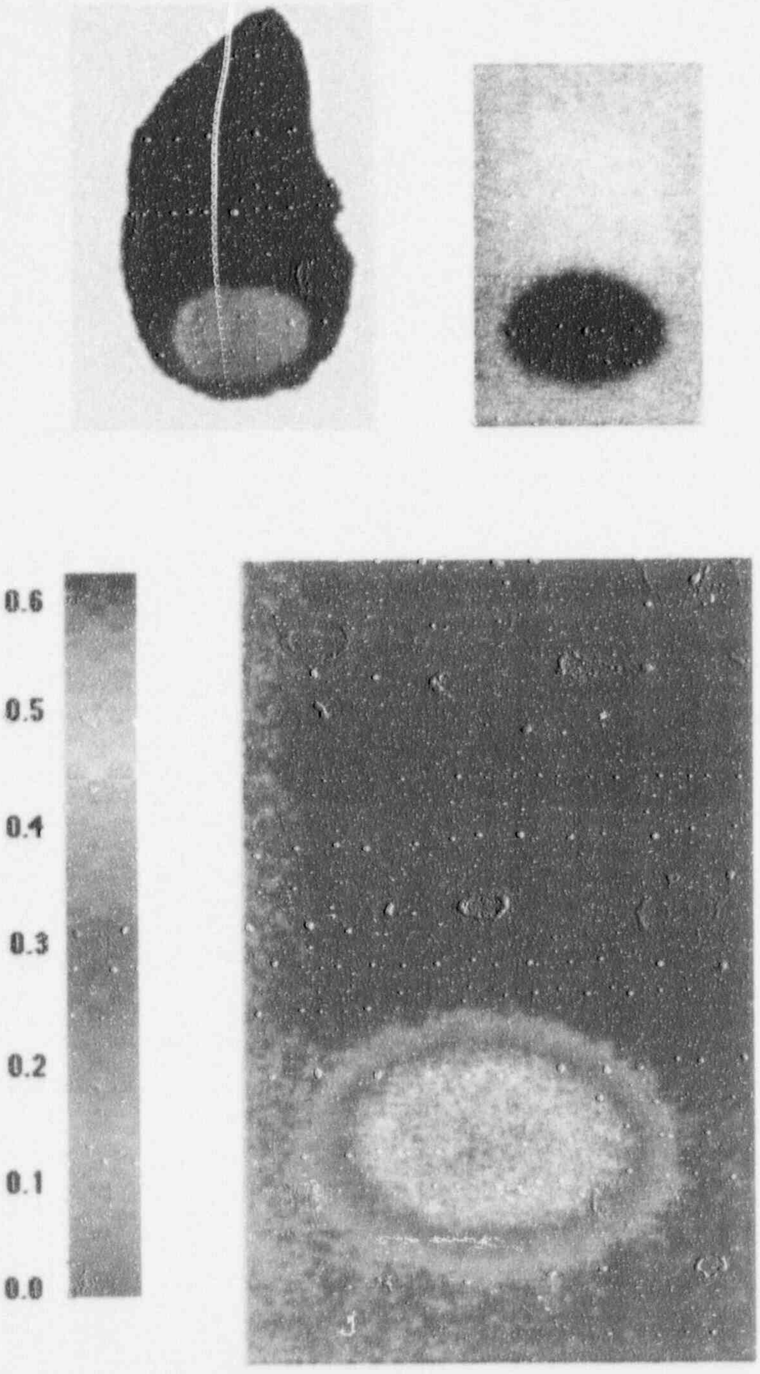


Figure 5. The visible image, X-ray image and mass distributiion contours of a molten steel drop (1650 °C) in 20 °C water from a drop test run.

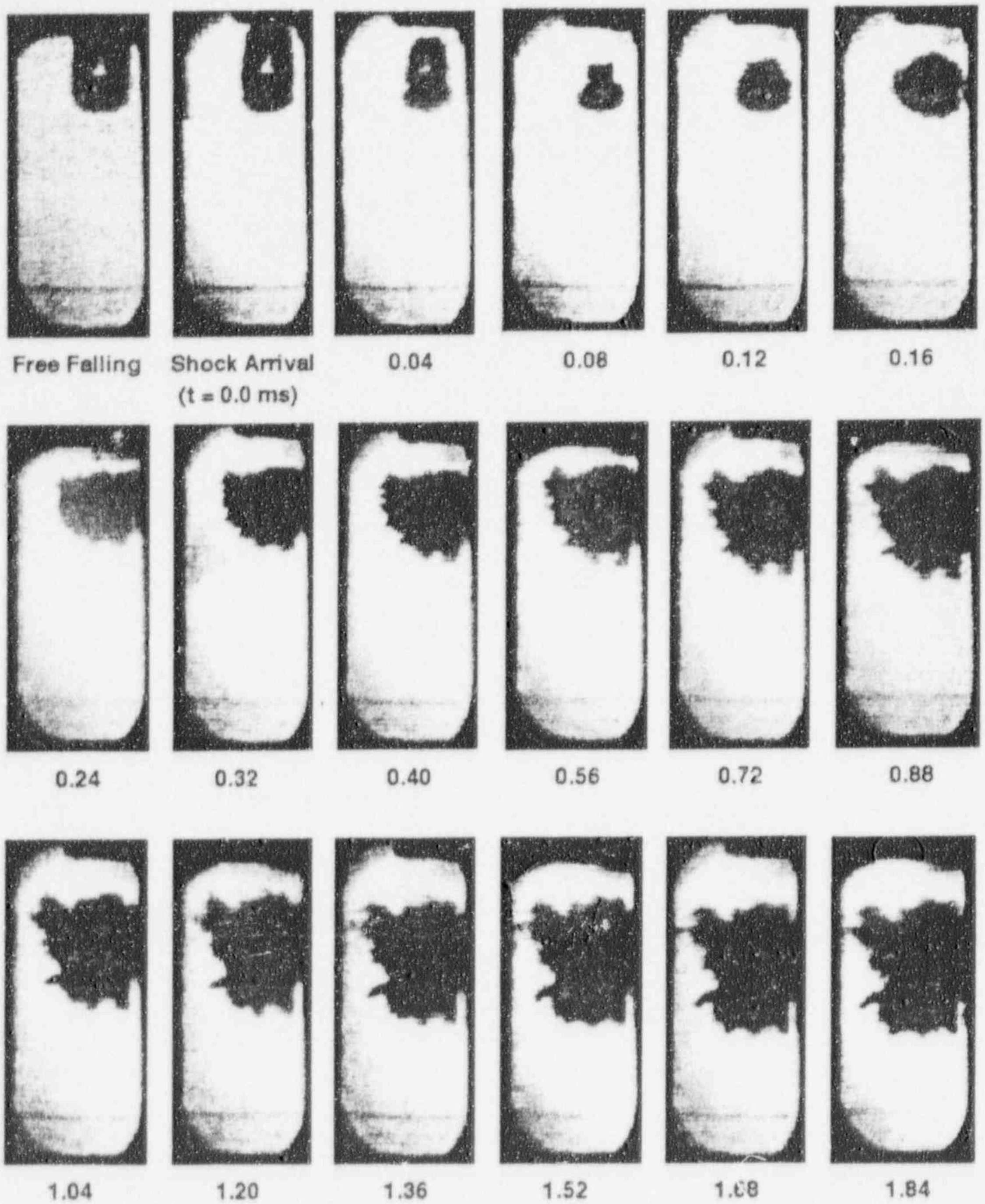


Figure 6a. High speed movie images of Run S-1-16.2.

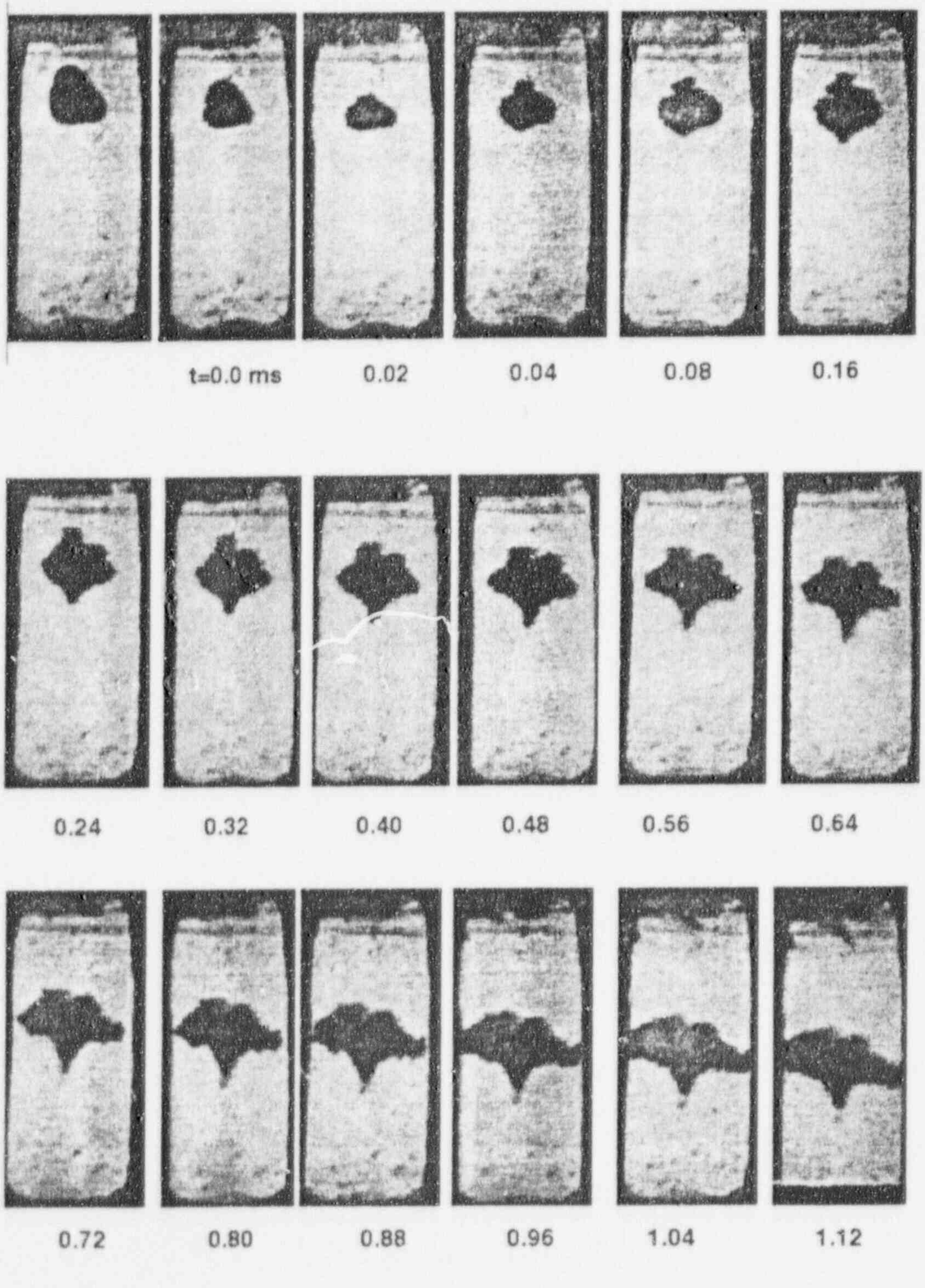


Figure 6b. High speed movie images of Run S-4-15.5.

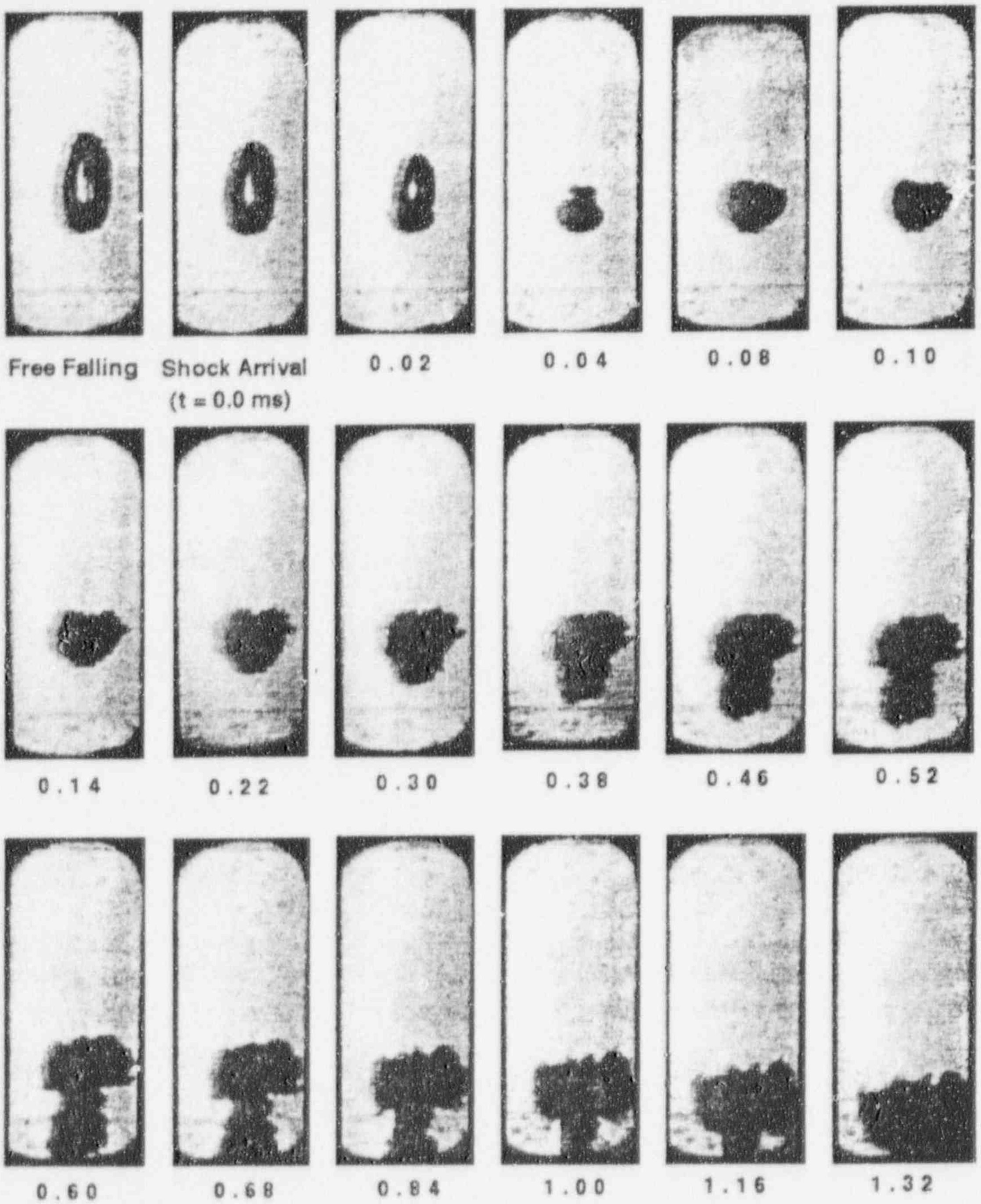


Figure 6c. High speed movie images of Run S-4-16.5a.

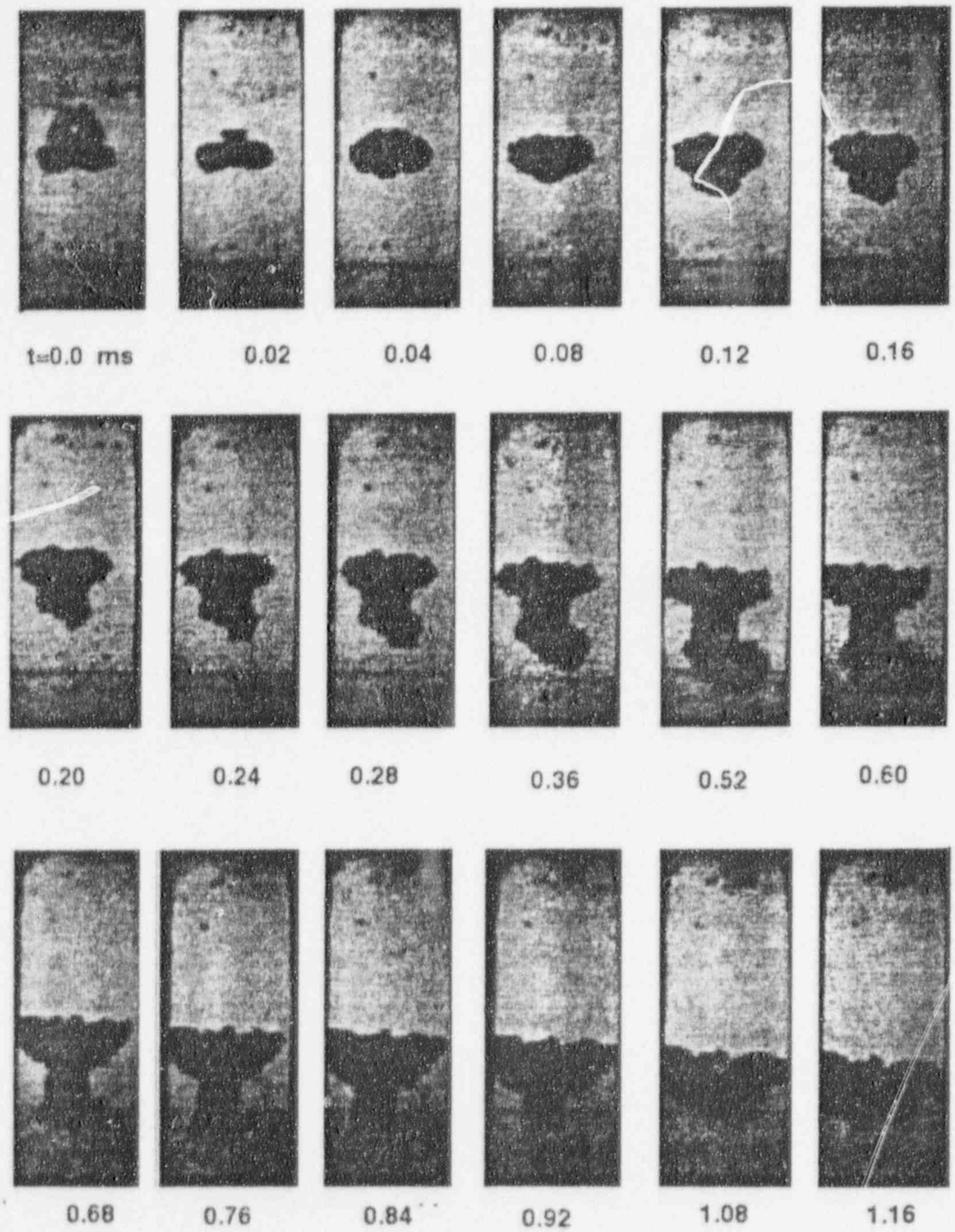


Figure 6d. High speed movie images of Run S-4-16.5b.

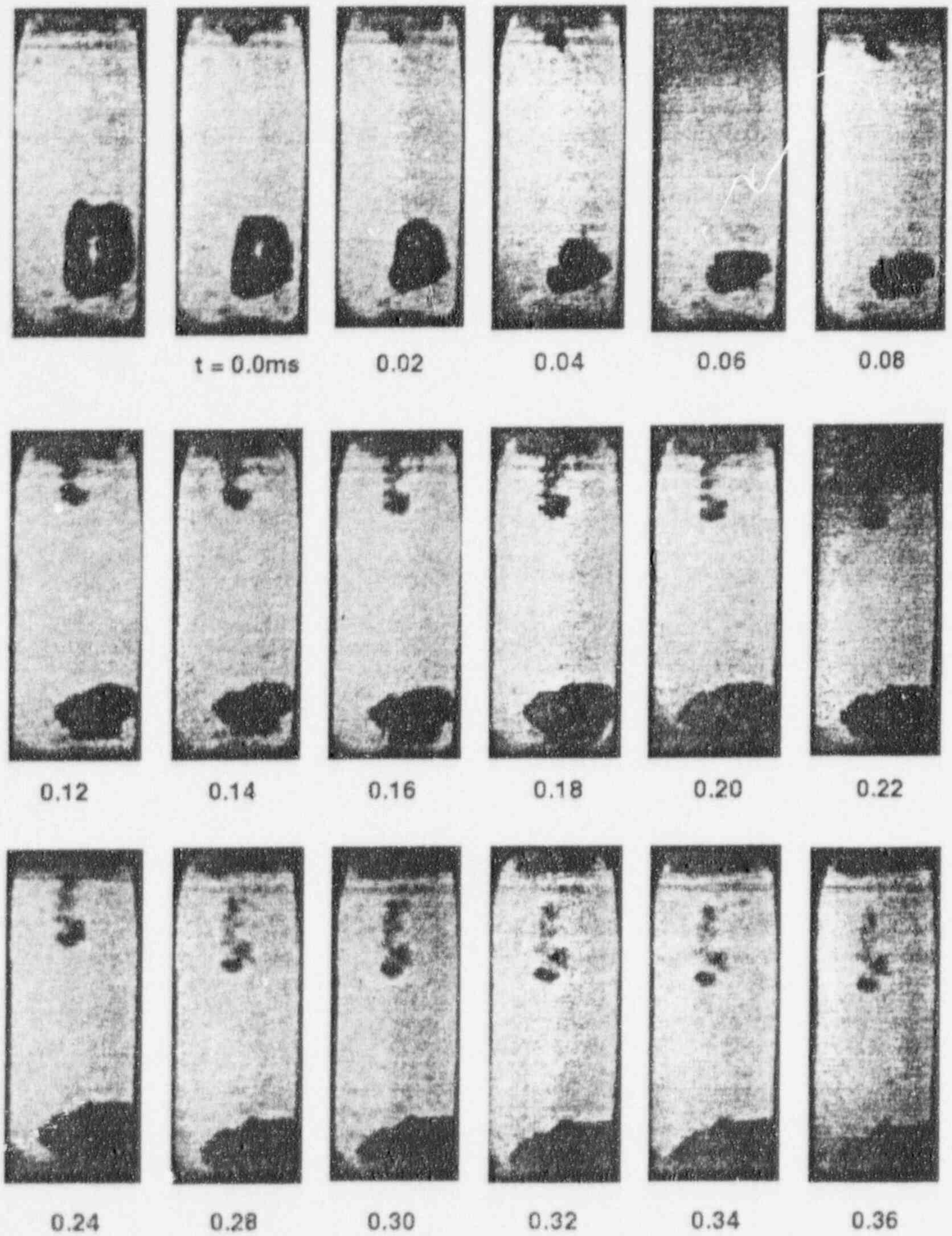


Figure 6e. High speed movie images of Run S-4-15.5(0.06).

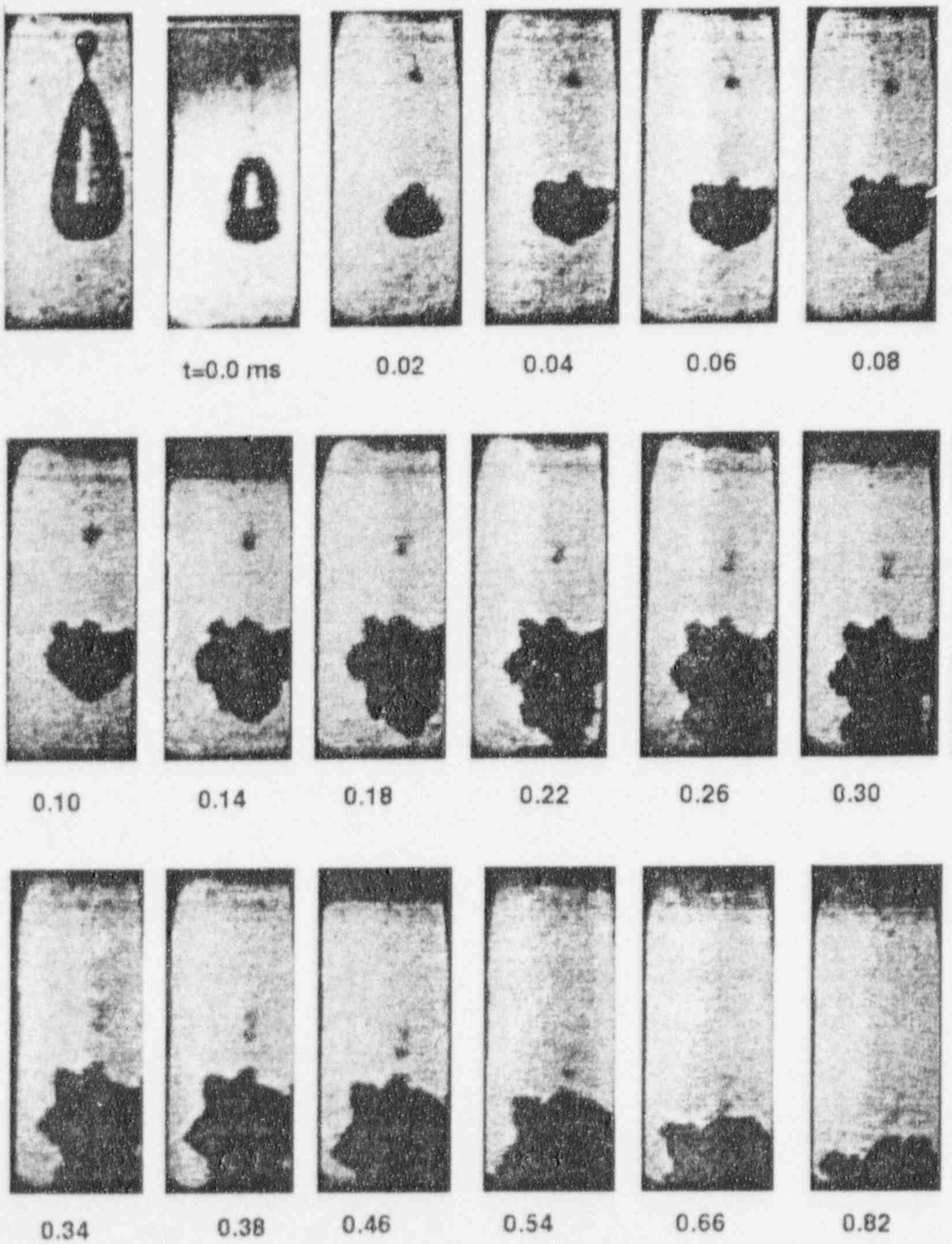


Figure 6f. High speed movie images of Run S-4-16.5(0.06).

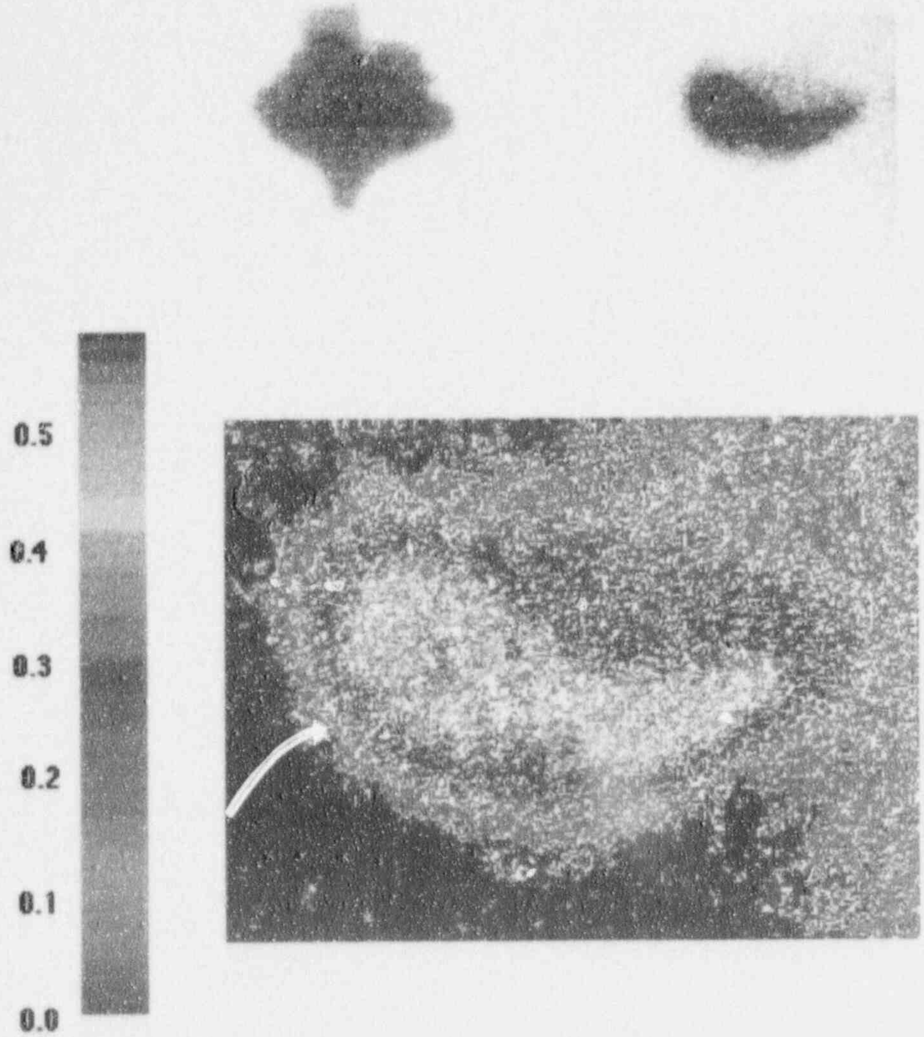


Figure 7a. Visible image, X-ray image and the mass fraction contours for a fragmenting drop at 0.32 ms after shock arrival for Run S-4-15.5.

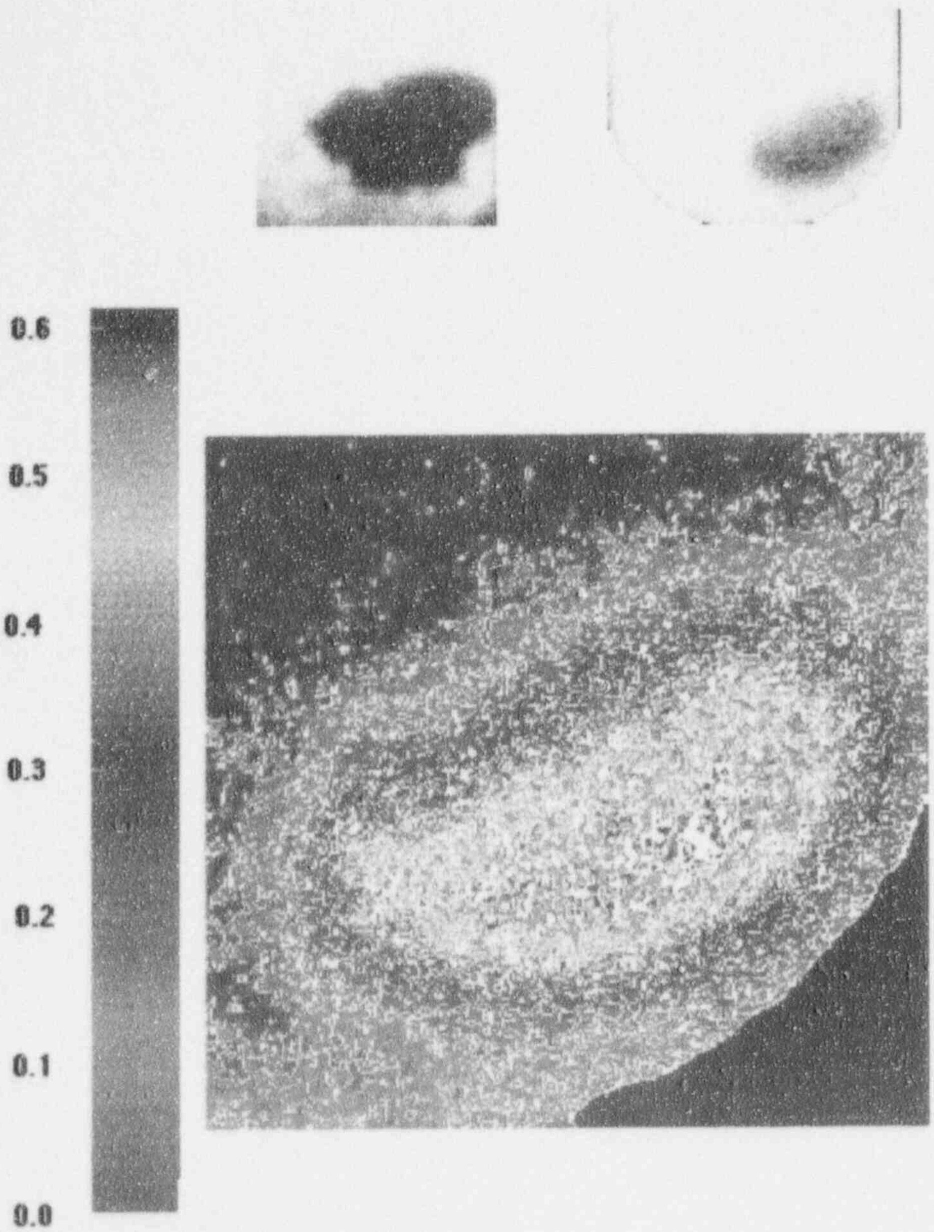


Figure 7b. Visible image, X-ray image and the mass fraction contours for a fragmenting drop at 0.12 ms after shock arrival for Run S-4-15.5(0.06).

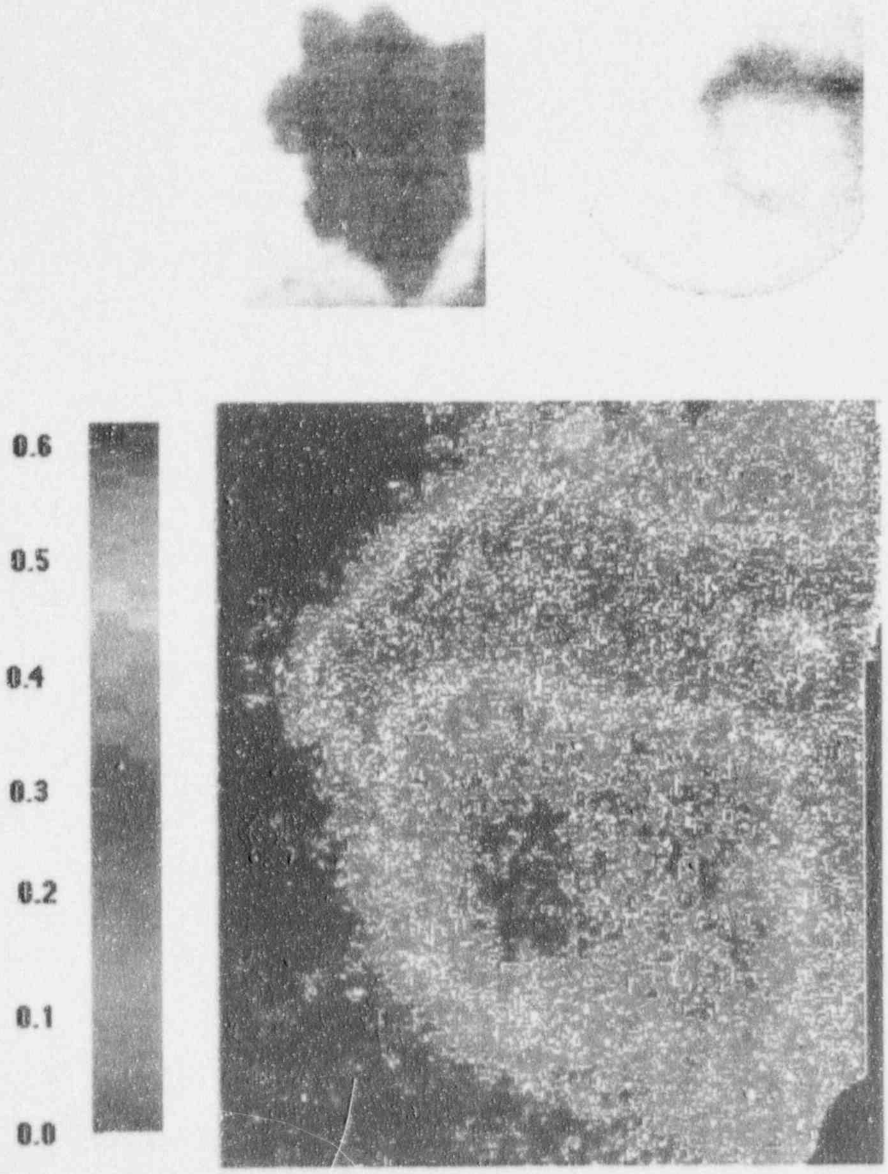
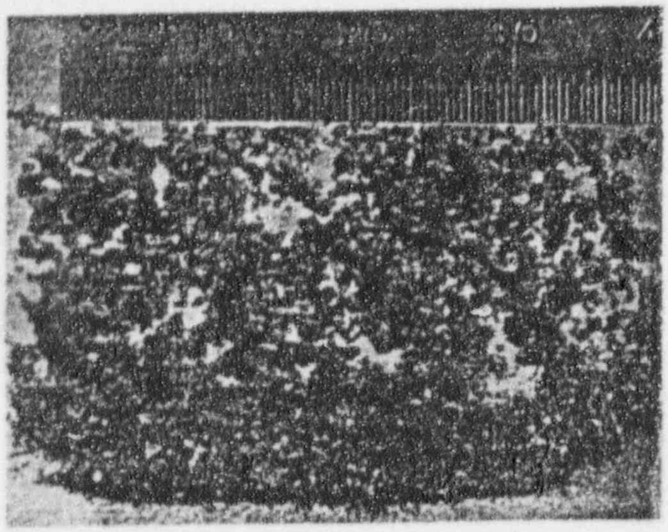
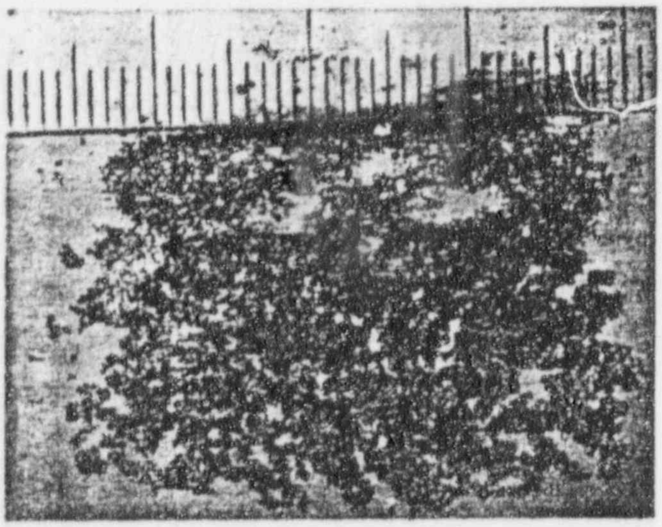


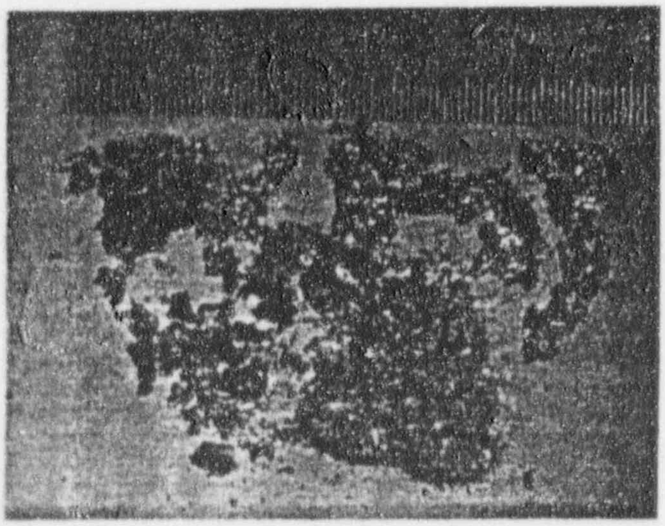
Figure 7c. Visible image, X-ray image and the mass fraction contours for a fragmenting drop at 0.22 ms after shock arrival for Run S-4-16.5(0.06).



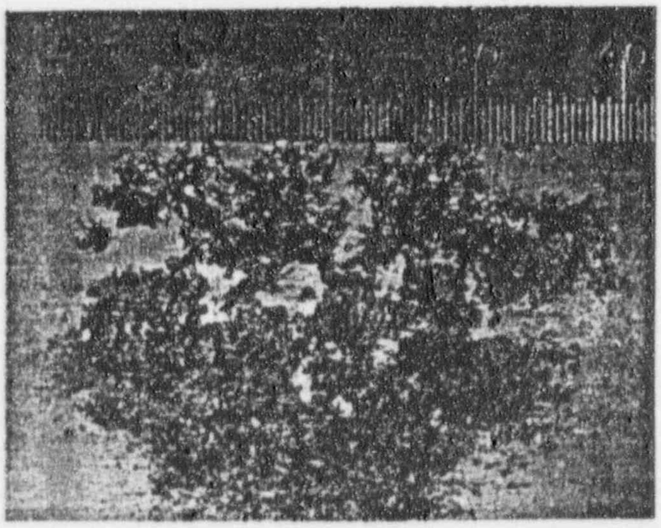
S-4-15.5(0.06)



S-4-16.5(0.06)



S-4-15.5



S-4-16.5b

Figure 8. Photographs of debris collected for four runs conducted at 265 bar.

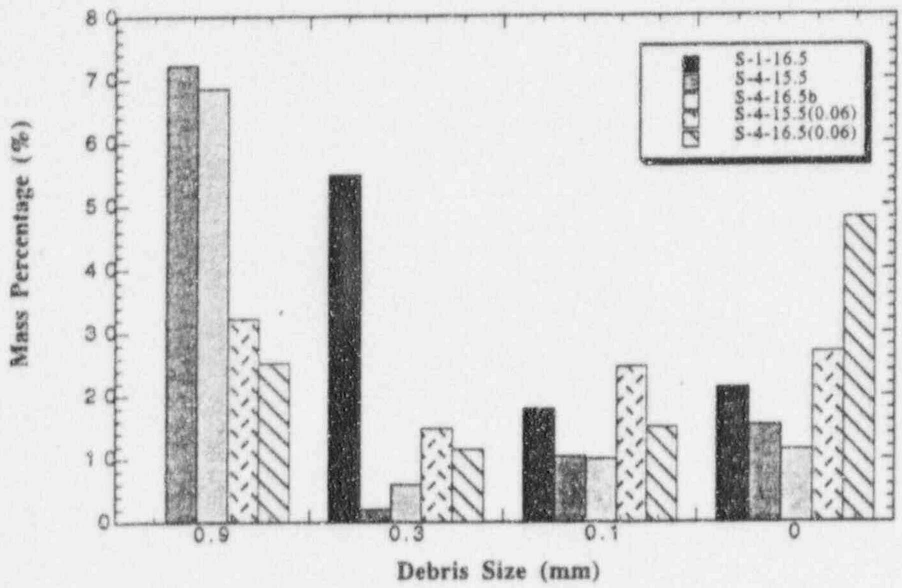
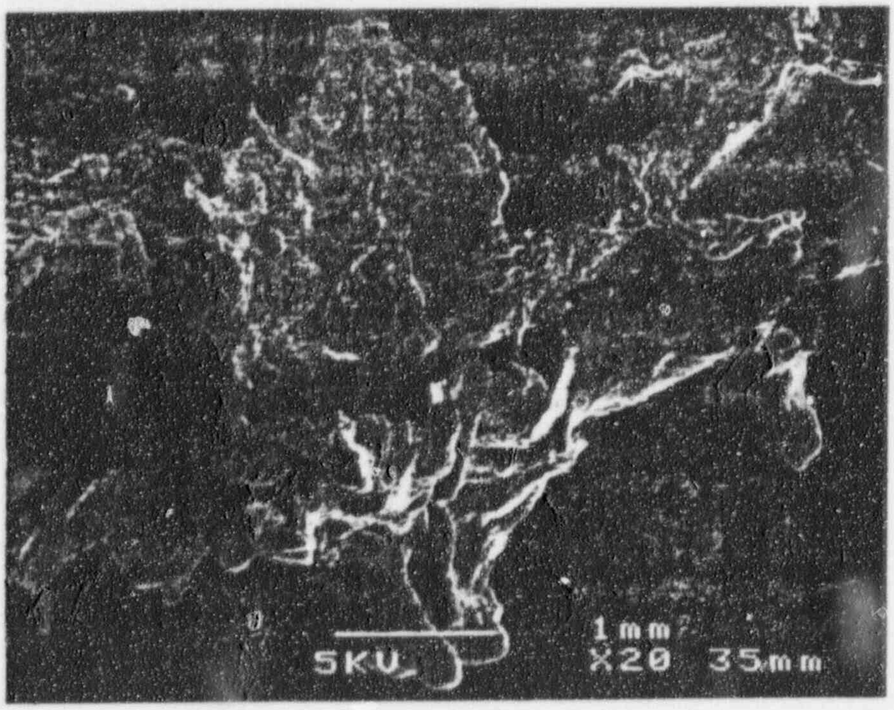


Figure 9. Size distribution of debris for the different runs.



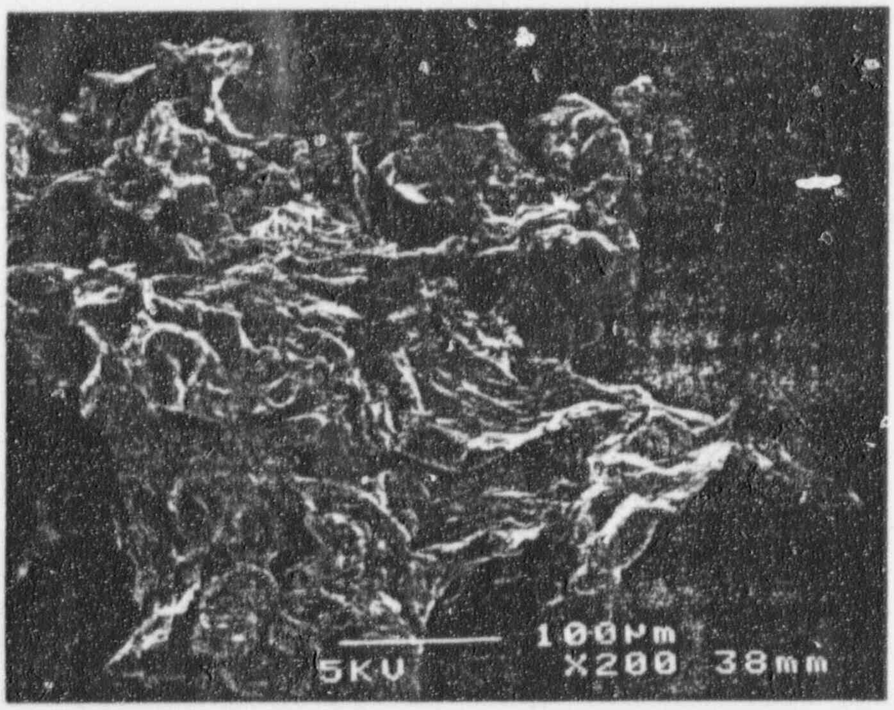
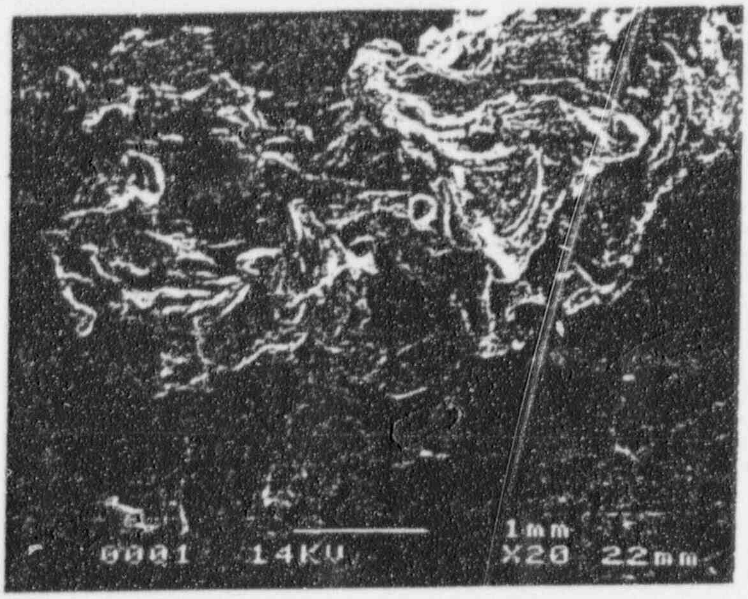
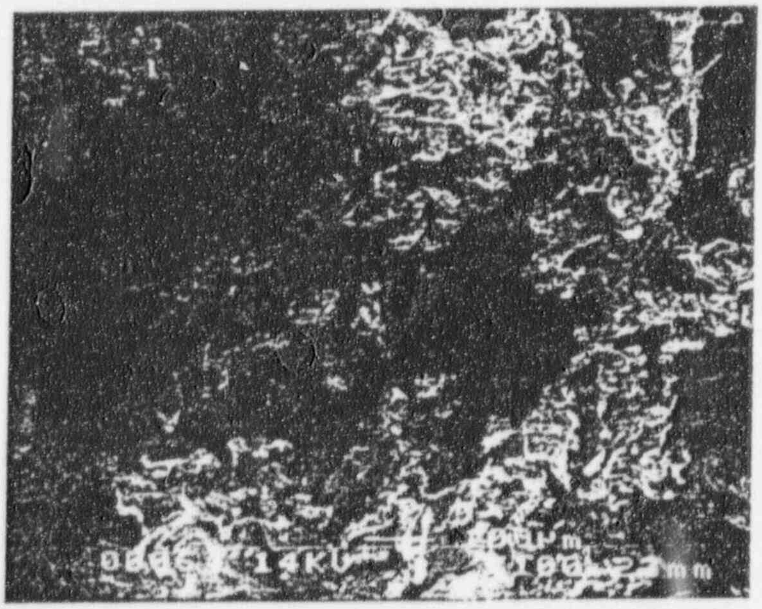


Figure 10a. SEM photographs for run S-4-15.5. The top photograph is for a "large" particle and the bottom photograph is for a "small" particle.

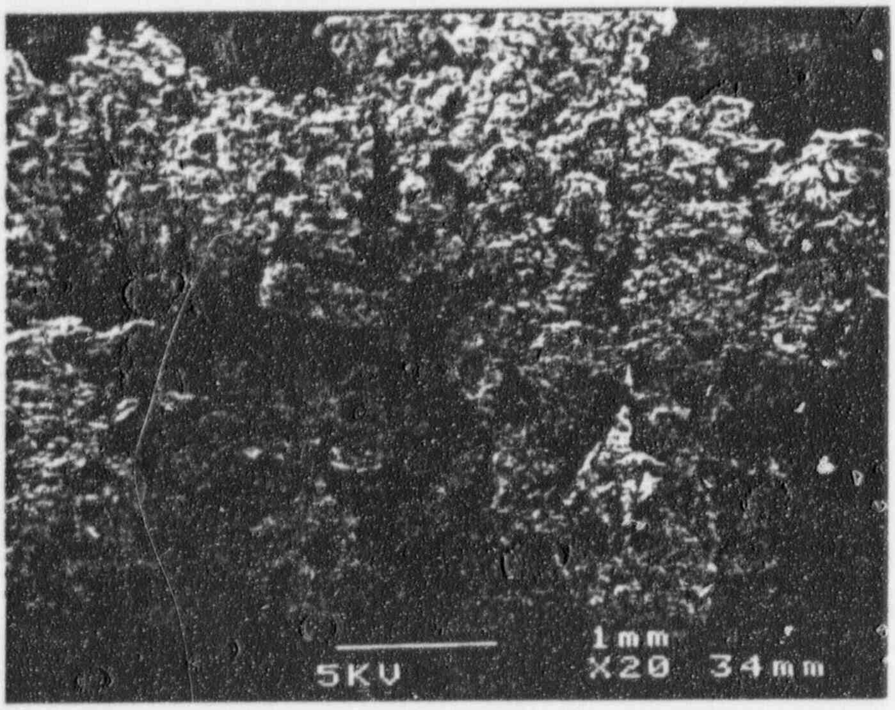


(9a)



(9b)

Figure 10b. SEM photographs for run S-4-16.5a. The top photograph is for a "large" particle and the bottom photograph is for a "small" particle.



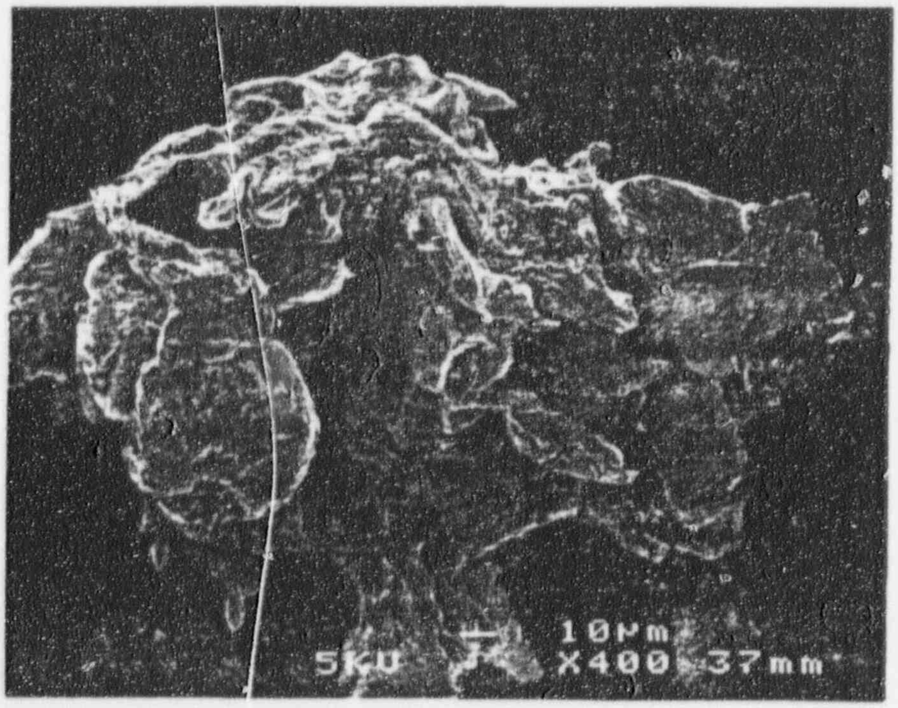
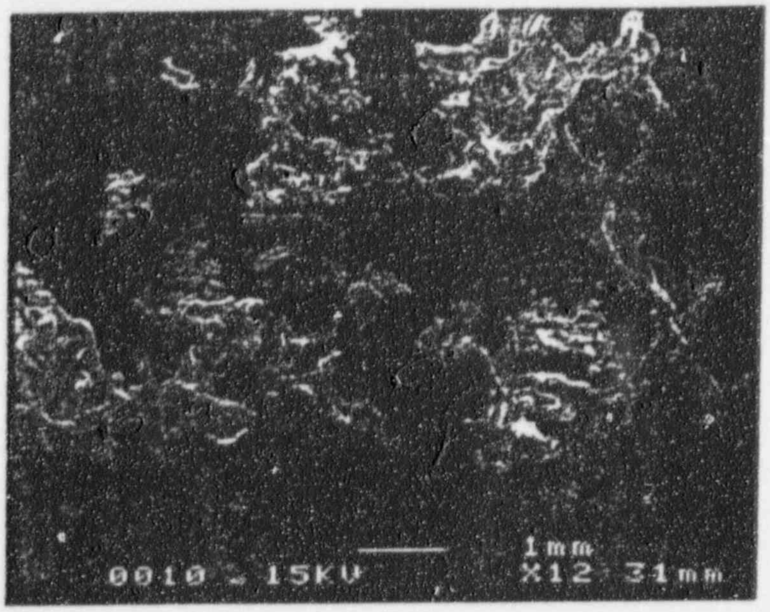


Figure 10c. SEM photographs for run S-4-16.5(0.06). The top photograph is for a "large" particle and the bottom photograph is for a "small" particle.



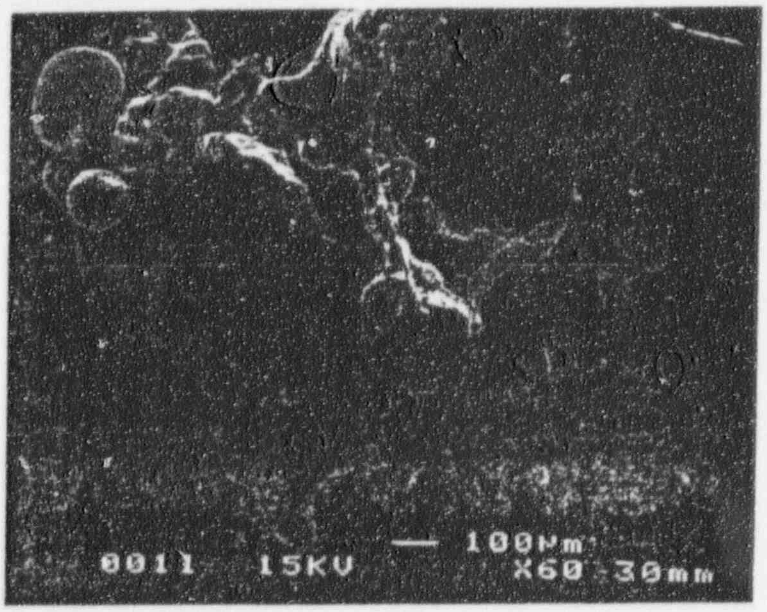


Figure 10d. SEM photographs for run S-1-16.2. The top photograph is for a "large" particle and the bottom photograph is for a "small" particle.

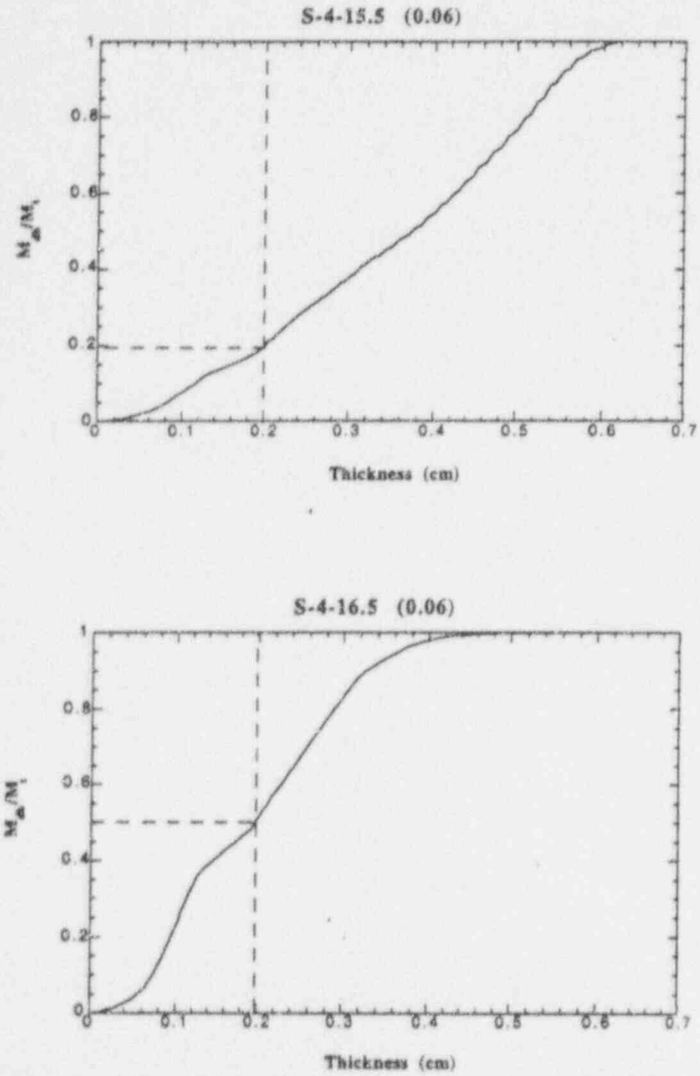


Figure 11 Mass distribution curves and the identification of the "cutoff" mass thickness for the two-phase X-ray data.

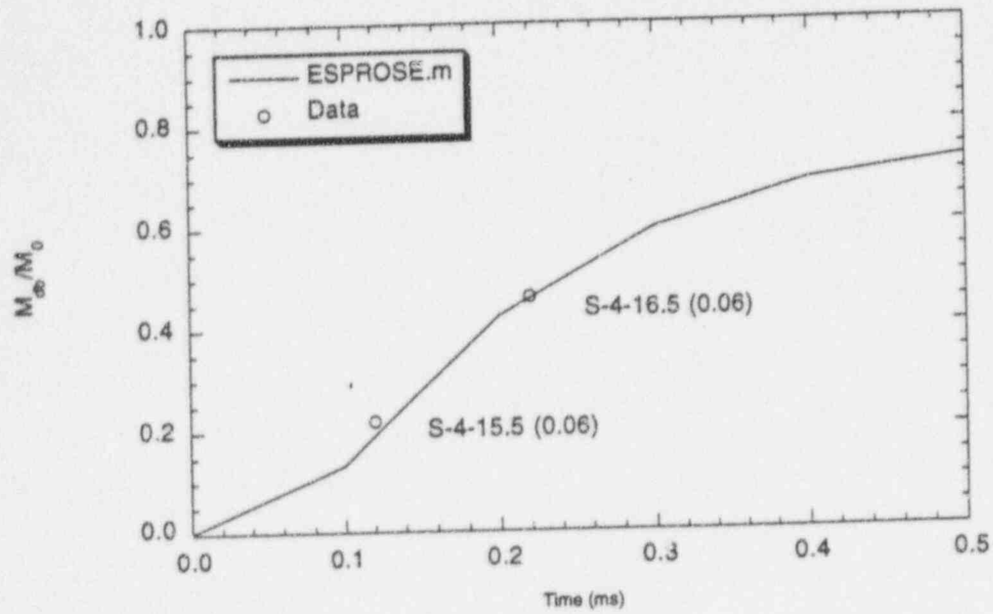


Figure 12. The ESPROSE.m interpretation for the two-phase runs using an enhancement factor $\gamma_t = 3$.

APPENDIX F

EXPERTS' COMMENTS AND AUTHORS' RESPONSES

TABLE OF CONTENTS

General Comments and Point-by-Point Responses to:

<u>Note:</u> the mark ⋆ ⋆ ⋆ ⋆ ⋆ in the author's responses indicates agreement, or no comment.

F.1. Response to S.G. Bankoff (Northwestern)

General Comment and Highlights

General and unqualified agreement with the conclusions of the work under review.

Point-by-Point Responses

The principal documents which were read by the author were DOE/ID-10541 (June 1996), DOE-10460 Vols. 1 and 2 (July 1995), and DOE-10849 (Jan. 1995), as well as various papers published and/or presented by Prof. Theofanous and one or more of his co-authors. My general impression is that this is a massive piece of work, which attacks all aspects of the steam explosion problem in the Westinghouse AP-600 reactor, and conclusively demonstrates that failure of the vessel, to say nothing of the containment, is physically unreasonable. If no failure occurs in the reactor vessel, essentially no release can occur to the inside of the containment building, and hence the threat to the public health and safety is eliminated. In the process of developing the evidence in terms of focused experiments, development of new and improved codes, tying in work done around the world, and developing a methodology for assessing the safety goals and margins for rare, but high-consequence, hazards, a set of tools has been developed which represents a huge step forward in examining severe accidents in new types of advanced nuclear reactors and in existing nuclear reactors.

In other words, in execution, scope and potential consequences, the total of this work represents a very important achievement.

* *

1. I think that the ROAMM approach makes very good sense for rare, but high-consequence, events. I believe that a similar approach has been used before, but never so explicitly and clearly stated. In particular, the recognition that there are "intangibles" which will never be known in advance, conservatively bounding them at each stage, and then enveloping the pdf passed on to the next stage, makes the uncertainties clear.

* *

2. There is no estimate of the conservatisms induced by ignoring fluid-structure interactions, assuming plastic flow with no strain hardening, and using a much

lower yield stress (330 MPA) compared to the measured yield stress (450 MPA). Such estimates would be helpful.

The scaling of the fragility with the yield stress of the RPV wall is now discussed in an addendum to Chapter 3.

3. All the codes (ESPROSE.m, ABAQUS and PM-ALPHA) appear to be well-tested against available data, and have been conservatively adjusted.

* *

4. I would think the result that the greater the load localization, the smaller the effective impulse, has practical limits. For a perfectly plastic material, a delta-function load (point source) will always penetrate continuously. The introduction of bending moments to spread the load, of course, represents the real situation. However, how far does the use of the effective impulse go for failure criterion with a very concentrated load?

Both in terms of amplitudes and areas we have well covered the conditions of interest here. But this is an interesting question, so we carried out additional calculations with still more concentrated loads. The results are shown in the addendum to Chapter 3, and they indicate that the scaling effected by Eq. (3.10) is still appropriate, although somewhat conservative.

5. The net result, from the axisymmetric and non-axisymmetric calculations, that the details of the loading pattern are not particularly important is, in itself, very important. This result would seem to hold not only for the AP-600 reactor, but for all reactors, and greatly reduces the probability of local failure.

* *

6. All the premixing work that I am familiar with (including my own) assumes that a corium jet falls into the lower pool by melt-through of the LSP (lower support plate). A great deal of work has been done (and is contemplated for the future) on jet breakup, with various materials, jet diameters, temperatures and velocities, that is predicated on this assumption. The unexpected result that for the AP-600 melt-through will occur through the side, rather than the bottom, of the melt pool, is therefore of first importance. Of course, this happened at TMI, but in AP-600 there is a thick stainless-steel reflector around the core. The code capability allows calculation of the subsequent premixing and melt/water

dynamics. There can be no large melt jet in transit; stratification appears very likely even in transit; and the whole scenario of damaging multiple explosions disappears.

* *

7. The simple models for blockage formation and blockage coolability, leading to non-availability of downwards relocation paths, and transition to a molten pool, are made credible because of the relatively flat radial power distribution in the AP-600 design. This lumped approximation would have to be re-examined for other reactor designs.

* *

8. The calculations of melt length scales and local void fractions lead to quantitative results which are more realistic and detailed than previously available. As expected, liquid water is rapidly depleted from regions of high fuel concentration, and the boundaries of such regions can be quite sharp. Board-Hall thermodynamics theory for steady plane shocks, previous multiphase calculations of the flow fields behind the shock front (Sharon and Bankoff) agree that regions of large void content cannot sustain shock propagation at supercritical pressures. This is the principal reason that the SERG-2 panel felt that the a-mode failure was not physically reasonable. Precisely the same results are obtained by the PM-Alpha and ESPROSE.m calculations.

* *

9. The residual uncertainties proposed in NUREG-1524 were jet breakup triggering, 2D vs. 3D codes, and chemical augmentation. For the AP-600, jet breakup is no longer a major concern, as discussed above. The 2D vs. 3D controversy is no longer relevant, since validated 3D codes are now available. Chemical augmentation with the real corium produced in the reactor will have no important effects. Triggering is the sole intangible which will never be known for a real accident. However, it is irrelevant if it is assumed that triggering always occurs at the worst time and place, and the result is evaluated by energetics, which is to say the validated codes. The approach taken of triggering by setting one mesh to a high initial pressure seems to me to be a perfectly valid procedure.

* *

10. Convincing arguments have been addressed, backed up by a huge volume of high-quality experimental, analytical and computational work, that the AP-600 reactor will not fail in the course of a severe accident. This implies that all later scenarios of containment-building pressurization and heat-up are no longer necessary. In my opinion, this closes the severe accident scenario for the AP-600, and leads to consideration for licensing. The consideration of other reactor types, on the other hand, does not appear to be so straightforward, and further work needs to be done.

* *

11. 1. DOE/ID-10503 "Propagation of Steam Explosion: ESPROSE.m Verification Studies", by T. G. Theofanous, et al. Aug. 1996, and update of Sect. 4.2.1

This is a convincing document, laying out the evidence that ESPROSE.m has the capability successfully to predict various shock and vapor explosion scenarios. These range from simple steady, one-dimensional shocks propagating through single-phase liquid and homogeneous gas-liquid mixtures, for which exact (or nearly exact) solutions can be found, to experimental shock and explosion data in the SIGMA and KROTOS facilities. The 1-D wave dynamics were tested for shock speed, fluid velocity, reflection at a rigid wall and reflection at a free interface with venting for single-phase, liquid-air and liquid-vapor cases, using ESPROSE.m and CHAT. The small deviations between the analytical solutions and the codes can be attributed, at least in part, to the fact that the analytical solutions used an assumed constant sound speed in the liquid, taken as 1500 m/s and 2000 m/s, while ESPROSE.m used the real properties of water. In some cases, as in the deviation in reflected shock speed, the differences in the analytical predictions for the two assumed sound speeds is considerable, but ESPROSE.m gives a smooth function of shock pressure which interpolates between the two limits, and is hence more credible than either of the two analytical shock reflected shock speeds. As a check on the wave dynamics with reflection/transmission at interfaces between two materials of different acoustic impedance, which governs the unioading-explosion coupling near a free interface, the CHAT code, using the method of characteristics, was written. For large amplitudes the quasi-linear code CHAT-QL evaluates the coefficients in terms of the local fluid properties. These codes were then compared with ESPROSE.m for pressure and velocity distribution for 1-D single-phase venting and for shock speed, fluid velocity, reflected

shock speed and shock amplitude for a 10% void non-condensible steam/water mixture, with excellent results. This sort of independent cross-checking lends considerable confidence in the basic structure of the ESPROSE.m code, which is a finite-difference code in laboratory coordinates. Exact solutions in 2D geometries are then given for infinite pool, cylindrical open pool and cylindrical closed pool geometries, by superposition of an infinite array of sources and sinks in order to obtain reflection and transmission behavior at rigid and free boundaries. These solutions are in themselves impressive, and likewise the general agreement between the code and analytical predictions for pressure as a function of time over the two-dimensional region. In fact, the agreement between the two predictions for the centerline pressure distribution as a function of time and distance is remarkable.

There is also good agreement between the ESPROSE.m prediction and the experimental data in the SIGMA facility with all-liquid, and with a liquid-air mixture in the expansion section. The key parameters of time of arrival and amplitude of pressure waves at several locations are well-predicted, particularly in view of some high-frequency ringing in the experimental transducers. The same is true for multi-region runs with high pressure ($\Delta P = 68$ and 136 bars) with different initial void fractions.

* *

12. One aspect of vapor explosions which is difficult to model properly is the presence of strong energy sources, especially near a free boundary, which are caused by local explosions (rapid mixing) of fuel drops produced in the course of jet breakup. These sources can distort venting and reflection phenomena near free boundaries. This was modeled first by characteristics solutions with single internal heat sources, with the energy assumed to be going only into the vapor, with heating rate increasing with velocity:

$$Q_g = Cu^{1.5} \qquad (3.2)$$

with C being an empirical constant. More explanation is needed for the assumed form of this equation and the magnitude of the exponent, based on energy dissipation. However, excellent agreement is obtained for various assumed values of C between CHAT and ESPROSE.m. However, comparisons with data for single exploding drops are lacking.

Here we addressed a potential numerical issue, and the form of Eq. (3.2) is immaterial. All we wanted was to create a source which increases in intensity with velocity, so we pick a rather strong dependence on u to the 1.5 power. Through the coefficient c, we can further adjust the "base level" of the intensity of such a source. We then show how one can have different degrees of interaction between such a source and venting, and we show also that ESPROSE.m performs quite well against this rather major computational challenge. We do not see any need for special experiments in this area.

> 13. *At the other extreme is a plane shock wave moving into a fuel/steam/water mixture at low pressure. This is the scenario envisaged by Board and Hall, following the one-dimensional theories of Lifshitz and Zeldovich. In the B-H theory the average specific volume of the mixture is plotted against pressure, starting with adiabatic compression to a peak pressure (von Neumann point) at which the steady mass-continuity condition for the flow behind the shock front is satisfied with minimum entropy generation. This results in the Chapman-Jouguet (C-J) condition of tangency to the reaction adiabat, or Hugoniot curve, leading to a minimum of shock speed with pressure. The ESPROSE.m calculation shows the development of the shock into a steady-state detonation, using a fragmentation rate given by*
>
> $$Fr = \rho'_f \frac{C}{\Delta x} \qquad (4.1)$$
>
> *where ρ'_f is the local macroscopic density, C is the "expected" shock speed (1500 m/s), and Δx is the grid size. Why the fragmentation rate should be a function of Δx is not clear, nor is the form of this equation. More explanation is needed. However, detailed comparisons between 2D and 3D versions of ESPROSE.m are encouraging, and the ESPROSE.m P-V lines are close to the expected Rayleigh line, and bounded by the shock adiabat.*

There is nothing special in Eq. (4.1), except that we wanted to be sure the fragmentation rate is fast enough, so as to be comparable with the "infinite" rate assumed in the steady detonation theory. The form of Eq. (4.1) assures that fragmentation occurs within the time it takes the wave to traverse one computational cell. We have since that time done calculations with very high, constant, fragmentation rates also. By improving our steady state theory (see Appendix D) to account for finite compression-trajectory effects on the Hugoniot, the agreement now is even better than before.

14. These results are excellent back-up for the interpretations of the KROTOS experiments given in DOE/ID-10489. All in all, even at this stage of limited comparison with integral explosion tests, one has confidence in the prediction of pressure-vs-time at various locations on the pool boundaries, and consequently of the initial kinetic energy of large masses impacting on reactor structure.

* *

15. Typos:

p. 1-1	*verified*
Fig. 1	*viscosity*
4-3 and ff	*Hugoniot*
	von Neumann
	C-J point

Typos corrected.

15. 2. DOE/ID-10504 "Premixing of Steam Explosions: PM-ALPHA Verification Studies", Sept. 1996

This is, once again, a thorough, and high professional, document on the verification of the PM-ALPHA code against available experimental data and known physics. The agreement with a wide range of data, from single particles settling in water, to particle swarms, both cold and hot (up to 2000° C), to integral tests with prototypic materials at high pressure, is rather remarkable. The breakup constant β, has been chosen to fit the integral data from several tests, but it is used consistently. The Richardson-Zaki exponent for a monodisperse system of spherical particles has been used without modification for thermal effects, with excellent results. The FARO experiments, which gave very little usable data on the jet breakup and dispersion, has been well-approximated for the measured steam flow and pressure. The overall result is that the code seems to be well suited for licensing purposes.

* *

16. However, some specific comments may be made and questions raised:

1. *Eq. 2.3. This equation is incorporated into the code, but no independent check on the R-Z exponent is made. On the other hand, the R-Z exponent*

was chosen by comparison with a large body of data on systems of spherical particles.

2. *Figs 9 and 10 (p. 2-10) are interesting in showing decaying oscillations, and an attractor above, but close to, the steady drift-flux/particle volume fraction curve. This physics appears to be new, and should be further investigated.*

3. *p. 2-16. The careful treatment of the radiation boundary condition with slight subcooling is noteworthy.*

4. *Figs. 8 and 9 (pp. 2-30/2.41). The comparisons between the predictions for front position and the data for the Q5 - Q11 experiments in the QEOS series is remarkable. The level swell is not well predicted in the first 0.1 s, but this may be due to experimental uncertainty. For the important range $t > 0.2s$ the agreement is excellent.*

5. *The explanation for the absence of a pressure hump at early times compared to measurements for Q17, as being due to the extra radiant heating before impact, seems to me to be reasonable.*

6. *Turning to the MIXA experiments, there is reasonable agreement with the pressure data, and excellent agreement for the cumulative steam flow.*

7. *Similarly, there is remarkable agreement with FARO L-14 water level swell, pressure, and pressurization rate, especially considering the complex geometry of the equipment. The additional information from the code on local void fraction, melt temperature, melt volumetric fraction and melt location seems to me to be very useful, in view of the inherent limitations of the experiment. My own view is that the cost-benefit ratio for further experiments of this sort is large.*

⋆ ⋆

F.2. Response to G. Berthoud (CEA Grenoble)

General Comment and Highlights

The review is generally agreeable, but reluctant to fully accept that a bottom relocation path is physically unreasonable. This is a key point of our evaluation, and we provide responses to the reviewer's specific questions. We expect these plus our responses to similar questions from other reviewers to be helpful in further focusing the issues towards resolution.

Point-by-Point Responses

1. This document presents an analysis of the potentiality of lower head failure of the AP600 resulting from a Steam Explosion. The conclusion that the risk is negligible (≪ physically unreasonable ≫) is quite convincing, and is based on:

1. *the fact that water will be saturated and at 1 bar due to complete depressurisation to the containment pressure and that these conditions will lead to large and rapid voiding which is not favorable for large S. E.*
2. *the fact that we have permanent blockages at the bottom of the core that will impeed any coherent relocation through the core support plate*
3. *the fact that relocation will occur sideways through the reflector and core barrel and so that the Steam Explosion will occur in a 3D geometry without any large constraint allowing large sustained pressure*
4. *the fact that - even if reflooding is taken into account - when the melt will be ejected sideways, we will have enough time to heat the added water up to saturation and so to prevent good mixing.*

The validity of the conclusion is then linked to the validity of the above four arguments.

As for the first argument, there is no doubt that water will be saturated as far as reflooding is not taken into account, tile fact that the pressure will be atmospheric cannot be discussed here as this is justified in another report (IVR Report - table 7.3) however, I think that this has to be justified as the voiding will be less important at pressures a little bit higher, around some bars. At these pressures, we can also recall that it was found it was easier to trigger an explosion in the single iron oxide droplet experiments of Nelson in Sandia.

The fact what we have up to now no evidence of explosion in experiments using reactor like materials (Krotos) (and that this is due to the non occurrence of good

mixing) is stressed by the authors. But once again these Krotos experiments are performed at 1 bar pressure while in Faro experiments at pressures of 50 and 20 bars, with saturated water some mixing was obtained. In a near future a Faro experiment using initially saturated water at 5 bar will be performed and we will then have an indication of the quality of mixing at small pressure.

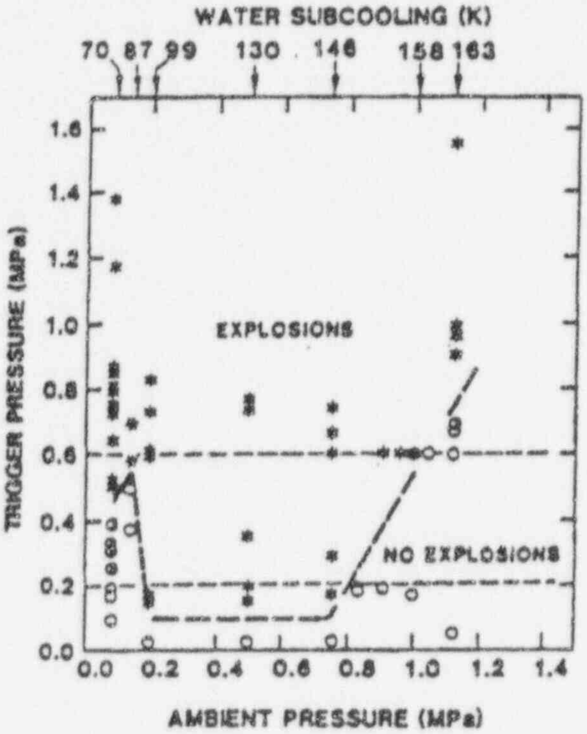


Figure 8. Relation entre le "trigger" ńecessaire au dèclenchement neclenchement d'une interaction et la pression ambiante (cas de gouttes d'oxyde de fer de 2, 9 mm de diamètre à 2230° K tombant dans de l'eau à 298° K).

Because of the passive design of the AP600 the pressures applicable to the severe accident management window are in the range 1.1 to 1.7 bar. This is too narrow a range to bring in significant pressure effects, and for completeness, we have run some calculations and satisfied ourselves that indeed this is so (see addendum to Chapter 5).

2. I will now go through the different chapters trying to analyze the justifications which are presented to support the crucial arguments mentioned previously.

Chapter 2: Problem definition and over all approach

• It is mentioned that it is only recently that pressures in the kbar range were observed experimentally in constrained one dimensional geometry. However, I think that a pressure peak of the order of the kbar amplitude and millisecond duration was measured in the Sandia FITS-RC2 experiment which was well vented (initially open at the top and later vented at the bottom as the vessel left the ground). But this was obtained using iron-alumina thermite and subcooled water.

We are aware of the experiment, and its energetic natures (from movies), but we do not have indications in the literature of reliable pressure measurements.

3. • Another important argument is that ≪ because of extensive voiding, we need only be concerned about the first relocation event, and only for early trigger in it ≫. This seems to be justified by the premixing and explosion calculations presented later but I wonder why, after a first event, when water is sloshing back a second event cannot occur at about the same location where the structure will has been already dynamically loaded and eventually already deformed by the first event.

As seen in Chapter 7, the structure response remains within the elastic limits. Subsequent relocations in the scenario proposed by the reviewer would be too gradual, and water would be quickly depleted from the lower plenum.

4. Chapter 3: Structural failure criteria

In this chapter, it is stated that ≪ the time-duration of the loads of interest here is less than the structural frequency ≫, so it is expected that ≪ peak strain would be basically independ of the details of the pressure pulse shape ≫. However, nothing is said about the estimated value of the natural frequency of the R.P.V. which seems to me to be of the order of magnitude of some msec so not so far from the load duration.

However, all the analysis is made with the analytical solution of Dufley and Mitcheli which assumes ≪ short pressure pulse ≫ and allows to evaluate the plastic equivalent strain with incorporation of strain rate effect by formula (3.2). But the comparison of the analytical results to ABAQUS calculations shows that the analytical relation gives conservative results for the plastic strain evaluation (fig. 3.2).

No. All the analysis is **not** made with the analytical solution; it is made with ABAQUS. The analytical solution is used to obtain insights helpful to the task of coming up with the lower head fragility.

5. Another mitigating factor is investigated: the effect of load localization which shows that for a given impulse, the equivalent plastic strain is smaller when the loading is smaller. Use of these results is then made by assuming that a fraction β of the impulse is used for bending energy so that only an ≪effective impulse≫ is applied for the evaluation of the equivalent plastic strain. We are told that β is a material and geometric ≪ constant ≫ but I have not found any indication of its evaluation i.e how the results shown on fig. 3.8 and fig. 3.9 are obtained. As fig. 3.9 is used to evaluate the effect of loads calculated in Chapter 6, I think that it should be a little more explained.

The β is chosen so as to fit the "data" (i.e., the results of ABAQUS simulations). The value is 0.05. This was added on Figure 3.8.

6. It also seems to me that the localized loadings are applied on the axis of the hemisphere (see table 3.2). does the fact that these localized loadings will occur on the side of the hemisphere with singularity where the sphere is linked to the cylinder will modify the conclusions we can draw from fig. 3.9.

The cylinder juncture to the lower head is quite a bit higher than where the water level is, and the further below location where the premixture develops. Moreover, in Chapter 6, we have full-vessel simulations (see Eq. (6.2)).

7. N.B. There are some errors in table 3.2 as for the value of A_o which does not correspond to $\frac{\pi d_o^2}{4}$ as written in the caption.

Typo corrected.

8. Chapter 4: Quantification of melt relocation characteristics

This is a very important task as most of the boundary conditions for premixing and explosion calculations are obtained from such an evaluation.

• The downward relocation path (arguments 2) is not envisaged: ≪ we expect this path to be blocked by molten cladding and the blockage be robust ≫. This expert judgment is supported by the large heat sink associated with the large amount of ≪ cold ≫ materials in the lower part of the core. As it is said that

due to be big stainless steel reflector, the first relocation will be delayed compared to what occurred in TIM and that at this time, we will have a large oxide pool, it is important to know if this molten pool will reach the region of the lower fission gas polonium where the heat sink is not very large and where we can have a breakdown of the supporting material. However, in the paper, the blockage is said to occur in the region of the 7 cm ≪ lower Zr plug and lowermost spacer grid ≫. Some calculations are presented to show that the plugging time of this region by melt with negligible superheat is of the order of seconds. For this calculation I have some trouble with formula 4.2 where, as for me, λ is not the same as in the Carslaw and Jaegger text book but I did not try to perform the calculation. We can also make another remark: if we have some breaking down of the fission gas plenum region, when the molten pool arrives we may have superheated molten material from this pool that with flow in the lower blockage region for some times before plugging It would be interesting to know what amount of molten materials can be transfered in the lower plenum through the holes in the core support plate before plugging of the passages in the blockage region. As for this plugging time - which is crucial to support argument 2 - it would be interesting to see more realistic calculations including the influence of the interface thermal resistance between the crust and the solid wall that will slow down the freezing process and then increase the plugging time.

If the blockage was to fail, releasing material into the fission gas plenum region, the material entering the Zr plug region would **not** be superheated, and will carry with it a significant metallic fraction from melting the cladding in the fission gas plenum region. Thus plugging would be **very** fast, and negligible melt could escape downwards.

9. • *As for the blockage coolability:*

- the stable blockage thickness should be sensible to the radiation factor f_r which is set to 0.7 without any explanations

- the cooling of this blockage is ensured for about 100 mm which is the time required to vaporize the water which fills all the volume between bottom of active core and bottom of core support plate. It is later estimated that meltthrough of

the reflector by the molten oxidic pool will occur between 76 and 91 mm according to the amount of oxidation (80 to 95 mm in the calculation ≪ without ≫ preheating). If we add the time require to melt through the core barrel, we get timing of the release of the same order of magnitude than the insurrance of blockage coolability. As all these calculations are order of magnitude ones, I think that argument 2 (no downward relocation path) may be questioned.

All relevant factors were evaluated with some conservative bias, and these are **not** order of magnitude calculations. Rather, we would characterize them as basic-principles based conservative estimates, clear and supportable in every respect. So, to dispute the quantitative result, one would need to be specific about which input or aspect of the calculation is being questioned. In the report, we also emphasized that beyond the 100 min, the heat capacity of the core support plate provides further margins to failure—this is a key point in the whole argument. In any case, some further elaboration on uncertainties is provided in an addendum to Chapter 4.

10. • Molten pool formation

- In the initial heat up calculation, are the reflector and core barrel in contact everywhere as it is shown fig. 4.8 and 4.9? In that case the cooling effect will be overestimated and the melt superheat underestimated.

No. The melt superheat is only controlled by the pool convection processes. The crust thickness adjusts itself only to the losses, and this is not important.

11. - During the transient heat up calculation, what happens to the molten cladding and how the calculation with the effective thermal conductivity is eventually modified?

The effective conductivity is not modified. The anoxidized cladding is assumed to drain.

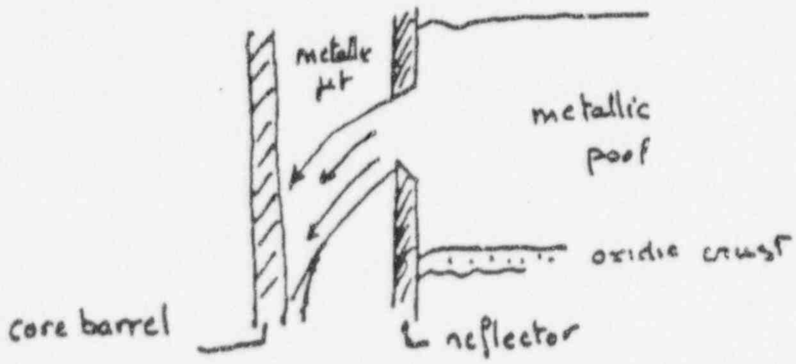
12. • Molten pool calculation

Such a calculation is performed for the oxidic and metallic pool, and there is a crucial hypothesis which is the presence of a stable oxidic crust at the upper surface of the oxidic pool. In the document, it is mentioned that it is assumed that the clad drains but is it fully true? Cannot we have some metals included in the moving down oxidic pool? What happen to the part of fission products which are released at fuel melting? Will they modify the molten pool behaviour for the stability of the upper crust and the evaluation of the differents fluxes?

The stability of the upper crust is not important here. If it is distorted and broken by some vaporization from below, it will form again right away. We know that the metal layer will "eat through" the reflector rather quickly, and the exact timing of it does not make any difference. Certainly it makes no significant difference to the treatment of the oxidic pool either.

13. • Melt through and melt release calculations

It is said that rapidly the metallic pool will melt through the reflector but it is assumed that the metal ≪ will be gradually draining ≫ into the space between the flats of reflector and the core barrel. Cannot we have some kind of metallic jet impacting on the core barrel with some rapid meltthrough leading to a steam explosion between metals and water?



No. Because of the highly erosive property of the metal it could not accumulate to significant depths to produce the stream needed to penetrate the "cold" core barrel. The process will be more like a gradual overflow.

14. • From the above analysis it is concluded that when the oxidic melts through the reflector, there is no metal on it and that failure of the core barrel occurs soon after. First, it would be interesting to evaluate the time required for core barrel meltthrough (if there is an open space between the two of them).

But there is another problem if the space between the flats of the reflector and the core barrel is already filled with the metals from the metallic layer, how the oxidic pool can rapidly go through the core barrel. This situation may be a promoter for downward relocation if this added metal may increase the time for meltthrough.

As noted on p.4-23, the total capacity of the spaces between the reflector and the core barrel is ~10 tons of steel, and this corresponds to 50% of the reflector corresponding to the pool height. The oxidic pool will fail the reflector and the core barrel well before such an extensive melting of the reflector; that is, before the spaces are completely filled with metals. If this were not the case,

the failure of the core barrel would occur at one of the corners of the reflector, making the failure more localized and hence producing a more benign pour. In any case, there can be no significant enough delays in melting the core barrel as compared to the times necessary to lose lower blockage integrity.

15. • As for the location and size of tile failure, most of the information is obtained from expert judgment and should be further justified:

- the failure ≪ is **expected** *≫ to be local azimuthally and very near the top of the oxidic pool. I would agree with that statement as even, if the calculation is 2D cylindrical once a flat will fail, the rapid relocation will impeed failure on other flats. But I would not be able to give any probability for 2 quasi simultaneous failure, or 3....*

- for the size of the breach, it is said that 0.4 m ≪ **would appear** *geometrically a good upper bound on the first breach width and that a ≪ 10 cm axial gap is believed to be conservative ≫.*

- there is no mention of the rapid increase of the size of the breach during the melt release as it has been observed in experiments. However, as only short duration premixing-triggering scenario are taken to be of interest, this enlargment would not be important. But, if we take into account steam explosion occuring when water is sloshing back after a first event, this has to be taken into account.

See response to previous question about the postulated "series" of steam explosions.

16. Chapter 5: Quantification of Premixtures

Given the melt release conditions (flow rate, location, temperature and composition), the premixtures are calculated with an improved version of PM-ALPHA which is now 3D and includes a melt fragmentation law (which was lacking up to now) as it is recognized that it is interesting ≪ to know the distributions of the melt length scale ≫. However, this fragmentation law is not described and this should be done and justified as fragmentation is responsible for voiding (≪ the rate of voiding increasing rapidly with the rate of breakup ≫). I would also like to know why the ≪ breaking law is operative only for as long as the coolant has a void fraction of less than 50% ≫. If the fragmentation was always operative, voiding would be larger so there must be a good reason for doing so but I do not see why.

The breakup (not fragmentation) law is described in the PM-ALPHA verification study, which was also supplied to the reviewers. There is a void fraction limit (conservatively set to 50%) to express the fact that, breakup occurs significantly due to the presence of water–inertia effects and melt-water thermal interaction effects. See, for example, our interpretation of MIXA and FARO data.

> *17. The melt entrance conditions into water are also specified and not calculated:*
>
> *- entrance velocity whose evaluation is correct*
>
> *- distribution of the melt ≪ over an effective radial width of 10 cm ≫ with a melt volume fraction evaluated to get the correct mass flow rate. This distribution is crucial in determining the amount of vapour which is produced as the larger the entrance area, the longer you are in the film boiling regime in which the steam production is at maximum. This behaviour was observed in MC3D recalculations of FARO tests, where a doubling of the pressure increase (linked with vapour production) was obtained with a doubling of the diameter of the melt flow. Recently CHYMES 2 recalculations showed the same trend.*

Steaming rate should not be confused with voiding of a premixture. Here we are principally interested in the extent of voiding, **not** steaming rates.

> *18. - initial droplet diameter which is set to 20 mm (a large enough value to represent a minimally broken up melt stream). This parameter is also important for vapour production. It would be interesting to see sensitivity calculations with diameters varying from 10 to 1 cm.*

The initial melt length scale is absolutely unimportant, as long as it is large enough to represent a minimally broken melt stream as we expect to be the case here in the travel through the vapor space. We have demonstrated that by the 2 cm no-breakup case, which is less voided, but still rather benign, a larger length scale will still be more benign. Now, due to the breakup, in the advanced portions of the jet the vapor, like a chimney, also would cover the less broken portions and give still more benign explosions.

> *19. I am not so sure that the melt will be transformed in a droplet population before entering the water. We may have a large melt stream on the wall with subsequent fragmentation into the water but with a different law than droplet fragmentation. Would it lead to a ≪ benign evolution ≫ as it is mentionned. This is again an expert judgment.*

This concern would be applicable if we used a mechanistic law for droplet breakup, but for this and other reasons we don't. Rather, we vary parametrically the law, and **envelop** the behavior regarding extent of voiding as related to the **law** (and extent) of breakup.

> *20. As for jet fragmentation calculations with THIRMAL, I cannot trust them if the fragmentation is still governed by Kelvin Helmholtz type calculations. Moreover, in FARO experiments with 10 cm melt jet, it took more than 2 m of water to break the jets in a 50 bar atmosphere for which voiding is smaller.*

At 50 bar the voiding is smaller and the steam velocities generated are also smaller (than at 1 bar), and this is why in FARO we expect less breakup. Still, however, the premixture was highly voided, and the melt extensively fragmented according to our interpretations of the tests. Ours are fully consistent calculations using the proper non-local radiation absorption law. The THIRMAL calculations are offered as another perspective, still parametric, not mechanistic. We do not believe anyone can do mechanistic calculations that can be trusted; moreover, we do not believe such would be a good approach even for the future (it would run into serious verification problems due to inherent problems with directly quantifying this aspect in experiments). We do not know where the reviewer's accertion about the FARO tests derives from.

> *21. Chapter 6: Quantification of explosion loads*
>
> *Nothing is said about the parameters used in ESPROSE-m 3D but as the trigger uses a 100 bar steam release, we may think that the hydrodynamics fragmentation law will be correct Due to the small amount of melt involved in explosion calculations, there is no problem with the energetics of the explosion and we are only interested in dynamical loadings of the RPV. This is done by the estimation of the impulse and of the local area of loadings from ESPROSE-m results. I have some problems to understand how $\frac{d_o}{DS}$ is estimated page 6.3 from the area evolution as shown on fig. 6.5.a.*
>
> *In the text, it is said that peak impulses are around 0.1 and 0.2 MPa.s with effective area around 0.1 m^2 (which gives $\frac{d_o}{DS} \sim 0.15$) and from fig 6.5 c where I find a 0.2 Mpa.s impulse, I do not understand how I get $A_o \sim 0.1$ m^2 from the area evolution which is shown.*

We take the area over which the main impulse is delivered. For example, in Figure 6.5(a), this occurs in the time interval from 0.3 to ~0.5 ms. Over this time interval the area is then seen to be ~0.1 m^2. Then using the equation on p. 6-3 we find $d_o/D_s \sim 0.16$.

22. Apart from this problem, if I accept the figures mentionned in the text, I agree when coming back figure 3.9 that there is no risk for $I \sim 0.2$ and $\frac{d_a}{DS} \sim 0.15$ as $\frac{\epsilon 3D}{\epsilon} \sim 0$ This is confirmed by the ABAQUS calculations of the tsvo most energetic explosion calculations obtained by PM-ALPHA plus ESPROSE-m.

* *

23. As a conclusion, I can say that if we accept the scenario which is retained by the authors, I think that - whatever my remarks about premixing quantifications - the AP 600 RPV cannot be challenged by a steam explosion. However, I would like to have more established confirmations of this scenario by mechanistic calculations when possible or parametric calculations when it is not.

The main thing to be confirmed is the impossibility to have a downward relocation i.e

- the possibility to have a break down of the lower fission gas plenum rather than a continuous draining. This will give a sudden access of the core support plate holes to the melt

- the influence of the already relocated metallic pool on the oxidic release. It may take a longer time to break through and the blockage integrity may then be challenged. The influence of an interfacial resistance between the oxidic solid crust and the wall - specially at the top of the pool - will also participate to an increase of the time of break through and of the evaluation of melt superheat.

Other branches for the scenario should also be evaluated:

- the possibility of the metallic melt to rapidly go through the core barrel leading to metal-water steam explosions

- the possibility to have steam explosion at later times (in the oxidic case) when water is sloshing back after a first small scale (i.e low energetic)event.

As for reflooding scenario, the fact that water will be closed to saturation should also be evaluated.

This is a summary of points made and responded to above.

F.3. Response to M. Bürger (U. Stuttgart)

General Comment and Highlights

Many specific issues are raised in this review, about almost every aspect of the analysis and supporting documentation. However, it is also stated that in an overall way the analysis is convincing and that the spirit of the criticism is to help provide further supporting evidence. To the extent possible, this is done in the point-by-point responses below.

Point-by-Point Responses

1. *1. Purpose, Procedure and Main Conclusions of the Study*

The purpose of the work is to show that the lower head of a reactor like the AP600 withstands the load of steam explosions. According to the ROAAM philosophy, all physically meaningful causal paths that could lead to failure have to be investigated. The decomposition yields the following central areas of analysis:

1. *Since pressures in the kilobar range have been obtained in the KROTOS experiments (although not yet with corium and in one dimension), a direct exclusion of lower head failure cannot be done. Thus, detailed calculations of possible explosion loads are required, taking into account the specific geometry with respect to venting effects. This is done by use of a 3D version of ESPROSE.m.*
2. *The possible spectrum of melt/coolant mixtures developing in the lower head due to an assumed core melting must be determined. This is done by use of a 3D version of PM-ALPHA.*
3. *Possible timings and strengths of triggers have to be considered. Due to the uncertainties, an enveloping approach is pursued here, concerning the timing as well as the strength.*
4. *Since close to the wall pressures in the kilobar range are obtained in the calculations, specific investigations on failure criteria are required. This is done by a simple estimate and also by means of the ABAQUS code. Considerations on the possible interaction with thermal loads are also required.*
5. *Considerations on the melting and relocation process in the core and the release to the lower plenum have been considered as necessary for restricting the possible spectrum of melt/coolant mixtures. This is done by separate estimates.*

The main arguments in the report are:

It is assumed that a pool of ceramic melt surrounded by crusts forms in the core due to the cooling capabilities of remaining water in the lower plenum (at the beginning of melt motions with level at ~25% of active core height) and the large heat capacity of the lower part of fuel bundle with lower Zr plugs and the lowermost spacer grid. The key points are then that a downward relocation path of melt through the core support plate is excluded and meltthrough of reflector and core barrel is assumed yielding finally a sidewards relocation through the downcomer.

This sidewards relocation is restricted in extent assuming failure at the upper end of the pool based on the analysis of heat transfer from the pool and assuming plausible failure sizes. Further, it is argued that only one failure location is available within relevant times for premixing of the relocated melt in the water and triggering. Strong voiding of the mixtures under the expected conditions of saturated water is expected and calculated by 3D-PM-ALPHA. Thus, only small amounts of melt in the lower tens of kg are considered to be potentially explosive. This is taken to directly exclude large break possibilities for the lower head. Various calculations with ESPROSE.m assuming sufficiently strong triggers are additionally taken to exclude also local threats to the RPV. This is finally done by comparison with failure criteria for the RPV wall yielding directly (without application of the probabilistic framework) the conclusion that failure is physically unreasonable.

Further, reflood scenarios are evaluated to even mitigate the possibility of vapor explosion threats, due to cooling and preventing melt outflow. Mixing with the melt in the pool is not considered as effective (small yield of stratified explosions). In addition, preventing outflow by reflood would also mean to exclude mixing of melt with highly subcooled water, which is considered as the only case with a potential to challenge the lower head due to increased penetration depths without excessive voids.

Cases with thermally weakened RPV walls are restricted to later phases of melt outflow. Then the water and mixtures are already assumed as strongly voided. Reflood FCIs are considered in the report in stratified configuration, i.e. water above a metallic layer. A threat is excluded due to rapid spontaneous interactions with the subcooled water and rapid freezing of the metal surface before a thick

water layer establishes which could yield sufficient constraint for strong pressure buildup.

Based on these considerations, the major conclusion of the study is that steam explosion-induced lower head failure in an AP600-like reactor is "physically unreasonable".

* *

2. 2. General Co. ents

The procedure as well as the general arguments are convincing. This concer especially:

- *The argument that a strong cold trap at the core support plate, espec ally if still connected with water, can prevent the downwards release path to occur before sidewards release at the upper region of the melt pool. This yields a significant reduction in melt flow rates to the water, especially to a possible downwards release in multiple streams. If the cold trap at the bottom is strong enough, no downwards release will occur until all melt is released sidewards due to a continuous failure progression.*
- *Then, the saturated coolant condition prevents larger premixtures without high voids, since larger premixtures could only develop within longer times. The geometrical conditions of the flow through the downcomer also favors this.*
- *The strong voiding of mixtures calculated with PM-ALPHA is thus plausible.*
- *With the small mixtures (small melt masses) of not extensive void plausible from the above statements, the ESPROSE results are also plausible (the obtained pressures even appear astonishingly high - probably due to the restricted venting).*
- *Thus, also the conclusions on the threats are plausible.*
- *The high number of calculations with PM-ALPHA and ESPROSE covering a wide range of conditions can also be taken as supporting.*

In spite of this agreement in principle, there remain problems in the details of the argumentation and performance of the analysis. Improvements may be performed to even better confirm the statements and conclusions as a basis for use in licensing

actions. This will be discussed as follows in some detail. Since, in my opinion, the statements on the relocation path are a most critical point, I firstly concentrate on this, then considering the subsequent analyses on premixing and explosion. I will not consider the aspects of structural failure criteria which appear to be well established. I will only give few arguments on possible further scenario aspects concerning reflood.

★ ★

3. 3. Comments On Melting and Relocation

Firstly, it is shown by estimating plugging times from a freezing model based on semi-infinite heat conduction that the plugging takes place in a range of seconds (for the lowest initial rod temperatures 0.6 s with Zr and 2.4 s with UO_2). However, the melt flow is not taken into account in these considerations. This means, that the freezing zone may extend over quite a distance. E.g., ~0.6 m would result for 1 s with 0.6 m/s as a typical velocity from CORA experiments. Higher velocities would result with thicker melt films. Thus, the final blockage formation should require some more time and distance (need of additional melting and melt flow or compaction to a crust by remelting and relocation of upper parts of a partly blocked region). Further, this process may yield local incoherencies of the crust formation, i.e. also weaker regions, although the cold traps at the bottom give certainly a unifying trend. Thus, in order to further verify the statement of rapid blockage formation calculations with a core melt code would be desirable. These could also yield a more detailed perspective on related (subsequent) important questions, especially the heatup of the cold trap regions and the water level development.

To our knowledge there is no mechanistic code calculation out there that shows the possibility of direct melt relocation through the bottom of the core. Even for BWRs, with much more open geometry and a much larger metallic component in the core, there are significant doubts about such a direct relocation scenario. We do not think such calculations are necessary.

4. Secondly, assuming an existing blockage with an overlying melt pool, it is checked whether a steady state with a stable crust below melting point (~2800 K for oxidic or 2100 K for metallic material respectively) can exist. Metallic and ceramic crusts are considered alternatively, with a heat flux from the molten ceramic pool above of ≤ 0.02 MW/m^2, a volumetric heating of ~0.5 MW/m^3 in

the ceramic crust and cooling from below via radiation. Here, it appears not clear to me why the fraction of fuel volume is only taken as ~30% (p. 4-6). This seems to be a value for intact structures. However, if the metallic parts are all relocated during establishment of the ceramic crust, then this may consist essentially of UO_2/ZrO_2 (~80/20 wt ratio). The local shape factor should also not be decisive due to the crust formation from upper material. Taking a value of decay heat of 300 W per kg fuel (p. 4-18) this would yield ~2 MW per m^3 of the UO_2ZrO_2 crust.

Existing and relocated fuel volumes and relocated metallic melt volumes were consistently taken into account. The relocated material is taken at the average core power, while the fuel stubs that support the blockage is taken at the local peaking factor — but without releasing its volatile fission products as it is found in a cold, solid state. See additional results in the addendum to Chapter 4.

5. *For the downward heat flux from the molten corium pool above, a maximum value of 0.02 MW/m^2 (fully developed) is assumed. This is derived from Eq. (4.14) based on the Steinberner-Reinecke correlations for a rectangular geometry (typing error in (4. 14): exponent 0.095 instead of 0.049). However, this correlation is only confirmed for $Ra' < 5 \cdot 10^{10}$. For the conditions considered here, I obtain a value of $Ra' \sim 10^{17}$, assuming $H = 1.8$ m and $Q = 2\ MW/m^3$.*

The correlation is also derived for non-isothermal lateral boundary conditions, in contrast to the present assumptions. The influence of the lateral boundary conditions appears to be small, however. For a case with vertical cylinder and melting point temperature at all boundaries THEKAR calculations [1] also yield a rather similar correlation ($Nu_u = 0.935\ Ra'^{0.10}$), but only for Ra' numbers below 10^9.

The value of 0.02 MW/m^2 is absolutely insignificant in comparison to the heat flux generated due to the heating in the blockage itself (see also the addendum to Chapter 4), which is in the 0.1 to 0.2 MW/m^2 range. Thus the 0.02 value could be varied by +100% or more without changing the results — such uncertainty is not expected in the kinds of questions asked here.

6. *But, the main question is to me whether - in view of the above arguments and at least some lateral cooling potential - the assumption of a rectangular pool geometry is a too strong idealization and other geometries closer to hemispherical shapes can really be excluded. Such geometrical variations would yield significant variations in the heat transfer to the lower boundary. The influence of the*

lateral boundary would increase (natural convection influence versus stable stratification). For a hemisphere (certainly an extreme under the given flat radial power shape) even a mean heat flux of 1.05 MW/m^2 would result at the curved lower boundary according to (5.28) from the IVR report and at the center still 0.1 MW/m^2 according to (5.30a) from IVR. With a thermal load from the melt pool of 0.1 MW/m^2 and $Q = 2MW/m^3$ in the crust only 3 cm of stable ceramic crust would result from equations (4.3) and (4.4), with 0.02 MW/m^2 about 5 cm.

No. The scenario described in the report does not lead directly to a rectangular pool. It is rather the final state, evolving through intermediate shapes such as that in TMI. Exactly because the fluxes on the vertical and slanted boundaries are greater, and including the non-coolable nature of these intermediate blockages there will be a gradual expansion of the pool, all the way to the reflector radially, and eventually to the final cold trap at the core bottom, where the blockage can be stabilized by radiation cooling. Neither the 0.1 MW/m^2 nor the 2 MW/m^3 values are appropriate. Nevertheless, even a 3 cm blockage at the lowermost extremity of the core would be more than sufficient to contain the melt pool.

7. *Further, the first blockage should be metallic and a ceramic crust should settle above. Then, the combined system of ceramic and metallic crusts should be considered. This yields a lower bottom temperature, thus lower radiative heat removal. Therefore, the crusts should become even thinner. If, due to heatup of the lower structures, the lower region of the metallic crust remelts and relocates, this yields a further decrease of downwards heat removal from the ceramic crust region, thus inducing further remelting.*

This sequence of events describes one part of the phenomena associated with the pool expansion phase (as discussed in the answer to the previous question). The fact remains that this expansion will be stopped radially by the reflector, and axially by the cold traps at the lowermost ends of the bundles. There, the blockages will be coolable and we have shown them to be so both for metallic as well as oxidic compositions. Because of the tight geometry even a few centimeters of blockage would be adequate to support the molten pool.

8. *Finally, the downwards relocation path appears not yet as surely excluded as stated. It is also to be mentioned here that local melt streams into the melt pool could strongly enhance the local heat transfer to the bottom crust as shown in [1]. Thus, together with the uncertainties of the process of crust formation considered above and the smaller crust thicknesses of the above estimates, local*

inhomogeneities of the crust may become important and may induce local failure at the bottom.

This mechanism is not appropriate here. Here the pool forms from the middle and top with a downwards progression. By the time of interest, when the blockages have reached the lower extremities, there can be no supply of long-duration **cold** streams of melt. Also, the pool is rather deep (compared to the experiments mentioned) and would mix, and stop well before the cold plume could reach the bottom crusts.

9. However, the basic idea that significant cooling potential is provided from the remaining water in the lower head and the massive core support plate is promising. Perhaps, some further calculations related to the above objections could yield further support, But, the steady state consideration for the crust may not be sufficient in general. Calculations on the time-development of melting and crust development with available codes may be necessary for better confirmation.

Following the whole melt progression process, as suggested here, is a very complicated matter, and subject to much greater uncertainty than the basic-principles approach taken in the report. Such calculations may be useful in providing another perspective, but we don't believe we could defend them as the basis of our case. We are aware that this reviewer may have such capability, and would like to see related results be added here for such further support.

10. The main statement is that sidewards melt-through occurs significantly before possible downwards relocation, within the time of ~ 100 minutes during which effective cooling from remaining water above the core support plate is available. The basic statement is that sidewards cooling is much less effective than downwards cooling. The evaluations in the report take the sidewards boundary condition as adiabatic, i.e. no lateral heat removal is assumed. This appears to be a too strong restriction. On one hand, heat removal by the produced steam should be taken into account. On the other hand, heatup of the RPV wall by radiation from the barrel and outside vessel cooling by flooding should be considered. Taking a temperature difference of ~ 500 K over the barrel and reflector and an outer barrel temperature of ~ 1000 K as given from the calculations (Figs. 4.8 - 4.12) nearly half of the heat flux through barrel and reflector could be radiated to the RPV wall (if taken at saturation).

The radiative heat sink to the vessel wall and through it to the outside water was taken into account. Because of the low pressure, steam cooling (through the reflector holes) was found to be negligible.

11. The calculations on core heatup and melting essentially yield the timing for melt pool formation (~ 42 -57 minutes from core uncovery to 20%) to be related to the times for evaporation of water above the lower core support plate and for heatup of reflector and barrel. The further heatup of the ceramic pool and the overlying metallic melt layer resulting from reflector melting as well as the heatup and melting of reflector and barrel is calculated by means of equations (4.10) - (4.15), of which (4.14) has been questioned above (questioning the assumption of rectangular pool shape). With the lateral heat flux in the ceramic pool an additional time of 34-38 minutes is calculated for reflector melting. This is taken to verify lateral melt release at a time with still effective cooling from below (water above lower core plate). But, it has to be remarked again that lateral heat removal is neglected. At melting temperature, at least half of the lateral heat flux could be radiated to the RPV wall (if this is not taken to be superheated sufficiently). Further, heatup and melting of the sidewards ceramic crust as well as of the barrel is not considered.

Both questions (on rectangular pool and lateral heat removal by radiation and conduction) have been responded to above.

12. The considerations on the overlying metal layer resulting from the melting reflector seem not to yield important effects with respect to the final melt release. Although earlier melt-through of the reflector can be expected in this range, this only means that the reflector melt is essentially relocated into the gaps between the reflector and the barrel. But then, the refrozen material must melt again to get break-through.

Not so, because the axial path now is of large dimension, and the relocated metal goes to the bottom of these spaces and builds up from there.

13. Certainly, freezing heats up the still solid reflector and barrel material. But, the material and energy redistribution by these processes may yield some azimuthal homogenization. Thus, the assumption of a local azimuthal failure may not be justified by the considerations on geometrical inhomogeneities in the report. In general, the assumptions on failure locations and size are problematic, although the bounding assumptions appear to be reasonable. In my view, the main objections could be, on one hand, those of above, questioning the exclusion

of downwards failure, and, on the other hand, the exclusion of several failure locations within a certainly short time frame.

Note that once a relocation begins, by local meltthrough, the melt height drops and there is less opportunity of other melt breeakthroughs azimuthally. Rather, we think the path, once opened, will continue to enlarge and melt will be released basically from the same location. This, however, will have to be very gradual. Also, regarding time coherence, as we understand from the mixing explosion dynamics, has to be seen in the few 100s of milliseconds range, while we would have a hard time visualizing coherence even on a time scale of a few seconds.

14. The latter point indicates a further deficit: the further course of melt release is not considered sufficiently. Even with an outflow rate of 400 kg/s (see below) the time of outflow of the whole corium melt pool would last some minutes. During this time, failure could occur at multiple locations, overlapping in time. Further, local melt/coolant interactions could yield additional and also larger failures. A question is whether failures of the bottom of the crust by such interactions can be excluded. On the other hand, the strong voiding with the resulting necessity of early triggering to get explosions restricts the possibilities for coinciding events. Enhanced evaporation of the water pool also acts in this direction.

Subsequent events are obviously impossible to predict in exact sequence, but the train of though explained here by the reviewer is similar to ours. This is that as melt relocation will continue the water pool will remain with a lot of voids, and the water in it will deplete rather quickly. We would take issue only with the statement that failure will occur at multiple locations. As explained above, it is the nature of the process, with the upper 30 to 50% of the core barrel thinned out by melting, once a relocation begins it would tend to remain focused on the same path, enlarging it downwards.

15. Scenarios of ex-vessel reflood are considered with respect to the time of vessel flooding but not concerning the establishment of effective lateral cooling as discussed above. The considerations on vessel flooding before or just about the time of reflector melt-through consider only the cooling aspects and thermal effects of focusing by thin metal layers. But, embrittlement due to rapid quenching may favor failure of the pool surroundings at any location. Further, especially entrapment explosions in the gaps may yield such failures and thus more extensive melt release.

Disagree. We consider all three types of reflood scenarios, and show that the two that are relevant (in terms of their timing) to the melt progression process, actually could lead to melt arrest within the core barrel boundaries. Embrittlement cannot lead to failure under these high temperature conditions. Moreover, as shown in Figure 4.15, in the "fast" scenario, the core barrel and reflector would be cooled well before they could heat up by the melt, and in the "medium" scenario, one would require a totally singular coincidence between core barrel meltthrough and water level traversing the downcomer length.

16. 4. Comments on Breakup and Premixing

From the assumed failure location and size, melt flow rates of 200 to 400 kg/s are estimated, yielding ~5m/s entrance velocity into the saturated water pool at a level in the range of the core support plate. Then, the next main point is in my view the fragmentation process. It is stated that adequately bounding the effect of various degrees of breakup leads to extensive voiding developing rapidly in all cases. This voiding of premixtures is calculated with a 3D version of PM-ALPHA. The melt stream is assumed to be broken up initially into drops of diameter 20 mm ("large enough value to represent a minimally broken-up melt stream"). However, as compared to a coherent stream of ~11 cm diameter (with 400 kg/s and 5m/s), this yields a surface of factor 8.4 higher and correspondingly a higher heat transfer and steam production. Transient breakup could thus yield significantly less steam production. On the other hand, the breakup may then not be sufficient for explosive premixtures. A factor of 6 still results for two jets of melt with correspondingly smaller diameters which facilitates breakup again to some extent. Thus, mixtures with smaller void may result from transient breakup and assuming several jets with smaller diameters. On the other hand, there remains certainly a limitation to breakup due to the time consuming process of breakup.

In my opinion, these contrary effects with respect to getting an effective mixture, i.e. too less breakup or too strong voiding with stronger breakup, should be explored more for getting the inherent limitations to explosive mixtures. Although the statement of strong steaming appears to be plausible, it may not be possible to demonstrate it for all possible cases, as indicated above. A smaller window for explosive mixtures may become plausible taking into account the above effects of time requirement for breakup and too coarse breakup combined with weaker voiding. Perhaps, some additional variations with PM-ALPHA could be done to

show this, e.g. by considering plausible time laws for breakup of coherent jets together with varying breakup length scales.

Indeed, the THIRMAL calculations give some perspective on this, showing the extreme cases of little stripping of small fragments for a thick jet (7.3 cm diameter) and coarse breakup for the smaller jets (2.9 cm and 1.8 cm) due to long-wave instabilities. However, certainly cases in between these extremes of breakup behaviour should be considered. Further, the present state of jet breakup modeling cannot be taken as verified. This is also indicated by the significant differences between results based on Kelvin-Helmholtz instabilities and on the theory talcing into account velocity profiles (Miles) which have been obtained with IKEJET, e.g. [2].

Since multiple jets may occur from one hole by some separation effects (e.g. connected with the failure mode) or from several failure locations, the restriction of mass assumed in Appendix D, p. D-6 for the case with thinner jets appears not to be justified. Also, the concluding statement of p. 5-12, "that both length scales and void fractions are well encompassed by the PM-ALPHA calculations" appears to me as too rough, in view of the variations of cases indicated with the THIRMAL calculations and considered above. On the other hand, I agree in principle to the expectation of strong voiding based on the situation considered, with melt into saturated water and with the necessity of breakup for explosive mixtures. Further variations may even better demonstrate this, as indicated above.

This is really an open-ended question. The key point, however, is that one cannot have simultaneously enough interfacial area for a strong explosion and low enough void fractions to produce energetics. This was amply demonstrated by the calculations made already. We have made some additional calculations, and we have provided further interpretations on this compensating mechanism in the addenda to Chapters 5 and 6.

17. I think, this could also be done for the situation of bottom failures of the melt pool, excluded in the report.

We also believe that bottom failures could not jeopardize lower head integrity, but prefer to not open up this direction of thinking **arbitrarily**; that is, without a viable blockage failure mechanism.

18. The exclusion may also be better based by considering additionally the cold trap properties of the lower spacer grid and the Zr plugs quantitatively for

conditions after boil-off in this region and with melt relocation to this region before lateral melting of reflector and barrel occurs (or: improved considerations on the timing of the events, with respect to the above discussions).

The explicit consideration of blockages in the Zr plug region is now provided in the addendum to Chapter 4.

19. 5. Comments on Explosion, together with Premixing

A trigger of sufficient strength is applied to the mixtures in ESPROSE calculations to quantify explosions. The chosen trigger appears to be sufficiently strong to produce strong escalations as in the KROTOS experiments, but its strength is not assessed with respect to possible trigger strengths. I agree that with a sufficiently strong trigger the escalation dynamics may no longer depend on the trigger strength (if overdriven cases are excluded as unrealistic). Thus, the results of the numerical tests may indicate such a limiting strength and no need for further variation. In view of the effects in the KROTOS experiments, the chosen trigger can also be considered as strong enough to yield major effects. Looking at early rather than later times for bounding the effect of trigger timing appears also appropriate in view of the strong voiding (excluding other possibilities of melt release as discussed in the previous chapters).

* *

20. The results given in the report show significant differences in the maximum local pressures, the maximum impulses as well as loaded areas and times of loading depending on the chosen melt mass flow, the breakup behaviour and the time of triggering. E.g., with the higher melt flow a maximum pressure > 5000 bar, impulse 100-120 kPa·s and maximum area of 5 m^2 results choosing β = 10 for fragmentation and an instant of triggering at 0.05 s (case C2-10(0.05)), as compared to nearly 104 bar, 100-200 kPa·s and 3.5 m^2 for β = 20 and 0.12 s (case C2-20(0.12)). On the other hand, there seem also to be similarities or bounding trends. E.g., for case C1-10(0.05) with the lower melt flow, b = 10 and 0.05 s, the results are rather similar to C2-10(0.05), with somewhat smaller impulses and areas in C1-10(0.OS). But small shifts in trigger time give also strong differences, e.g. pages C.3- 1 6 to C.3-20 in the report for case C2-20. The same is valid for the comparison of C2-10 and C2-20 with similar trigger times (pages C.3-13 and C.3-16). This is certainly due to the relation between time

development of breakup and voiding, producing optimum mixture configurations at different times. This is one cause of uncertainties in getting explosive events or not (together with triggering time).

Concerning this problem of sensitivity, the large number of calculations performed is convincing. They yield maximum events but in a limited range and not as singular cases. Some questioning I still have with this respect concerns the choice of β for premixing breakup and perhaps the underlying time law of breakup (not given in the report). Since the maximum loads appear to be obtained with case C2-20(0. 12) as compared to the lower β, it is not quite convincing to jump to nb and not to consider cases between. Other time dependences may yield further variations. This concerns the questioning of above concerning the premixing process as well as the melt flows.

See addenda to Chapters 5 and 6 for further insights into relating premixture characteristics to resulting energetics. Through these interpretations a more coherent picture regarding the origins and implications of the relatively narrow "explosion-sensitive" region can be gained.

21. Concerning the latter point, it is to be remarked that the main effect of multiple melt streams into the water - if taken as saturated - would be that a larger region is loaded by the explosion (perhaps also some further escalations in more extended premixtures may be possible, this could be checked by ESPROSE calculations) and that thus the venting will be further limited. Also the pressure relief in the vessel wall will then be limited. Thus, it is important to further confirm the exclusion of such multiple events (small windows for this!) or to check the coincidence effects.

Actually, our pour characteristics correspond to multiple melt jet streams, rather closely spaced within the lateral space dimension assumed in the meltthrough. To widely separate these jets would be tantamount to assuming a much larger azimuthal coherence, which as discussed above, we do not consider appropriate.

22. It remains to formulate some general questioning concerning the verification state of PM-ALPHA and ESPROSE. Although a lot of work has been performed on this, I think that severe questions remain. Even if numerical aspects may be considered as well established, also with respect to 3D, there remain open areas concerning the physical formulations. These are e.g.:

• *Checks with MAGICO were - to my knowledge - restricted to relatively small volume parts of spheres.*

See addendum to Appendix B of DOE/ID-10504.

23. • *In general, correlations for exchange processes in three phases are uncertain and need further clarification.*

We have an extensive data base on film boiling in steam-water flows, and a rather elaborate non-local radiation transport model. Also, our drag laws are well based on existing fundamental knowledge. Further, a wide range of tests on the multifield aspects show that the code performs well. Of course, there is always room for further developments, but the issue here is whether the reviewer sees any specific limitations or concerns.

24. • *The uncertainties on jet breakup have already been mentioned above.*

Our approach is specifically based on explicitly recognizing, and bounding, these uncertainties.

25. • *The microinteractions formulation for hydrodynamic fragmentation in thermal detonation waves needs further clarification and development, for single drop a well as finally for drop assemblies. This concerns the conclusions based on theory and on single effect experiments as well as on KROTOS experiments.*

This was already recognized in the report, leading us, therefore, to take a conservative approach in the microinteractions parameters.

26. *With respect to FARO and KROTOS analyses, different premixing and explosion codes have shown the capabilities to calculate the experimental behaviour. But, the underlying physical formulations and thus the physical interpretations differ strongly. Further, even no convincing comprehensive understanding has up to now been gained on the differences especially in premixing behavior between UO_2/ZrO_2 and Al_2O_3 in KROTOS. Thus, further work is necessary to get approved understanding, models and codes. In general, the results would be more convincing if supported also by other codes based on a common physical understanding and corresponding formulations.*

Clearly, this is so, but we cannot be responsible for the codes of others. KROTOS was not designed and is not appropriate for understanding premixing behavior. What is needed is better diagnostics on the premixtures triggered (not predictions) so that predictions of energetics can be made on a more secure basis.

27. 6. Comments on Reflood Scenarios

As already remarked under 3. of my comments, the ex-vessel reflood should also be taken into account with respect to the considerations on the cooling aspects determining the conclusions on the relocation path. In the context of vessel reflood also the possibility of embrittlement and thermal stresses favouring failure should be considered. It should be shown that also under these conditions larger melt release and in this case contact with subcooled water is avoided or not threatening. Entrapment explosions, e.g. in the gaps between reflector and barrel, should also be addressed concerning a possible increase in failure and melt release.

This was addressed in the questions above.

28. Concerning the interaction of reflood water with melt pools, I agree to the argument of rapid small-scale interactions, rapid solidification and in general small effectivity of stratified explosions. However, it should be addressed whether relevant effects of mixing due to the water impact and due to small-scale interactions (also taking into account the falling-back of expelled water) can be excluded. The situation of reflood under conditions of still existing melt/water mixtures in the lower head may be even more important than the extreme of the melt pools in the lower head, if the reflood water could enhance mixing again, collapse steam, favor further melt release by the above processes, and this under the conditions of already settled melt, i.e. thermal load at the bottom.

Reflood scenarios in this pressure reactor are not arbitrary in their timing, but rather well-defined, as explained at the end of Chapter 4 and in Chapter 8. On this basis we have considered all that needs to be considered.

29. 7. Some Comments for Formal Improvements

Some typing errors of relevance are given below (I had no time for further detailed checking), together with some need for detailed descriptions:

- *Ra' in Nomenclature: factor g is missing, g also missing in Nomenclature.*
- *P. 4-6, second line from below: 0.2 MW/m^2 instead of m^3.*
- *P. 4-18: effective power density .. of 0.26 W/g: does this mean of core material in contrast to fuel?*

- *P.4-21, eq.(4.14):* $Ra'^{0.095}$.
- *P. 5-2: giving the breakup law would be helpful.*
- *P. 5-4: some further informations on this and on the other color figures would be helpful, e.g. length scales, quantities of melt volume fraction.*
- *P. 5-5: z directed to top, but in subsequent results inversely.*
- *P. 5-10, 13th line from above: "melt" instead "coolant".*
- *P. 5-10, second line from below: "two slower pours" - seem to be better characterized by pours with smaller diameter.*
- *P. 6-1, second line from below: "propagation intensity is basically independent of the magnitude of the trigger." - It is not quite clear to me what is meant, e.g. with "propagation intensity".*
- *P. 6-1, end of second paragraph: (Theofanous et al, 1996a)?.*
- *P. 6-4: 6th line from below:* $0.1m^2$
- *Fig. 6.2: should be better characterized: length scales, pressure scales*
- *Fig. 6.4: locations are identified in Fig. 6.2?*
- *Fig. 6.5: location of peak loading: where?, not included in 6.4!*
- *P. B.1-1 etc.: color characterization is not quite clear to me: fuel-void, red lines, blue isolines in jet.*
- *Appendix C: color pictures: case? characterizations?*
- *choice of parameters in the ESPROSE calculations, especially concerning micro-interactions?*
- *P. C.3-13 etc.: locations?*
- *P. D4, 6th line from above:* R_{zone} *instead* $R^2_{zone} \leq R_1$

All typos have been corrected and requested information was supplied to the text. "Propagation intensity" refers to the strength of the explosion. We appreciate the reviewer's efforts.

30. References

[1] Mayinger, F. et al.: "Untersuchung thermodynamischer Vorgänge sowie Wärmeaustausch in der Kernschmelze. Teil I: Zusammenfassende Darstellung der Ergebnisse." Institut für Verfahrenstechnik, Technische Universität Hannover, Abschlußbericht BMFT - RS 48/1, July 1975

[2] Bürger, M., Cho, S.H., v. Berg, E., Schatz, A.: "Breakup of Melt Jets as Precondition for Premixing: Modelling and Experimental Verification." Nucl. Eng. and Design 155 (1995) 215-251

* *

F.4. Response to T.A. Butler (LANL)

General Comment and Highlights

Two reports were provided. The first addresses the fragility and indicates that the portions of the curves above the threshold failure (10^{-3} probability) level may rise faster than given in the report, because we neglected progressive failure effects (through the wall thickness). The second report examines the loads in relation to the fragility and concludes that, since the two do not intersect, the above criticism on the fragility does not affect the conclusions of the report. Based on this, the present response does not address this criticism. We plan to carry out the kinds of calculations suggested and will include the results in an addendum to Chapter 3, in the final report. Also, this reviewer provides evidence that the appropriate yield stress is 450 MPa (rather than the 330 value utilized), and that the strain rate effect may not be as strong as taken in the calculations. These variations are mutually compensating.

Point-by-Point Responses

1. INTRODUCTION AND SUMMARY

The comments contained in this review are restricted only to a review of Section 3 of the subject report. The report's authors have done a good job of scoping the possibilities of failing the lower vessel head under the assumed loading conditions. Well established analytical approximations were used to establish the validity of the finite element model that was developed to study local failure of the head. A more detailed model needs to be developed to include transverse shear effects and to simulate failure of damaged elements during the course of the calculation. This lack of simulating progressive failure is the weakest point of the analysis. Appropriate simulation of progressive failure has to be included in order to obtain defensible results that can be included in probabilistic evaluations.

SPECIFIC COMMENTS

Finite Element Model:

Use of the shell elements in ABAQUS is acceptable for determining the distribution through the thickness of all components except for transverse shear. In the ABAQUS thick shell elements transverse shear is approximated by constant shear through the section. This is not judged to be adequate for evaluating the possibility of a shear type of failure. A better method for getting good approximations for all of the strain components would be to use several continuum elements through the thickness rather than the thick shell element. Use of many more elements

would make the runs longer, but use of the explicit version of ABAQUS would help in this regard (see below). In addition, the use of an axisymmetric finite element model would afford the opportunity to use a much more dense mesh in the analysis with run times that are still relatively short. The structure and all of the loads that were considered are axisymmetric.

In fact, our main interest is for non-axisymmetric situations, and many of our calculations were **not** axisymmetric.

2. The mesh should be considerably more dense in order to resolve fine details in the strain distribution, especially those details relating to strains other than in-plane strains. Referring to Figures 3.5a-c, even the in-plane strains vary from their maximum levels to just half that level over just one or two elements.

Although not stated in the report, I assume that the implicit version of the ABAQUS code was used for these calculations. The implicit version is always stable but may not always be convergent. There is no indication in the report as to whether the time step was varied to ensure a convergent solution. A better alternative may be to use the explicit version of ABAQUS for the short transient solutions that are required for the types of loads being considered here. The explicit version of ABAQUS also offers the opportunity to use a failure model that would give more realistic failure predictions (see below).

We used the implicit version, and considered convergence — see addendum to Chapter 3. This is now noted in the report.

3. The statement that the time duration of the loads is less than the natural frequency of the head may not be correct. A handbook solution of the frequency of a full sphere with the same dimensions as the hemispherical head gives a natural period of 1.5 ms, very near to the 2 ms pulse duration used in this study. It is no wonder that, as stated, the impulse time is "non-negligible."

Our point was the same (i.e., that it is not much greater), and for the same effect. To avoid confusion, we changed the "less" to "similar." Also, the statement is true for the actual loads in Chapter 7.

4. Load-Strain Behavior:

Use of the Bodner and Symonds approximation for the dynamic yield stress is a reasonable approximation. However, use of the values assumed for the constants

D and p should be justified more thoroughly. The values used here are for mild steels, and may not be appropriate for the alloy steel that is used in the pressure vessel being evaluated. Obtaining a good approximation for this relation is particularly important because the maximum strain is very dependent upon it. I used an axisymmetric model with 15 continuum elements through the thickness to replicate some of the calculations in the report. The results showed that the maximum strain went from 0.52 to 0.16 with addition of the rate model for the yield stress. Considering the magnitude of this difference, one should certainly be very careful in the selection of the rate parameters.

That the effect is strong was discussed in the report, as was our choice of the lower yield stress also (300 as compared to the as tested 450 MPa value), as a contingency in lieu of directly applicable data. In the addendum to Chapter 3, we show results using 450 MPa, with and without rate dependence.

5. Dexter and Chan (1990) address the effects of strain rate and temperature on A533B steels. This alloy is close to A508 steel and may provide some useful information in developing an appropriate dynamic yield stress model.

These data suggest that the rate dependence is not as strong as in our original calculations. All we can do is bound the behavior, as described in the response to item 4 above.

6. Failure Criteria and Fragility:

This is probably the most difficult aspect of modeling the response of the vessel head. The failure criterion that is used in the report is probably realistic and conservative except for one important aspect. The model, as reported here does not remove the load carrying capability of elements that have exceeded the failure criterion. Maintaining the load carrying capacity of damaged elements can give significant over-estimates of the capacity of the structure. I used the explicit finite element model mentioned above to look at this aspect of the problem and found that, depending on the parameters used for the ABAQUS failure model, the head could fail for the loads that are reported. I strongly suggest using some sort of failure criterion embedded in the computational model for future calculations.

Calculations with 3D bricks are underway and will be included in the final report, to refine the fragility as appropriate.

7. The subject report briefly mentions the effect of stress anisotropy on the failure strain. This is an important issue and needs to be more fully evaluated. The work referenced in the report by Pao and Gilat was performed on Charpy bars (roughly uniaxial strain) and by Shockey et al. was performed in pure shear (no hydrostatic component). Therefore these data don't address the important effects coming from multi-dimensional stress fields. Data summarized by Ju and Butler (1984) show that A533 alloy steel when in equal biaxial tension fails at an equivalent strain equal to about one third the strain for uniaxial tension. Equal biaxial tension is the stress state at the "pole" of the lower head where failure would first be expected. The alloy content of A533 steel is similar to that of the A508 steel considered in the subject report. Mirza, Barton, and Church (1996) reported the effect of the stress field on failure strain and its effects in transitioning from ductile to brittle failure characteristics. Johnson and Cook (1985) also discuss the effects of the stress field on fracture of ductile metals. Other references that may be of help include Jones and Shen (1993) and Ferron and Zeghloul (1993).

This again refers to refinement of the fragility and will be addressed in the final report.

8. As previously mentioned, the head would have to be modeled with continuum elements to accurately predict transverse shear strains. In addition, a failure criterion for transverse shear needs to be established. It is unlikely that the failure criteria discussed in the above references are adequate. They may however give some guidance in establishing the appropriate criteria. It is possible that when the loading conditions are investigated more closely, the load cases that lead to the highest shear load (such as case l+) can be eliminated obviating the need for this criterion.

Calculations are underway to refine fragility with regards to transverse shear strain effects.

9. Miscellaneous:

1) The use of the higher yield stress 450 MPa is justified based on actual data from Server and Oldfield (1978) where the average yield stress is approximately 440 MPa for A508 steel (very close to the Japan Steel Works Ltd, value of 450 MPa). This is one parameter with ample data to support the use of the actual, as-tested value.

This comment bringing in the Senser-Oldfield data is very helpful. The additio.ıal margins gained by using 450 MPa, instead of 330 MPa, is discussed in an addemdum to Chapter 3.

10. 2) The statement is made that A533B steel has a carbon content of 0.19 vs 0.16% for A508. Information from Server and Oldfield (1978) and the ASME Code show that A533 has a carbon content of 0.25% maximum and A508, Class 3 has the same upper limit for carbon content. Actual analyses show carbon content from 0.21 to 0.25% for both steels.

* *

11. 3) Chapter 3 in the subject report does not mention radiation embrittlement effects. If they can be dismissed, the reasons should be given.

4) For SA508 the transition from ductile to brittle behavior starts at about room temperature. The report should give the approximate material temperatures during the postulated event to show that it is well above room temperature.

Yes, the end-of-life RTNDT for the AP600 steel forging at the beltline region is specified as 23 °F. The lower head, less irradiated, would be even better than that. At the time of interest, the lower head would be between 50 and 100 °C.

12. 5) The presence of flaws is not addressed. I assume that in service inspection will have identified any that are significant in affecting ductile fracture.

Yes.

13. References

Dexter, R. J. and K. S. Chan, "Viscoplastic Characterization of A533B Steel at High Strain Rates," Journal of Pressure Vessel Technology, Vol 112, (1990) 218-224.

Ferron, G. and A Zeghloul, "Stain Localization and Fracture in Metal Sheets and Thin-Walled Structures," in Structural Crashworthiness and Failure, N Jones and T. Wierzbicki, Eds. (Elsevier Applied Science, London and New York, 1993), Chap. 4, pp. 131-163.

Johnson, G. R. and W. H. Cook, "Fracture Characteristics of Three Metals Subjected to Various Strains, Strain rates, Temperatures, and Pressures," Journal of Engineering Fracture Mechanics, Vol. 21, No. 1, (1985) 31-48.

Jones, N. and W. Q. Shen, "Criteria for the Inelastic Rupture of Ductile Metal Beams Subjected to Large Dynamic Loads," in Structural Crashworthiness and Failure, N Jones and T. Wierzbicki, Eds. (Elsevier Applied Science, London and New York, 1993), Chap. 3, pp. 95-130.

Ju, F. D. and T. A. Butler, "Review of Proposed Ductile Failure Criteria for Ductile Materials," Los Alamos National Laboratory report LA-10007-MS (NUREG/CR-3544), April 1984.

Mirza, M. S. and D. C. Barton, "The Effect of Stress Triaxiality and Strain-Rate on the Fracture Characteristics of Ductile Metals," Journal of Materials Science, Vol. 31 (1996) 453-461.

Server, W. L. and W. Oldfield, "Nuclear Pressure Vessel Steel Data Base," Electric Power Research Institute report EPRI-NP-933, December 1978.

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14 Letter dated January 8, 1997 to L.W. Deitrich.

The purpose of this letter is to clarify the appliability of comments I made in the attachment to my previous letter to you dated December 1996, regarding review of the report entitled "Lower Head Integrity Under In-Vessel Steam Explosion Loads," by T.G. Theofanous, et al.

The comments made on that attachment were limited to Chapter 3 of the subject report and, consequently, affect the fragility curves developed in that chapter The fragility curves are subsequently refereed to in reaching conclusions in Chapters 6 and 7 of the report. It is important to make clear, however, that the fragility curves in question do not have major effect on the conclusions reached in those chapters. The loads developed in Chapter 6 and applied in Chapter 7 are low enough that the vessel response is definitely below the lowest probability level used in defining the curves (10^{-3}).

I should also point out that I concur that the probability levels used in developing the fragility curves are conservative. The association of these levels with strain magnitudes through the vessel wall are acceptable. However, the calculated strain levels used to develop the detailed curves above the 10^{-3} *level may not be conservative. If the curves are ever used for evaluating higher loads; they should be reevaluated based on the review comments that I previously submitted. Implementation of the information in these comments will affect the shape of the*

curves and could shift them toward lower levels of impulse load (to the left in figure 3.11 of the subject report).

If I can be of further help please do not hesitate to contact me.

* *

F.5. Response to D.H. Cho (ANL)

General Comment and Highlights

This reviewer finds that additional supporting work is needed before the results can be used in the licensing area. The review process, by comments and responses, has produced additional supporting work. Specific issues raised by the referee relate principally to scenario aspects such as the possibility of "secondary" explosions through a downward relocation path, premixing at a higher pressure, and reflood FCIs. These are addressed point-by-point below

Point-by-Point Responses

> *1. In response to the request made in your letter of June 17, 1996, I have reviewed the report "Lower Head Integrity Under In-vessel Steam Explosion Loads" by T. G. Theofanous et al. You indicated that this report and a companion document together "intend to demonstrate the effectiveness of 'in-vessel retention' as a severe accident management concept for a reactor like the AP600". You further indicated that "the purpose of this review is to assess whether this intent has been achieved to a sufficient degree for the results to be of use in the regulatory/licensing area". Based on my review of the report, I find that additional supporting work would be needed if the conclusions of the report were to be used in the regulatory/licensing area.*

The nature and need for the "additional supporting work identified are addressed, point-by-point, below.

> *2. On page 9-1, the authors state that "Methodologically, the assessment involved only a slight scenario dependence, principally on the permanence of the blockages preventing direct downward, through the lower core support plate, relocation", and that thus the assessment is of Grade B, in the ROAAM scale. I think the scenario dependence is more than slight, so the assessment may be more of Grade C than Grade B in the ROAAM scale.*

Disagree. Scenario dependence concerning uncertainties and a complex, long evaluation. Here we have a well-defined behavior and a robust assessment for it.

> *3. Suppose a steam explosion would take place in the downcomer region or in the lower plenum, as described in the report. The explosion may not be strong enough to fail the lower head, but it may be energetic enough to mechanically disrupt the blockages formed at the lower end of the core.*

The scenario proposed by the reviewer is not reasonable. First, there is no water in the downcomer. Second, an explosion cannot annihilate the lower blockages.

4. Further, the explosion would likely expel some water from the lower plenum so that the lower core support plate may no longer be in contact with water (i.e., the ability to cool the core support plate would be lost).

As noted in the report, the heat sink of the core support plate is good for at least. Once the melt relocation begins, it will continue, vaporizing the rest of the water in the lower plenum, before failure of the blockages are possible. This was discussed in the report (see p. 4-3).

5. *Thus, the initial explosion, while not failing the lower head, could severely weaken the blockages mechanically as well as thermally. It would seem possible that a relatively small initial explo-sion would be followed by a massive downward relocation of core melt through the core support plate, setting the stage for a secondary explosion probably involving a much larger melt mass. The lower head may well survive such a secondary explosion, but a separate assessment of this possibility would definitely be needed.*

Based on the above, this kind of behavior is not physically reasonable.

6. *Based on the code calculations performed, the report concludes that the saturated coolant condition in the lower plenum leads to highly voided premixtures that have a dampening effect on the resulting explosion energetics. While I am not judging the validity of the calculations, I find it difficult to reconcile this conclusion with available experimental evidence. Experience tells us that triggering of a steam explosion would be more difficult with saturated water than with highly subcooled water. However, once triggered, the explosion energetics does not seem to depend on the coolant temperature that much. Consider, for example, the results of the KROTOS tests Nos. 28, 29, and 30 [H. Hohmann et al., "FCI Experiments in the Aluminum Oxide/Water System," Nucl. Eng. Design 155 (1995) 391-403]. In these tests, approximately 1.5 kg quantities of Al_2O_3 melt at 2300-2400°C were poured into a column of water and steam explosions took place. In KROTOS 28, the water was nearly saturated at 87°C while in KROTOS 29 and 30, the water was highly subcooled at 20°C. The energy conversion ratio was estimated to be 1.3%, 0.8%, and 1.25%, respectively, for KROTOS 28, 29, and 30. It thus appears that the explosion with the nearly saturated water was at least as energetic as those with the highly subcooled water. Similar findings*

regarding the effect of water temperature on the explosion energetics were also made in our recent ZREX experiments. Such experimental evidence would need to be considered when discussing the explosion energetics.

We wrote saturated, and we mean saturated. All KROTOS tests were subcooled. Due to the peculiar mixing condition, timing, and relatively low temperature (compared to reactor materials) all three produced premixtures relatively low in void. The distinction made here is not between 10 and 30% void fractions, but void fractions going to 60–90%. Empirical considerations, on conversion ratio, such as those offered here by the reviewer, cannot get us too far one way or another. It should be clear, however, that our methodology is consistent with the findings of the KROTOS experiments, as shown in DOE/ID-10503.

7. Perhaps additional parametric calculations in terms of the breakup and trigger timings might be useful.

We cannot understand this comment. If the reviewer can imagine a trigger time and/or β that is not bounded already, he needs to state it, so we can respond. See also addenda to Chapters 5 and 6.

8. In all supporting calculations, the water was considered to be saturated with the primary system completely depressurized to 1 bar. Even in a large-break LOCA, the containment back pressure would remain in the range of 2-4 bars for a long period of time. It would appear that a system pressure somewhat higher than 1 bar (e.g., 3 bars) would have been more realistic for the supporting calculations.

This is not correct. In AP600, at the time of interest, containment pressures cannot exceed $\sim$1.7 bar. In any case, to provide some perspectives on the effect of pressure and subcooling, we provide some new results in an addendum to Chapter 5.

9. Reflood FCIs were discussed in Chapter 8. I suspect that reflood FCIs in stratified configurations would be of secondary importance compared to the premixed explosions addressed in the rest of the report. Nevertheless, reflood FCIs need to be considered for completeness, particularly in view of the potential for vessel wall thinning due to chemical attack by the metallic melt. The authors should be commended for making an effort here. I would have to say that this effort represents a best-estimate assessment based on engineering judgment. At present, there is no adequate database or computational tool for large-scale stratified explosions.

Disagree. The assessment is based on basic principles. Simply put, you cannot create any significant impulse with an inertia constraint of a few inches of water!

10. On page 7-1, the authors state that "Also in this chapter, we would normally present a series of arbitrary parametric and sensitivity calculations, to illustrate, for cases where the base results happen to be benign, the margins to failure" and claim that "This, in effect, has already been done, too, by the breakup and triggering calculations, in the course of bounding the behavior". I believe additional work would be needed to make this claim fully valid, and I am confident that the authors will succeed in doing that.

If, notwithstanding the above, the reviewer has specific suggestions for further calculations, we can consider carrying them out.

11. Finally, the authors are to be commended for conducting such a detailed evaluation of a very complex issue.

* *

F.11. Response to M. Corradini (U Wisconsin)

General Comment and Highlights

The principal concern of this reviewer is about the melt release conditions. Other reviewers have gone into this type of question to a much greater extent, and our responses to them may be useful here too.

Point-by-Point Responses

1. *COMMENTS and QUESTIONS for DOE Report*

1] The authors do a good job in giving a context for their work. However, I am not sure if this analysis which is provided is a failure analysis for the AP600 reactor pressure vessel or a design analysis for the RPV. The former implies that it would be a 'best-estimate' analysis, while the latter must account for factors of safety to assure survival. The authors need to clarify this.

This is **not** a design effort. The approach is adequately explained in Appendix A.

2. *2] In section 3 the authors define the failure criteria and the fragility curve for the reactor pressure vessel. If I understand the approach a strain-failure limit is used and the associated analysis suggests a RPV lower wall failure probability ranging from 0 to 1 for a spectrum of loading patterns with impulses between 200 - 400 kPa-sec. Currently, I wonder how this failure envelope compares to that of previous LWR plants analyzed for an in-vessel steam explosion; e.g., the ZIP study in the early 1980s by Sandia and Los Alamos Nat'l Labs?*

What we could learn from the previous work we mentioned in the Introduction. In those days the wave dynamics were not available to the structural analysts.

3. *3] In section 4 the authors' major point is that the core and vessel design is sufficiently different from past LWRs, such that the core melt behavior is quite different. Two aspects are emphasized: first, the lower core support plate and the non-active fuel length above it [30cm] is large enough in size to delay the core melt progression downward; second, the core steel reflector in the radial direction is also thicker [over 15cm], also delaying and changing the details of radial core melt progression. In essence, the 'race' to the breach of the corium melt crucible, which is formed during the meltdown, downward or radially outward is governed by these boundaries. The authors use a specific 3BE core melt accident sequence*

to illustrate this behavior. If one accepts this premise about a radically different core geometrical design, a few questions arise:

a] What is the sensitivity of meltdown timing to downward boil off of water? More examples are needed.

None. The 100 minutes is calculated **after** the water has reached the bottom of the active fuel region (from then on it is heated only by radiation). This is conservative.

4. *b] Is the core melt event timing essentially independent of accident sequence? No guidance is given here.*

Yes. In this passive plant there is an essential "collapse" of sequences, as discussed in the report and in DOE/ID-10460.

5. *c] The corium exit flowrate seems to be set by the 'rip' in the reflector along the radial edge of the core region at the very top of the pool. Is this a realistic estimate, since it is not much more than that one would calculate from adiabatic heatup and meltdown of the core; e.g., as evidenced at TMI2 ? The authors suggest that 200 to 400 kg/sec "appears to be a reasonable range physically to bound the behavior"; but I wonder if we really know that much about this core melt failure progression in a radically new geometric design that this flowrate is a 'reasonable bound'? More justification is needed for one to 'buy' the argument.*

The basis in quantifying this intangible factor has been explained. Some judgment is required here. It really has nothing to do with details of melt progression or the "radically new geometric design." It is a question of how much of the core barrel area can melt through **coherently** in a time frame of a few 100's of milliseconds.

6. *d] This last question really leads me to the key question of this whole analysis; i.e., the authors leave me with the impression that there is a good deal of certainty in the melt progression and I have significant trouble accepting this premise. Specifically, the whole analysis hinges on the fact that the melt crucible which forms during the melt progression has a structural integrity of enough certainty that it would release the melt radially through a pour area no larger than 0.02 to 0.04 sq. meters. This estimate also seems to be robust enough that it would be a "bound" even with coolant reflood into the core region and any possible disruptive events that may occur. I am very dubious about this and would need to see more analysis to accept this as a 'reasonable bound'.* **This**

melt failure location and pour rate is the key determinant in limiting subsequent FCI energetics.

Again, see response to the previous question. Also, the reflood scenarios in this passive plant are pretty well defined, as addressed in Chapters 4 and 8 already. It would be helpful to know what aspect of the analysis is considered doubtful and why.

7. 4] In section 5, the authors detail their multidimensional premixing analyses. As stated previously, the melt flowrate of 200-400kg/s seems to predetermine the benign nature of the FCI energetics, but mixing is also part of the process. A few questions arise here:

a] Why has the effect of RPV pressure been neglected? Premixing will occur at elevated pressures not 1 bar [like 2-5 bars] and this will affect the mixing process. Also, the rise in pressure locally during mixing will cause the pool to subcool and this has been neglected. Were calculations done to 'bound' these effects?

For the AP600, the relevant pressure range is 1 to 1.7 bar. Calculations were run for 3 bar (see addendum to Chapter 5). Any subcooling, and its consequences are automatically taken into account in PM-ALPHA.

8. b] The authors seem to have only considered the premixing process as the melt falls through the limited water pool from its surface to the curved RPV bottom. Would not mixing continue as the melt continues to fall along the wall. This seems to have been neglected. Is this premixing process of no importance or is the premixing analysis with PM-ALPHA not valid for these longer times?

The premixing results presented were not taken as far out due to time constraints. We now have extended these results to much longer times (see addendum to Chapter 5).

9. c] The biggest effect of these small pours in my mind is that they may cause local FCIs which do not harm the RPV but totally change the melt pouring characteristics for subsequent melt pours; i.e., these small pours and associated FCIs will damage the core melt crucible and markedly increase the melt flowrate or change its location. The authors have gone to great pains to determine the fragility of the RPV wall, but totally ignore the fragility of the melt crucible and the effect of these FCIs. I would suggest that larger melt pours will be induced from the bottom of the crucible as well as along its radially edges with larger

holes, all caused by early small FCIs. How have these events been considered or conservatively bounded for RPV survival?

Subsequent events are very complicated, of course, but there are substantial solid barriers, even if the blockages were to structurally fail. Moreover, much of the water in the lower plenum would be expelled by the same forces (if present) that are considered to fail the crucible.

10. d] Finally, the PM-ALPHA model has a parametric fuel breakup model that is mentioned briefly, but has yet to be assessed against experiments. For these small pour rates, the model effect is not of great interest, but would be for larger pours in these complex geometries.Is this model discussed in the support documents?

See PM-ALPHA verification report.

11. 5] In section 6, the authors use ESPROSE.m in a revised 3D version to simulate theexplosions within the RPV lower plenum. Given the premixed mass of fuel we have a range of results given in Table 6.1. Only a couple of questions arise:

a] Why is the trigger time so short; i.e., much less than 1 sec? Is it due to the time to the RPV wall?Why cannot further mixing along the RPV wall cause larger explosions?

Because we determined that there is a peak (in time) in the explosive "quality" of the premixtures. See also addenda to Chapters 5 and 6.

12. b] Why is the impulse largest for the mid-range value of 'beta'? Is the impulseof 200 kPa-sec near the failure limit? or am I reading this prediction correctly?

Again, this relates to the explosive quality of the premixtures, as explained in the addendum to Chapter 5. Not at all. The 200 kPa·s case is not near failure. This is because the impulse is highly localized, as explained already in the report. Also, as explained in Chapter 7, for the actual loading, the vessel remained within the elastic limits.

13. c] The detailed calculational results in Appendix C abruptly stop in many cases at 1 or 2 or 3 milliseconds. Why? Is this an indication of something numerically fatal in the ESPROSE.m simulation or what is up?

Again, time limitations caused us to curtail some calculations that were not very interesting. More complete results can be found in the addendum to Chapter 6.

1. *INITIAL COMMENTS and QUESTIONS for DOE/ID-10504*

The overall report is quite informative, but I do have specific comments/questions that need to be addressed.

1] The analysis of the QUEOS experiments are very interesting. For any of the experiments [Q5, 6, 8, 10, 11] the visual image is compared to the code, and the leading edge, level swell, steaming rate, steam produced and pressure is compared. My first question is what is the criterion to determine the leading edge? In the pictures for the tests, specifically Q10 and Q11 it seems to me that PM-ALPHA is predicting the movement of the front to be faster than the data indicates. Yet in the plots the opposite is represented. Either there is a contradiction or I am observing numerical diffusion in the images and the researchers have a definition of the leading edge that "corrects or compensates" for this. I have seen the same behavior with IFCI and therefore, am sensitive to it. This needs to be sorted out before I would say that the agreement in the kinematics is acceptable. The MIXA results in Section 3 seem to indicate the same behavior to me and thus I am worried about this numerical diffusion. There was also no study of the nodalization effects in Section 4 and this is surprising given the results in Section 2. This seems to be a logical thing to do and really should be done.

As stated in the report the predicted "front" was obtained from Lagrangian tracer particles. Also as stated in the report, numerical diffusion can be controlled by the grid size, and results with still finer grids were promised, to improve the already quite good results.

2. *2] The second comment about QUEOS relates to the radiative heat transfer model. On page 2-16 the report states that the radiative model had to be changed from what is normal in PM-ALPHA to accommodate the experiments. Later on page 2-19, the report states that the tests do not meet the 'fitness for purpose' criteria, and one reason is that the temperature is too low [2000C compared to 3000C]. I am troubled by this empirical "fix" to model the test and thus, am wondering about the "mixed" transport model in PM-ALPHA. This is known to be a tough problem, clearly, but to arbitrarily change it seems too rough. Also, I disagree that the tests are not "fit for purpose". They are more fit than others and thus,* **are very relevant**. *Thus, the proportion of the radiative transfer*

that goes into bulk heating versus steam production is important to consider and improve upon.

All we are saying is that at 2000 °C the absorption length is so short that basically all heat will be delivered to the interface. Quite the opposite occurs at 3000 °C. This is **not** an empirical fix. About the relevance of the QUEOS tests, see the addendum to Appendix B in DOE/ID-10504.

3. 3] I would also like to see a calculation of PM-ALPHA/3D for QUEOS if indeed there is a benefit to a 3D calculation. It seems that the QUEOS tests are the largest and highest temperature simulant tests to date with solid particles; thus, it may be of use.

The QUEOS tests are only repeats of our MAGICO tests. Both reach 2000 °C and both have about the same masses. Moreover, and in contrast to MAGICO, nothing is known from QUEOS about the most important part of the interaction, which is the internal structure of the mixing zone. Since the QUEOS tests are axisymmetric nothing is to be gained by a 3D calculation.

4. 4] The report finally examines the FARO-LWR test L-14 as a comparison with a large prototypical simulant melt poured into water. This seems like a reasonable comparison test, but I am surprised about what data is compared. There is an enormous amount of data available over the first twenty seconds of the test [the first 5-6 seconds is reasonable before heat loss comes significantly into play] and yet the data comparison is sparse at best. I would suggest the following variables be displayed and compared over the first 5-6 seconds:

a] the total pressure and pressure rise rate [done now]

b] the steam and water temperature at a few locations since its 2-D

c] the kinematics of melt entry and arrival at the chamber base and settling

d] the surface area generated by the breakup as a function of time

e] the mean particle diameter as a function of time

f] the energy flow to vapor and coolant liquid and loss by fuel

g] the level swell of the pool [done but not for long enough times]

We included all information for which there was reliable data.

5. Also I am concerned about the arbitrariness of the dynamic breakup odel [β value = 50], that is used and described in pages A-34/35. This whole procedure

is a matching exercise for some value of beta unless the results begin with a jet of ~10cm and break up to a size that is consistent with the post-test debris data [as well as the amount left as a 'cake']. It would seem advisable to compare the 'frozen' model to other FARO tests to prove that results can be consistently predicted for L06, L08, L11, L19 and L20; all of which were high pressure tests for quenching. Also the 'mixed' transport including radiative transport would have to be held constant in these comparisons to prove the match of L14 has some limited 'universality'.

We do not take a "simulation" approach to breakup, nor, therefore, can our calculations of FARO be considered simulations. It is well known that no one else's calculations can be considered simulations. Our experience with other FARO tests has been equally good (starting from the first one, and already published).

6. INITIAL COMMENTS and QUESTIONS for DOE/ID-10503

The report is very well organized and describes in sufficient detail the ability of ESPROSE.m to perform shock propagation calculations for gas/water and vapor/water situations. I do not completely understand the origin of the CHAT [or CHAT-QL] code comparisons. Are these standard code models or a formulation of the authors to do a code cross-comparison? I understood them to be the latter, and thus I wonder about the need to compare to actual experimental data on shock propagation in single phase and multi-phase systems. This is a minor point, but I think for completeness a link to data is best. My main comments are about the comparison to the KROTOS data.

As explained, the CHAT (CHAT-QL) are special purpose codes for these numerical tests. We have comparison to wave dynamics in the SIGMA experiments also, as shown in the report.

7. 1] The initial statement is made that the KROTOS tests are a challenge since they have imperfect characterization of the initial conditions. One question may be if there are any other tests which give them more insight? After my own search, however flawed these tests are, these and other one-dimensional experiments are the best we have. My other comment is about the initial conditions. The comments on page 4-26 indicate that the fuel mass and flowrate, but there is a problem as far as I can tell. The mass is correct, but the initial jet size is not 1 cm but 3 cm and I think the flowrate of 1 kg/sec is too low by at least 50%. Finally, the fuel particle temperatures are different for each of the tests noted as

is the location and timing of the trigger. I am not sure that the authors are aware of this. I can send them this information if needed, but in the case of KROTOS 38, the initial conditions are not correct; e.g., the jet size is 3 cm and the trigger time is 1.12 sec at or near K3 and not at the leading edge, with a pour time of about 0.75 sec.

The size of the fuel inlet for the calculation is 3 cm, which is consistent with the jet size. 1 cm is the initial length scale assumed for the fuel. Its effect on the premixing is unimportant since it will be compensated by the value of the breakup parameter β, which is adjusted parametrically. The trigger is applied near K3, not at the leading edge (see Figure 3). The flowrate of 1 kg/s is the approximate value quoted by many previous publications on KROTOS (Hohmann et al., 1994). Our understanding is that there is no new measurement to improve this estimate of mass flow rate.

8. 2] The concept of using the parametric mixing model for a β value of 30 or 50, again raises the question of what is appropriate and why. The kinematics in Figure 2 don't have any comparison to the thermocouple data for position of the melt as a function of time and give no indication what 50 is "better" or more correct than 30 for a value. Also what is the time evolution of the particles as the jet breaks up from 3cm to what size? None of this is discussed at all.

As discussed in the text, $\beta = 50$ is the value at which "the melt penetrates to the region between pressure transducers K2 and K3." As shown in Figure 2, the penetration is much slower when $\beta = 30$ and much gaster when there is no breakup. The initial particle size distribution at the time of trigger was shown in Figure 3 of the report.

9. 3] The final point is the use of the parameter, $x_f = 0.5$ to 1.0. Does this parameter mean that when the value is 1.0 all the fuel is quenched as it is fragmented with some fraction of water and steam? If that is the correct interpretation then, the pressure plots do not seem to make sense to me. This is especially the case, since the predicted void in by Figure 2 and Figure 3 is very small. There is something missing in the description; since 1.5 kg of molten alumina has the energy of 6-7 MJ and thus must be quenched by almost all the 35kg of coolant if there is to be such a 'small' pressurization with such little void. How much water is "assumed" to be intermixed with this fuel to give the pressure signature we see? This is never discussed and it is the most crucial part of the model. The complete picture is missing and thus, I am not prone to agree this is a reasonable prediction until all the 'parameters' are specified and explained.

Also, comparisons to more than one test is needed. This has been done with other FCI models.

This question stems from the reviewer's misunderstanding of the parameter x_f and the microinteractions model. As explained in the report, "x_f is the fraction of computed liquid melt participated in the explosion." This factor is necessary because PM-ALPHA underpredicts melt-freezing (it did not account for surface freezing of superheated melt). When $x_f = 1.0$, ESPROSE.m predicted a peak pressure of 4000 bar, which is consistent with our previous microinteractions predictions. The amount of water intermixed with the fuel is calculated based on the microinteractions parameters, which were given already in Appendix C.

F.7. Response to H.K. Fauske and R.E. Henry (FAI)

General Comment and Highlights

General and unqualified agreement with the work under review.

Point-by-Point Responses

1. As requested in your letter dated June 17, 1996, the following comments are offered in the areas of Meltdown/Relocation Phenomenology and Steam Explosion Loads.

Meltdown/Relocation Phenomenology - We agree completely that a downward relocation path of the melting core material through the core support structure (and resulting large fuel pour rates) is "physically unreasonable". Furthermore, the predicted relocation off to the side and from a fully developed melt pool leading to a molten fuel pour rate into the lower reactor vessel plenum of about 200 kg/s, is consistent with the Three Mile Island Unit 2 Core Relocation as described by Epstein and Fauske (Nuclear Technology Vol. 89, p. 1021-1035, December 1989). Fuel pour rates of this magnitude by themselves eliminate concerns relative to global vessel failures, even if an energetic steam explosion is postulated. As illustrated by Epstein and Fauske (1989) and Theofanous et al. (1996), such low fuel pour rates limit the fuel that can be found in transit within the lower plenum to values at least an order of magnitude less than that required for incipient lower head failure (3 to 5 tons). Quoting Epstein and Fauske (1989) "A key aspect of the relocation is, then, that significant quantifies of corium melt were not mixed with water at one time. The slow melt relocation phenomenon is, perhaps, the most important piece of information gained from TMI-2 studies and should figure prominently in future assessments of steam-explosion-induced containment failure as well as lower reactor vessel plenum failure due to fuel debris overheating." This is clearly the case in the current assessment provided by Theofanous et al.

We regret having failed to mention this important reference. We will introduce this reference in the final report. Still, the inference to AP600 is not automatic, because of the reflector!

2. Steam Explosion Loads - Having eliminated the potential for global vessel failure, Theofanous et al. proceed to evaluate the potential for localized damage, by considering local shock loading, with peak amplitudes in the Kbar range as a result of a steam explosion occurrence. Again, the conclusion is that failure

is "physically unreasonable". This conclusion is further supported by noting the following observations.

The above loadings are produced by subjecting the limiting premixtures at atmospheric pressure to triggers resulting from releasing steam at 100 bar. Quoting the authors, "our triggers are chosen sufficient to initiate explosions, and they have no relation to what might arise spontaneously during a pour." We agree with this observation, and in fact believe that the occurrence of spontaneous steam explosions with the molten corium-saturated water system at atmospheric pressure considered by Theofanous et al. is "physically unreasonable". The enormous film boiling heat flux (~3 Mw/m^2) and corresponding vapor flux resulting with this system (several times the critical heat flux of ~1 Mw/m^2) promotes separation and prevents physical contact between the molten corium and water, a prerequisite for steam explosion given a fuel-water pre-mixture. Temperatures (~2000° C) which are well below the melting temperature of corium (~2700° C), would be required in order to reduce the vapor flux in connection with film boiling to fall below the fluidization vapor flux. The above considerations are consistent with the noted absence of "explosivity" for the corium-water system (I. Huhtiniemi et al., "FCI Experiments in the Corium/Water System", NUREG/CP-0142, 1712-1727, 1996). This is in sharp contrast to the noted "explosivity" with the often used molten alumina ($A\ell_2O_3$)-water system. Here the estimated film boiling vapor flux (~0.5 Mw/m^2) is well below the fluidization vapor flux allowing physical contact while the alumina is still molten. While the noted efficiencies are quite low, the super critical pressures observed with the alumina-water tests in the KROTOS facility (Hohmann, H. D. et al., Nuclear Eng. & Design, 155, 391-403, 1995), apparently encouraged Theofanous et al. to model such events and apply them to the LWR system.

* *

3. We also reviewed the approach taken to assess the steam explosion created impulsive loads. Certainly the efforts performed by the authors are impressive in the number of analyses performed and the detailed graphical presentation of the results. The calculations are based on the microinteractions model which appears to be applied in a self-consistent manner. Our question is whether this is the only way that the relevant experimental information can be interpreted. The underlying supposition in such models are that dynamic fragmentation and

intermixing of melt and water can occur on an explosive timescale. Certainly the available information shows that fragmentation can occur during an explosion. However, we are not convinced that the elements associated with fragmentation and intermixing on such a rapid timescale have been demonstrated. In particular, the SIGMA tests performed with molten aluminum indicate virtually no fragmentation for melt temperatures where numerous large scale studies have observed explosive events.

The relevant materials here are Fe and/or UO_2/ZrO_2. We have data now with Fe, and the behavior is quite different from that of $A\ell$. See also addendum to Appendix C of DOE/ID-10503.

4. Furthermore, the detonation concept is compared to the KROTOS molten tin-water and molten aluminum oxide-water tests. With the agreement from this comparison, the authors, in a previous DOE report (DOE/ID-10489), conclude that "low void fraction geometries can produce highly supercritical, energetic detonations." Our analyses show that there is an alternate explanation to the KROTOS experiments that requires no melt fragmentation. If this is the case, the comparison of the microinteractions model with the KROTOS experiments indicates nothing more than that ESPROSE approach is consistent with the experimental observations. It does not provide justification for the microinteractions physical concept.

The Microinteractions Concept and the SIGMA-derived law are **basic** information obtained with relevant materials under fully-simulated large-scale explosion conditions. This basic information cannot be doubted. KROTOS data and our interpretations of them with these laws indicate the consistency of these data with large-scale explosions, not the consistency of the ESPROSE.m approach with these data. Any other interpretations (we are not aware of such) need to show consistency with the SIGMA experiments also.

5. Therefore, we are of the opinion that the approach taken in this document is conservative in that it overstates the possible loads that could be created as a result of thermal explosions. If a design evaluation uses this model and concludes that the boundary would not be challenged, we believe that the conclusion is sound. However, if the modeling approach is used and the resulting loads exceed the capabilities of the structures, we do not believe that this represents an actual challenge to the system integrity.

* *

6. Before such a fragmentation approach can be recommended for realistically assessing the structure loads, it should be proven that a relatively small pressure increase would be sufficient to self-trigger a coarsely fragmented and intermixed system. In particular, it should be demonstrated that a coarsely mixed system could escalate from a small triggering event into an event like that characterized in these evaluations. Assuming that a single grid is filled with steam at 100 bars as a triggering mechanism, it is far too coarse to provide such a definitive representation.

Triggering was not addressed in this report, as was made clear already. This should not be confused with assessing the energetics in a triggered explosion. Since we are not aware of definitive arguments that steam explosions are impossible with reactor materials, we provide here an assessment of energetics, assuming not only that an explosion can be triggered, but also that this occurs at the worst possible time, during premixing.

7. In summary, we believe the modeling of fuel relocation and quantification of premixtures to be reasonable and consistent with experimental observations including the TMI-2 incident. On the other hand, the assessment of steam explosion loads appear to be very conservative. The corium-saturated water system is not likely to exhibit "explosivity". Therefore, a very strong case can and has been made for the effectiveness of "in-vessel retention" as a severe accident management concept for a reactor like the AP600.

* *

F.8. Response to D.F. Fletcher (U Sydney)

General Comment and Highlights

General and unqualified agreement with the conclusions of the work under review.

Point-by-Point Responses

1. Summary

This review covers the study of lower head integrity under steam explosions performed at UCSB by Theofanous and co-workers, together with the code validation reports for PM-ALPHA and ESPROSE.m. The study and validation reports contain a massive amount of very high quality work. The depth of the study and extremes to which the authors have gone to use validated tools is second to none world-wide. For example, no one else is performing 3D premixing and propagation calculations.

The work is of very high quality and in my view the conclusion that steam explosion induced lower head failure is unphysical is completely justified. The technical arguments support this with a high degree of redundancy.

1 Introduction

Firstly, I believe it is important to comment on both the quantity and quality of the documentation supplied for this review. The very complete verification manuals for PM-ALPHA and ESPROSE.m are unique. A minor semantic point but they are much more than verification (which implies that the code does what it should) manuals but are also validation manuals as they examine how well the code represents real experiments.

Secondly, I wish to record that I was impressed by the scope, depth and quality of this study. It provides a very comprehensive basis for rejection of steam explosion-induced failure of the lower head.

The remainder of this document presents specific comments on the Study and the two validation reports.

2 The Study (DOE/ID–10541)

This section deals with the main document of the study (DOE/ID-10541) and pays particular attention to the steam explosion part of the study.

2.1 Introduction

This section gives a brief summary of earlier work on lower head failure. It discusses three earlier studies by Bohl et al, Theofanous et al and Turland et al, all of which highlight the need for mechanistic pressure loading calculations before the lower head issue could be addressed adequately. This is the first such study in which this approach has been possible.

2.2 Problem Definition and Overall Approach

This section sets out the methodology to be used. Essentially, the now established ROAAM procedure is used in which the overall event is split up into well-defined physical processes that can be modelled, combined with intangible parameters (such as triggering time). The proposed sequence of events and the split between physical processes that can be quantified using a validated *model and those which must be treated in a parametric manner seems correct to me. In particular, I believe that the flow chart shown in Figure 2.3 gives a correct and well-judged progression of events. Details of the modelling will be discussed later. However, it is important to emphasize that the identification of a sound methodology is very important and I believe that the authors have done a good job at this stage of making the process transparent.*

2.3 Structural Failure Criteria

This section deals with quantification of the likelihood of vessel failure for a transient, localized load. The material is presented in a clear manner and there is a step-by-step progression from an axisymmetric model to the examination of localized loads. The analysis presented in equation (3.10) and Figure 3.8 provides a neat means of determining the effect of localized loading and the performance of equation (3.10) in correlating the data is impressive. Also I believe that the failure criteria given in Table 3.3 are sensible and fit the presented database.

This chapter is important in that it sets up the basis for the determination of whether a particular explosion loading will or will not fail the lower head. There would appear to be significant conservatism in the analysis, as noted on page 3-1 and from Figure 3.8 at the high impulse end, and therefore it provides the required function for this study.

2.4 Quantification of the Melt Relocation Characteristics

This section presents an analysis of the melt relocation characteristics. It is important to note that the analysis does not use a system code but instead a number

of highly specific models have been developed to address the physical processes deemed to be important. This was the approach followed in the Sizewell B study and seem to me to be the correct way to proceed. Based on my participation in the Sizewell B study I believe that the methodology used and the conclusions drawn are correct.

The melt flow rates and release conditions are consistent with those found in the Sizewell B study. In particular, I believe that massive pours of many tonnes per second have been ruled out on the correct physical basis.

In the section on reflooding the authors do not consider the possibility that a steam explosion may occur as the water refloods the molten pool. It is covered in a later section and it would perhaps be wise to have given a forward reference here.

Forward reference to Chapter 8 is added.

2. 2.5 Quantification of the Premixture

This section addresses the determination of the premixture configuration. Firstly, it is important to note that the highly 3D nature of the pour has been taken into account via the extension of the PM-ALPHA code to 3D. Thus the localized, rather than smeared in 2D, characteristics of the melt-water interaction process can be simulated. Secondly, it should be noted that melt breakup has been taken into account in a parametric manner. At first sight this may seem like a weakness, as many proposed breakup models exist. However, given that none of these has been properly validated it seems appropriate that the effect of breakup be addressed in a parametric manner. As pointed out in the report, in the event that the melt enters the water pool and runs along the vessel wall, there will be less mixing than calculated here and therefore the explosion energetics will be reduced.

Based on my experience of premixing experiments and modelling I have no difficulty in believing that only tens of kilogrammes of melt are likely to be mixed in the given configuration. Clearly the high voiding rate is a consequence of the water pool being saturated. I was left wondering whether in the event that the melt pour occurred during the reflooding processes whether there would be sufficient subcooling present to increase these masses significantly? My expectation

is that the increase would be by no more than a factor of two, which would still result in small mixture zones.

The likelihood of subcooled water in the lower plenum pertains to the "fast" and "medium" scenarios discussed in Section 4.4. The effects of subcooling and of a higher pressure on premixing are discussed in addendum to Chapter 5.

3. 2.6 Quantification of Explosion Loads

This section deals with the determination of the magnitude of the possible explosions that could be generated from the premixtures calculated using PM-ALPHA. It is important to note that these calculations, performed using ESPROSE.m are fully 3D and can therefore account properly for explosion venting. The validation of the model is discussed in a separate section. It is sufficient here to note that the code has been subjected to a very significant validation effort which I believe shows that it is 'fit for purpose'.

I agree with the approach adopted regarding triggering. Specifically, triggering at different times and looking for the maximum load is clearly conservative. In addition, the effect of the premixing breakup parameter β is consistent with experimental observations and highlights the fact that the uncertainties in breakup can be taken into account in a parametric manner.

Given the premixture configurations determined using PM-ALPHA I am not the least surprised that none of the explosions challenges the integrity of the lower head.

2.7 Integration and Assessment

This very brief section explains that as a consequence of the methodology and results there is no need to continue with the probabilistic approach because of the enormous mismatch between explosion loads calculated and those required for failure. In order to show that this is not an artifact of the approximate structural treatment, full ABAQUS calculations showed there to be no problem.

I agree that the only way to obtain a significant explosion is to have extensive mixing which requires highly subcooled water. I believe the arguments against this are sound, especially if one keeps in mind that the enormous amount of heat which would be stored in the lower core support structure would be available to remove subcooling.

2.8 Consideration of Reflood FCIs

This is an important section, as the above analysis has clearly shown that premixed explosions cannot cause failure of the lower head. I agree with the view taken that you need a very substantial overlying water pool to provide sufficient inertial constraint to generate a high pressure explosion. As in the previous scenario everything is against this, viz. the low water addition rates, the ease with which the melt surface freezes and the fact that as film boiling occurs the overlying pool will develop voids reducing its ability to constrain. The analysis rules out to my satisfaction the possibility that stratified explosions could fail the vessel.

2.9 Conclusions

The conclusions contain a summary of the results presented in the earlier chapters and presents a concise summary of the important physical features of the system and the physical mechanisms which lead to the conclusion that failure of the lower head by a steam explosion is unphysical. I really appreciated this carefully presented summary.

3 PM-ALPHA Verification Studies (DOE/ID–10504)

This section presents a review of the PM-ALPHA verification studies report. It is important to note up-front that PM-ALPHA has been the subject of continuous development and peer review (at conferences) over an 8-10 year period. It is therefore a mature piece of software.

3.1 Introduction

The main point of interest in this section is Figure 1 which lays out the verification and validation approach. This is very comprehensive and covers numerical aspects, comparison with other codes and analytical solutions and with experimental data. I can suggest no improvements to this validation matrix. It is also worth noting that this section highlights the new feature of PM-ALPHA, namely extension to 3D which is clearly needed in the Study. This clearly represents a massive amount of work but the new insights gained are definitely worth the effort.

3.2 Multifield Aspects

This section deals with the testing of the multiphase constitutive relations and the modelling for the sedimentation of particles or clouds of particles. PM-ALPHA

computational results are compared with experimental data and analytical models (based on the drift flux approximation) for the sedimentation of single particles and clouds. In all cases agreement is excellent. A novel feature of this presentation is that the trajectory of the solution in drift flux-volume fraction phase space is presented. These results show that the solution is approached in a variety of ways and helps to explain why multiphase numerics prove to be so complex. These results confirm that the code can reproduce the correct particle fall speed, an important feature the steam explosion study.

Numerous refereed papers have been presented showing that PM-ALPHA can simulate the MAGICO tests, where in most cases there is also phase transformation. These simulations also show good agreement with *local* data on mixture composition and void fraction. This is important as PM-ALPHA must predict the correct mixture composition if the calculations of explosion propagation are to be reliable.

Comparisons of PM-ALPHA simulations with data from the QUEOS tests are generally good. There is evidence of numerical diffusion in, for example, Figure 6 but the authors are aware of this and are planning runs on finer grids. In the hot cases I agree with the authors that both the relatively low melt temperature (making radiation absorption a surface phenomenon) and the gravity-induced subcooling are important. If the explanation of the difference in steam production advanced in the text is correct (namely the superheating of a layer of water during the fall stage) it means that interpretation of experiments of this type, where there is relatively little steam production, will always be very complicated. Given the short time available to the authors to analyze this data and the experimental uncertainties I feel that PM-ALPHA performed as well as could be expected.

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4. 3.3 Integral Aspects

The code comparisons with CHYMES and between the 2D and 3D versions of the code give a high degree of confidence that the basic numerical algorithm is correctly coded and that the 2D and 3D approaches are consistent. The comparison with data from the MIXA06 experiment is at least as good as that achieved by the experimenters using the CHYMES code. The lack of melt spreading in

the simulations is very similar to that found using CHYMES. The level swell and steam production data are well reproduced given the experimental uncertainties. Again it is fair to say that this test is well simulated given that there are several important experimental uncertainties regarding particle breakup and the steam flow rate.

The comparisons of code calculations with data from the L-14 FARO experiment are also good. In this experiment there is no local data and only global quantities, such as vessel pressurization and level swell, are available for code comparison. The choice of parameters to match these data seems very reasonable. I found the figures illustrating the non-local absorption of radiation interesting and these clearly illustrated the importance of this phenomenon for high temperature melts. To my knowledge these are the first calculations to include this feature, which is clearly of importance in high temperature melt applications.

3.4 Breakup Aspects

I completely agree with the chosen approach to breakup. As more tests are analysed it will be possible to increase the degree of confidence in the chosen values for the parameters. Clearly, given that the melt surface area transport equation is already coded it would be a simple matter to include a mechanistic model, should a validated breakup model become available. However, the analysis presented in the study shows that the overall predictions of loading are insensitive to the choice of these parameters. Therefore the lack of a detailed model does not in any way effect the conclusions of this study.

3.5 Numerical Aspects

The authors are clearly aware of the need to avoid numerical differencing errors and the presented calculations show that they are taking care to address this problem.

3.6 Concluding Remarks

I think this section identifies the correct areas for future focus. If I were the authors I would have made more of the fact that this is the most comprehensive validation effort to date and that the code has performed extremely well.

3.7 Appendices

Appendix A provides a comprehensive description of the constitutive laws and Appendix B provides a detailed paper on the MAGICO tests. The reviewer is familiar with the material in the Appendices and this has not been reviewed in detail.

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5. 4 ESPROSE.m Validation Studies (DOE/ID–10503)

Firstly, it is important to tackle head on the ESPROSE.m formulation, which I believe it is fair to say has not been widely accepted. I find it hard to understand why this is the case. Essentially, the novel feature in ESPROSE.m is the inclusion of an additional fluid (the m-fluid) which represents the fragments and the fluid in intimate contact with them which is being heated. The need for such an approach seems beyond doubt to me following the very careful experimental analysis of Baines [1] and my own attempts to analyze KROTOS-like tests using CULDESAC [2]. The authors have provided comprehensive experimental data for appropriate pressure loadings to show the finite mixing rate. They identify the need for an enlarged database but it should be recognized that the ESPROSE.m formulation is conservative in the sense that by mixing the fragments with only a fraction of the coolant they generate high local pressures. This point should be kept in mind when examining the use of ESPROSE.m results.

The remainder of this section contains detailed comments on the various chapters of the verification report.

4.1 Introduction

The main feature of this chapter is Figure 1 which gives the validation strategy. This is very extensive and to my knowledge is the first model to be subjected to specific wave dynamics and explosion coupling verification studies against analytical and experimental data. It also covers the two main experimental programmes KROTOS and ALPHA.

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6. 4.2 Wave Dynamics

The 1D solutions for the shock speed and particle velocity (important in relative velocity fragmentation) are excellent. The same applies to wall reflection studies,

the effect of void and the effect of non-condensable gas. The venting calculations also show good agreement with the CHAT results. I was curious to know why the calculations were performed for a pressure step of 40 bars over a base pressure of 100 bars and over a space dimension of only 1.4 cm. I would have preferred to see venting on the 0.1 m scale (with a 1 cm mesh) and a pressure difference of say 10 bars venting to atmosphere. Figure 9b shows that the ESPROSE.m results only exhibit dispersion at the first few time steps and that the numerical diffusion is modest.

The scales are really unimportant in these comparisons — we have many more not shown.

7. The 2D comparisons are impressive and show that ESPROSE.m captures the wave dynamics very well. The only point that this section raises for me is why in the type B behaviour the ESPROSE.m results have a spike at the origin (as expected from the source description) but the analytic solution does not (see Figures 7, 13 and 19). Is this simply a plotting omission?

Yes, this was plotting artifact and was removed.

8. The experimental comparisons with data from the SIGMA facility are interesting and show that ESPROSE.m is capturing the average wave behaviour well. Clearly, the pressure transducers are picking up many local reflection events which are due to the inhomogeneous nature of the 'mixture' and cannot be modelled via a continuum approach. I am surprised that ESPROSE.m has done so well for this system with the only apparent systematic difference is the tendency for a ∼1 ms time lag.

Actually, the maximum time lag is only 0.2 ms (1 ms is a major division — this was clarified in all captions).

9. This section provides very solid verification for the code algorithm and the choice of solution parameters.

4.3 Explosion Coupling

This section contains test cases in which energy is input into the gas phase via a parametric relationship in which the energy input into the gas phase is proportional to the fluid velocity to the power 1.5. This is done to represent the fact that in ESPROSE.m the energy is input in the m-fluid. Results for calculations

for both cases considered are in excellent agreement with the CHAT simulations. Figure 5, for simulations on a larger space scale, examines the effect of grid size. The comparisons are good with differences being confined to the interface region.

4.4 Integral Aspects

The analytical tests show that ESPROSE.m can perform well at the extreme limit assumed in the Board-Hall model. These calculations are interesting as they show explicitly the effect of the fragmentation rate and entrainment factor on the propagation characteristics. Figure 6 is interesting in that it shows dispersive-like behaviour but if the grid is as described earlier these are real rather than numerical. Could the authors comment?

The dispersive-like behavior is real; it is due to the slow fragmentation rate which allows the pressurization to occur gradually.

10. The confirmation that the 2D and 3D models give similar results is thorough and convincing.

I agree with the authors that the KROTOS tests are too poorly characterized for real validation studies and therefore I do not think this section is central to the validation case. The point about melt freezing is very interesting and the fact that the code under-predicts freezing times is important. This effect will be compounded by the fact that that the melt is assumed to be at a uniform temperature, whereas an outer shell will freeze first. Surface freezing provides the most convincing hypothesis (to me) of the non-explosive behaviour of UO_2 in KROTOS.

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11. 4.5 Numerical Aspects

I agree with the conclusions drawn. The presented calculations clearly show that the authors are aware of the need for adequate spatial and temporal resolution. In addition, the results show a good compromise between diffusive and dispersive errors.

4.6 Concluding Remarks

This is a very important section and I believe the authors have judged the current situation very well. I agree entirely with the conclusions they have drawn from the very comprehensive sets of calculations performed to date. There is a clear need for the high temperature SIGMA data and I am aware that plans to obtain this are well advanced.

I personally doubt that it will every be possible to characterize the KROTOS experiments much better and my experience with the MIXA tests tells me that there will always be something left to be measured. Therefore I agree that this is a lower priority. The comments on secondary pressure waves are interesting and clearly of a very fundamental nature. I do not believe that such effects could be addressed easily within the continuum model but I would certainly encourage their investigation.

Finally, I agree completely with the closing paragraph: moving to large-scale, multi-dimensional experiments will only add confusion.

4.7 Appendix A

I have no specific comments here. I am generally familiar with the modelling approach taken and I believe appropriate modelling choices have been made from the available database of constitutive laws. It should be recognized that it is in the formulation stage that the ESPROSE.m model differs fundamentally from others in that it is 3D and uses the microinteraction concept to allow for thermal disequilibrium within the coolant.

4.8 Appendix B

This section contains a description of the CHAT code used to provide analytical solutions for code comparisons. The model is formulated for the case of homogeneous flow of liquid and coolant (no slip but different temperatures). Thus the system has only real characteristics and therefore can be solved in an elegant and accurate manner. It provides an excellent means of testing ESPROSE.m.

4.9 Appendix C

This appendix is a reprint of a conference paper which describes the microinteraction data and its implementation into ESPROSE.m. I am familiar with this work (from the paper and visiting the facility) and believe it to be both unique

and of a high quality. Whilst at present results from low temperature melts have to be extrapolated to the reactor case, plans are well advanced to produce the required data.

4.10 Appendix D

This appendix also contains a reprint of a conference paper which discusses the manner in which the 'real world' differs from the Board-Hall model. It is very interesting as it shows how the inclusion of microinteraction physics produces propagation behaviour which is very different from the Board-Hall model and other propagation models which do not allow for micro-mixing. Essentially, it allows propagation in systems which are melt lean because the energy from the melt is transferred to only a fraction of the water present. It provides an interesting perspective on which to end the ESPROSE.m validation report and clearly illustrates what a significant advance the microinteraction concept has been in propagation modelling.

References

[1] Baines, M. (1984). Preliminary measurements of steam explosion work yields in a constrained system. Inst. Chem. Eng. Symp. Series, 86, 97-108.

[2] Fletcher, D. F. (1991). An improved mathematical model of melt/water detonations—II. A study of escalation. Int. J Heat Mass Transfer, 34, 24492459.

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F.9. Response to H. Jacobs (FZK-INR)

General Comment and Highlights

This is a highly skeptical review, questioning even the non-existence of supercritical thermal detonations in highly voided premixtures. Until the reviewer resolves this trivial point in his own mind, we can make no progress here.

Point-by-Point Responses

1. *1 . Introductory remark*

In order to put my comments to follow into the right perspective, I must state first of all that I fully agree with the general approach to the problem taken by the authors, i.e. the ROAAM. To what extent probabilities are used within this approach may depend on the purpose and the problem of the study. However, dividing the problem into its physical aspects, treating them in separate parts of the study that can be scrutinized by other experts and linking them in a well defined and verifiable way defines a clear path towards the resolution of the full problem.

Similarly I fully support the basic approach taken to treat the steam explosion problem. The material presented is based on and incorporates a lot of pioneering and exemplary work in this field. I do not want to shed any doubt on that. The only question I'm discussing is: Is the state of development sufficient to finally answer the question under discussion. This forces me to elaborate on potential weak points in the argumentation. If a technical field isn't developed sufficiently, even a 'peer review' cannot finally ensure the correctness of an evaluation.

Quite obviously, steam explosions are not phenomena that are well understood in the scientific sense, especially if we are concerned with such large-scale events as are discussed in connection with reactor safety analysis. Unfortunately, such events lie far outside the parameter range that can easily be studied experimentally. This is true of the initial temperature and the composition of the melt as well as the masses involved (as mentioned above). This dilemma forces us to largely rely on codes for extrapolating from the accessible parameter range to that of the envisaged accident situations. Ideally this extrapolation requires full knowledge and appropriate modeling of all relevant phenomena. Here again we are confronted with gaps, the relevance of which is difficult to judge. The concept of 'fitness for purpose' may be helpful in areas in which the consequences

of neglecting something can be estimated. But how about problems which have not yet been identified or the importance of which has not yet been perceived? In the present state of knowledge bad surprises cannot be excluded. The (only?) way to deal with this difficulty is to account for all (known) possible traps in the analysis (take a conservative approach) and to require a large safety factor. To some extent, this principle is followed in the study discussed here. But in my judgement not to a sufficient extent.

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2. From the point of view of quality assurance, a peer review like this one can become fully effective only if at least the background material was published since quite some time so that a thorough discussion of it has been possible among the experts. In the present case an important part of the background material was delivered very late during the review process. This reduces the relevance of the present review process.

A key point of ROAAM is that the review is **not** hurried through. Valid concerns are pursued for **as long as it takes**. In the present case, all documents were supplied by the end of September 1996. This particular reviewer was informed by DOE's project manager that he could take as long as necessary to supply his review, and it was sent about two months later, by the end of November 1997. Our responses, including updated versions of the reports, were made available to the reviewers a little more than 6 months later (about June 15, 1997), so that at this stage the process has been on-going for about 1 year. This was done by design, and, again, will continue for as long as technically substantive concerns exist.

3. 2. Scope of this review

This review is concerned with the steam-explosion aspects of the study. The contribution of this part of the study to the positive final conclusion, i.e. interacting masses that are insignificant from an overall energetic standpoint and even local loads that lead to elastic strain only, can be attributed to small pouring rates, a strong voiding of the premixing zone and early explosions. The first of these are to some extent a consequence of the melt-water mixing scenarios chosen and although core melt down is not my proper field of experience I must make a few comments on this because the way in which melt and water are brought into contact is basic for the subsequent events. The possibility of a small steam explosion inducing a larger one is neglected altogether. The second point, i.e. the proposed

strong voiding of the zone in which corium melt and water are intermixed prior to an explosion (the mixing zone or premixture), is instrumental in two ways: In addition to the small pouring rates it reduces the interacting masses, At the same time, this voiding seems to be one reason for the dying away of the energetics of explosions with increasing time of triggering which is the most convincing argument for considering early explosions. Of course, this finding also depends on the third point, i.e. the way in which the steam explosion proper is modeled. The above three aspects, i.e. scenarios and modeling of premixing and explosion are discusses one after the other below.

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4. 3. Technical evaluation

3.1 Melt relocation scenarios

By the scenarios it is defined how the melt relocates into the lower plenum and this gives the rates at which corium is fed into the lower plenum. Therefore this is an important aspect that must be scrutinized during the review step of ROAAM. I am not really an expert in this field, myself, but I must raise the question whether it is really possible to exclude with sufficient certainty a downward relocation that could lead to much higher corium flow rates depending on the number of holes in the core support plate through which corium flows into the lower plenum. My doubts in this respect come from the agreement of the experts in this field that the late phase of core melt down, i.e. the melt relocation phase, is not well understood and from the virtual absence of mechanistic models for growth and especially radial expansion of molten pools. The study that is under discussion here tries to bridge this gap using simple and clear estimates of conditions influencing the thermal stability of a metallic crust. But in these estimates, e.g. no consideration is given to the possible formation of eutectics which might drastically reduce the melting temperature and thus crust stability. One might also speculate that some hot material could drop into the water remaining below the corium pool, thus decreasing the time until it is evaporated and thus the time of crust stability. In the present study the evaporation time 'happens to be just about equal the time it would take to melt through the reflector and core barrel.' Of course, there is in addition the thermal inertia of the core support plate. But as soon as its top falls dry, its surface temperature will increase and thus reduce the effect of radiative heat transfer.

Aside from control rod materials which would lead the relocation and thus be eliminated, all other eutectics possible are well covered by our metallic blockages. Since paths for relocation are not available, by huge margins, one is not free to **speculate** that "some hot material would drop into the water . . ." helping accelerate the evaporation. Finally, the reviewer's supposition that "as soon as its top falls dry, its surface temperature will increase and thus reduce the effect of radiative heat transfer" implying blockage failure is incorrect — see addendum to Chapter 4.

> 5. *Another possible uncertainty is the stability (leak-tightness) of a sideways (radially) advancing crust. This process might induce transverse forces on the supporting stubs of fuel pins which these cannot withstand in their damaged condition. So the crust could fail and the oxydic melt could flow freely towards the core support plate and possibly through it. (Table 4.1 indicates that the 'cold trap' is not likely to stop flowing oxydic corium.) Here one may recall that processes of this nature occurred during the TMI-2 accident [1] although, in that case the whole melt pool was submerged. As witnessed by several tonnes of corium that solidified within the core support assembly, a large amount of corium has flown down through about 4 peripheral fuel elements around core position R6. Another downward relocation occurred at core position K8. The latter may have been brought to a stop above the core support assembly. But we do not know how and by what margin.*

The TMI did not have the zirconium pellets at the bottom of the core. Still, the melt was trapped **above** the core support plate, preventing relocation through the downwards path. Contrary to the reviewer's intention TMI actually fully supports our scenario.

> 6. *Finally, the possibility of a large coherent steam explosion that is induced by a smaller one (e.g. one of those considered) is completely left aside. Such event might proceed in different ways. The common starting point of these would be the mechanical destruction of the crust keeping the melt pool. This might be caused directly by the action of the pressure of the first steam explosion or indirectly by the pressure of another melt-coolant interaction due to the addition of some water into the upper zone or on top of the melt pool. The induced steam explosion would then occur either within the core volume (if there were still water left) or in the lower plenum after the melt released from the broken melt pool*

has drained through the still open holes in the lower grid plate. It is sometimes argued that such melt couldn't encounter water in the lower plenum because that would have been driven away by the initial steam explosion. However, the first (weak) explosion might have caused essentially a sloshing movement of the water so that this could mix very effectively with the corium streams when returning. In this context one should also keep in mind that with a large molten corium mass available and melt-water interactions occurring, large amounts of mechanical energy may become available. So it is often hard to argue that some process was unlikely.

These comments do not take into account the geometric features of an AP600 core trapped **above** the massive core support plate. Nor do they take into account the highly localized, in both space and time, pressure pulses predicted. Water on top of the melt is unimportant in this respect for the same reason that we are not concerned for late FCIs, as explained in Chapter 9. Moreover, as explained in this chapter, water on top of the molten core is not physically relevant in the AP600.

7. 3.2 Modeling of premixing

Premixing is the process that is thought to be required to set the stage for any large scale coherent steam explosion. It is, at the same time, expected to inherently limit the masses participating in an explosion by the 'water depletion' effect, i.e. removal of liquid water from the premixture by large amounts of steam that are created due to fast heat transfer. As these processes are difficult to simulate directly in experiments, recourse is taken to numerical modeling with the code PM-ALPHA.3D. For the scenarios considered, this code predicts strong voiding of the volumes accessed by melt. In combination with a cut-off of propagation that is effective at high voiding this gives a strong limitation of the melt masses that can interact. And this is the second pillar on which the final result of the study is resting.

While there are good arguments for the concept of 'water depletion' and also some experimental observations that appear to support the idea in principle, there remains the question whether the quantification given by PM-ALPHA.3D is sufficiently reliable. The program predicts 'the major portion of it [i.e. the fuel] being in a highly voided region ($\alpha > 80\%$)' and also that the void fraction 'gradient is very steep', i.e. the void fraction increases from values around 20 % to more than 80 % within a short distance. Such behavior, however, was not seen

in the premixing experiments that are being conducted at Forschungszentrum Karlsruhe in order to study the phenomenon and to collect data for code validation [2], [3], [4]. It is too early to draw final conclusions from these experiments, but the void fractions in the surrounding of broken up 'fuel' appear to be smaller than expected.

The QUEOS experiments were run under conditions quite different from those of MAGICO, from which the reviewer's "**expectations**" may have derived. We provided quantitative interpretations of the available QUEOS tests and see that this lower voiding should,in fact, have been expected. More imporantly, to this day we are not aware of any published, reliable void fraction maps over the premixing zone in the QUEOS experiments. We have such detailed maps in MAGICO (see Appendix B of DOE/ID-10504 and the addendum to it), and show that even with very dilute pours (0.5%) we get void fractions in the 60–70% range, extended over the whole mixing zone. The QUEOS pours are too short, and too concentrated to reveal the important thermal interactions that lead to extensive and persisting voided premixtures.

8. One may also draw attention to data reported of the KROTOS experiments [5]. In these tests molten alumina was poured through an orifice with 3 cm diameter into a 10 cm wide tube filled with water, it mixed with the water and strong steam explosions occurred either spontaneously or following an external trigger. The melt temperature was high, typically 2600 K, but the water was subcooled which, of course, tends to reduce voiding. In the KROTOS tests #28 and #29 the water was subcooled by 10 K and 80 K, respectively. In both cases the steam volume fractions within the reaction tube were 4 % only. But as these are mean values over the whole tube which may contain some regions occupied by water only, it may be more relevant to point out that the steam volume was only about half the melt volume. In test #30, subcooling was again 80 K but the melt mass was larger and its breakup was more intensive. In this case the steam volume fraction reached 23 % but this is again only 1.3 times the melt volume. So we must check how well the above cited calculational results of PM-ALPHA.3D are founded which imply steam volume fractions that are larger than the melt volume fractions by well over an order of magnitude.

Our calculations of KROTOS yield similarly low average void fractions, so, again, there is not surprise here. Until void fraction information is available from other experiments, our principal

verification basis must remain MAGICO-2000. The present data base is extensive, covering explicitly various regimes (introduced in the addendum to Appendix B), and we believe it is quite sufficient.

9. The original PM-ALPHA was one of the two pioneering codes that used three velocity fields for describing the separate motions of melt, liquid water and steam at the cost of adding considerable complexity to the already quite complicated two-field description of two-phase flow. But this is the only way in which one can hope to develop a reasonable description of the phenomena during a steam explosion. The fairly standard multiphase equations used provide compliance with the conservation equations only. All the controlling and very complicated physics in the three-phase (and at least) three-component mixture must be described by constitutive relations. Here the difficulty arises that one of the main purposes of such codes is to extrapolate from the experiments that are possible in practice to the envisaged accident situation. This implies extrapolation from simulation materials (sometimes even solid spheres) to the expected (but still quite uncertain) molten corium, from often quite low 'melt' temperatures to temperatures around 3000 K, and from the mostly very small scale of experiments to the reactor size. There are a few experiments in which one or the other of the above initial conditions is not as bad as indicated here but as the experimental difficulties grow enormously as the expected accident conditions are approached, the experimental information on the initial conditions and details of the processes is often poor in these cases so that a successful comparison of calculational results with integral experimental results doesn't necessarily indicate correctness of the theoretical model. Indeed, one can expect a code to perform the required extrapolations only, if all relevant mechanisms are modeled mechanistically and with sufficient accuracy.

This is why we have a very carefully developed verification plan, covering all aspects of the calculations.

10. However, the constitutive relations used in PM-ALPHA are often heuristic, sometimes parametrical. The latter is described in the report for the melt breakup model but is true as well for one formulation of the evaporation rate. The other formulation looks more physical but still does not allow for the possibility that evaporation and condensation occur concurrently in the same integration volume (calculational mesh) due to limited subcooling of the water and intensive

local (radiant) heat flux to the vapor/liquid interface where the melt drops are covered by a thin vapor film only, as e.g. on those parts of their surfaces that are oriented towards the direction of motion. So any extrapolation to accident conditions must be afflicted with large uncertainties.

Only the breakup law is parametric, and its basis and rationale have been explained in the report. Most importantly, and counter to the reviewer's claims here, PM-ALPHA includes a correct phase change model (simultaneous evaporation and condensation), as well as a non-local radiation deposition model (it is unique in recognizing this important physics among all such codes). Evaporation and condensation can occur in the same integration volume.

11. Validation of the original PM-ALPHA code by comparison with experiments was first described in Reference [6] which is also reproduced as Appendix B in the special verification report [7]. An appeal of the general agreement reached may be obtained from the data on the leading edge advancement. With cold spheres this agreement is mostly reasonable. With sphere temperatures of about 1600 K the data are reproduced within about a factor 2. In the 'production runs' of the present study the initial temperature of the melt will have been beyond 2900 K so that the uncertainties will certainly have increased quite considerably.

We have data now up to 2300 K, and in all cases the code very accurately predicts the front advancement. We do not know where the reviewer found the factor of x2!

12. Here we are mainly interested in the high void fractions that have been measured and predicted during the verification process. The data given in [6] have been obtained with the MAGICO experiment and have been described as highly relevant ('the measurement not only provides insight into premixing, but represents probably the most important test for computer codes'). Hence our expectation to find high local void fractions in our own experiments. However, the local void data presented in [6] have been measured in a position or better line or 'small region' (of unknown size) 15 cm below the initial water level. This depth is only two thirds of the equivalent diameter of the pour. We may guess that the measuring volume was centered with respect to the particle jet (the pour). How its width compares to the width of the pour is not known. The measurement was performed at 0.35 sec, i.e. just after the end of (or behind) the pour, probably in order to avoid the presence of many spheres at the level of the measurement. These circumstances appear to have produced the observed high void fractions

possibly without too much contribution of steaming. It is our observation from the QUEOS experiments [3], [4] in which streams of spheres are poured into a water pool in a similar way, that the particle cloud is always followed by a gas filled chimney - with cold spheres as well as with hot spheres. This is largely a consequence of the momentum transfer between the particles and the water while thermal effects are of secondary importance - they essentially influence the way in which the gas chimney is closed again. That this is also true in the MAGICO experiments is clearly shown by Figures 14 and 15 in Reference [6] which illustrate a 'cold' run. This means that the reported high void fractions have little to do with the so-called 'water depletion' effect and there is no experimental support for the high void fractions calculated in the 'production' runs at positions far away from the melt entrance. One might add that corresponding to our observations in the QUEOS experiments, thermal effects just start to be detectable in an overall sense (beyond local effects around each individual sphere) at sphere temperatures as low as 1600 K. Even at the much higher temperatures beyond 2300 K that have been reached in QUEOS, no high void fractions could be observed outside the initial gas chimney produced by the entering clouds of spheres (essentially by momentum transfer).

The QUEOS behavior is peculiar to the experimental conditions and it is quite predictable with PM-ALPHA. The addendum to Appendix B of DOE/ID-10504 should be helpful to the reviewer in sorting out the differences in his own mind. Comments such as in his last sentence need to be supported by data, for otherwise such points and responses can only produce confusion.

13. In the main body of the verification report [7] global estimates of the water content within the mixing zone in QUEOS are used for further checking PM-ALPHA. Unfortunately this type of data is hardly suited for a quantitative comparison with code calculations. The difficulty is that the result very much depends on the choice of the outer radius of this zone because, due to the weighing with the radius squared, it is this region that dominates the integration over the total volume. In the experiment this difficulty can be overcome to some extent by precisely determining the shape of the mixing zone from high-quality photographs - at least to the extent that a qualitative result can be obtained. However, in code calculations, the calculational mesh is not able to sufficiently resolve this outer boundary. So, what is given in [7] is the 'PM-ALPHA result for the central region of the mixture, containing the main portion of the particle

cloud.' As a consequence, the calculated value is somewhat ambiguous and Figure 13 in Chapter 2 of Reference [7] unavoidably compares quantities with different definitions.

We did the best we could with the data available in QUEOS. The weakness is not with the calculation (with fine grid and Lagrangian particles we can resolve the mixing zone to a very high degree) but rather with the experiments that gives only a very rough estimate of a zone-average void fraction.

14. It remains that the code in this case predicts low voiding (in contrast with the production runs). But here the code appears to have gone to the other extreme due to its inability to describe evaporation in the presence of subcooled water which even leads to the reported underestimation of evaporation (steam flow) rate and pressure rise. To explain these discrepancies by possible liquid superheat of the water in the experiment is probably inappropriate in the presence of large free surfaces.

Incorrect in both respects. Our code describes evaporation in subcooled water well, and we estimate the steam flow quite well. The extra peak is indeed due to water surface layer superheated, as described in our report. The reviewer has not provided evidence refute this real phenomenon.

15. Another uncertainty of the calculational results is due to modeling the corium breakup. The surface of a certain amount of material varies linearly with the (inverse of the) particle radius. Therefore modeling the corium as individual droplets with 2 cm diameter from the very beginning gives it already a quarter of the surface that it would have with drop diameters of 0.5 cm which can certainly be considered as well prefragmented (broken up). In the calculations presented, this initial diameter is combined with an entrance volume fraction of 25 % only so that there is an intensive thermal interaction from the very beginning. However, in the PREMIX experiments being performed at Forschungszentrum Karlsruhe [2], we have observed that a melt jet can penetrate to quite some depth into saturated water (e.g. 0.5 m for a jet diameter of about 4 cm) before it starts to break up and to interact more violently (still not explosively). In these cases the melt is molten alumina at about 2600 K the density of which is only about one third of that of corium. So this behavior is even more probable (should be more pronounced) with corium. Such dynamic breakup process with virtually no breakup in the beginning that allow the melt to penetrate deeply into the water followed by more rapid fragmentation that breaks the melt into medium-sized

drops (which mig be the most dangerous configuration) cannot be bounded by the parametric breakup model that was employed. Such bounding would require to model as well the entrance of coherent melt (melt being the continuous phase) that is not premixed with water artificially (by assumption) from the very beginning. In this context it is also important to note that breaking the melt into very small droplets (e.g. 0.2 cm) may be very optimistic because these small drops produce a lot of vapor, i.e. high voiding and may already start to freeze so that they can no longer participate in an explosive interaction. The importance of freezing for the benign explosion results reported is not discussed.

There is no instrumentation in PREMIX to provide information on the breakup characteristics. Our approach easily spans all regimes, from a coherent jet (large length scale) to a broken-up cloud. The transition is controlled by the breakup parameter. The cases provided in the report were a selection from trial runs over a much wider variation of initial size and breakup rates. Freezing is not important here due to the short contact times. In considering what kinds of voids can be produced with what kinds of drop sizes, the reviewer should take a look at the new MAGICO runs (see addendum to Appendix B of DOE/ID-10504).

16. 3.3 Modeling of explosions

The most important finding of the calculations in this area is the cutoff that occurs at higher void fractions. However, the model used to describe explosive interactions - the microinteraction model - has been developed on the basis of experimental observations in a situation with virtually zero voiding. The parameters of the model have been fixed using these experiments and it has been shown that the model can be made to give results looking reasonable (by proper parameter choices) by simulating a KROTOS experiment in which the local void fraction was assumed to be between 25 and 40 %. It has been the declared purpose of the microinteraction model to explain the occurrence of strong pressure increases in the presence of large amounts of water (low fuel to water mass ratio). And as such it is highly interesting from a scientific point of view and may be very relevant - in this special situation. But one cannot expect this same model (with the same parameter settings) to work properly in a completely different situation in which there is very little water present. The failure of this special interaction model to predict strong steam explosions under conditions for which it wasn't designed does not necessarily say anything about the occurrence of steam explosions in situations as suggested by the premixing calculations should these ever

occur. Especially in the case of larger melt masses (and possibly smaller overall void fractions) the lower plenum of a pressurized water reactor might provide enough external confinement for completely different interaction mechanisms to become effective. These mechanisms may need more time for their development but might in the end arrive at similarly effective interactions. An important example of mechanisms that may contribute to such alternate types of interactions are the thermal fragmentation mechanisms that may not need much water and are completely left aside in the present study. This might explain why the most efficient explosions are obtained very early (prior to 0.12 sec) followed by much less efficient interactions at later times in all cases with a finite breakup parameter.

Here we are interested in highly supercritical multiphase thermal detonations, as only they can challenge the lower head. It is not clear what "new type" of interaction the reviewer speculates about, but whatever it is, it is clearly of no interest here.

17. The picture is less clear in the cases in which additional breakup was assumed not to occur. As outlined in the previous section these might be the most interesting cases in this study. Here no clear maximum of explosivity has been found among the cases considered and it is argued that 'slightly broken up premixtures remain very benign.' However, Table 6.1 shows that in the case C2-nb the maximum peak local impulse is 30 kPa.s which may already be viewed as a low to intermediate value and that it occurs at the last trigger time considered, i.e. 1.0 sec. Nothing in the results presented supports the idea that the value might not be larger (and maybe important) at even larger triggering times.

See further calculations provided in the addendum to Chapters 5 and 6.

18. There is a further and independent argument for early triggering. It states that early triggering is due to the interaction of melt (jets) with structures. This widely used contention, however, does not agree with the observations from the PREMIX experiments at Forschungszentrum Karlsruhe. We have now performed 11 such tests and in 4 of these the melt was forced to interact with structures (vertical 'jet' on horizontal plate - in one case even equipped with compartments). Only one of these tests (the last one performed on 21 August 1996) lead to a violent thermal interaction (a weak steam explosion) about 0.8 sec (almost a full second!) after melt-structure contact [8]. One may also make reference to the

KROTOS tests, in which the otherwise very explosive alumina melt settled at the bottom of the reaction vessel copying its shape when solidifying, in cases in which the water was saturated and no external trigger was applied [5]. So, melt-structure interaction does not necessarily provide early triggering.

We did **not** limit our range of interest for trigger times based on such kinds of arguments.

19. 4. Summary

The affirmative final result of the study follows from three findings: low corium-water mixing rates, very high void fractions in the premixture, and, partly depending on that, effective explosions being possible only during a subsecond period at the beginning of premixing. I have serious doubts about all three of these. With respect to the melt relocation scenarios I doubt that the present state of knowledge allows to definitely exclude downward relocation paths that could lead to much larger relocation rates. Not really being an expert in this field I must leave the judgement to those experts, provided they can positively defeat my arguments. In addition, processes that are induced by a first (weak) steam explosion might lead to a more effective melt-water mixing and thus to a larger steam explosion. With respect to premixing, the very high void fractions predicted by the code PM-ALPHA even outside the gas channel that immediately follows a mass plunging into water don't seem to be supported by experimental evidence. The code itself is not provided with sufficiently mechanistic models and is not sufficiently validated to support the high void fractions by itself. With respect to the explosions, the failure of the code ESPROSE.m, i.e. the peculiar interaction model in it (the microinteraction model), to predict efficient explosions in highly voided premixtures, doesn't prove that such explosions were not possible on the base of different interaction mechanisms, even if highly voided states would occur.

We strongly disagree with all points in this summary. Each one of them has been refuted above. Apparently the reviewer cannot see the impossibility of propagating supercritical thermal detonations in highly voided premixtures. This is the most trivial part of the subject, and until he resolves this in his own mind, we can make no progress here.

20. Literature

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[2] F. Huber, A. Kaiser, M. Steinbruck, and H. Will, PREMIX, Documentation of the Results of Experiments PM01 to PM06, Forschungszentrum Karlsruhe Report, FZKA 5756 (March 1996)

[3] L. Meyer and G. Schumacher, QUEOS, a Simulation-Experiment of the Premixing Phase of a Steam Explosion with Hot Spheres in Water, Base Case Experiments Forschungszentrum Karlsruhe Report, FZKA 5612 (April 1996)

[4] L. Meyer, The interaction of a falling mass of hot spheres with water, 1996 ASME/AIChe/ANS National Heat Transfer Conference, Houston, TX, August 3-6, 1996; ANS Proceedings, HTC-Vol. 9, pp. 105-114

[5] H. Hohmann, D. Magallon, H. Schins and A Yerkess, FCI experiments in the aluminum oxide/water system, Proc. CSNI Specialist Meeting on Fuel-Coolant Interactions, Santa Barbara, CA, January 5-8, 1993, U.S. Nuclear Regulatory Commission Report NUREG/CP-0127, NEA/CSNI/R(93)8 (March 1994) pp. 193-201

[6] S. Angelini, T. G. Theofanous, and W. W. Yuen, The mixing of particle clouds plunging into water, Proc. 7th Int. Mtg on Nuclear Reactor Thermal Hydraulics NURETH-7, Saratoga Springs, NY, September 10-15, 1995, NUREG/CP-0142, Vol. 3, pp. 1754- 1778

[7] T. G. Theofanous, W. W. Yuen, and S. Angelini, Premixing of Steam Explosions: PM-ALPHA Verification Studies, Report DOE/ID-10504 (September 1 996)

[8] H. Will, private communication (to be presented at the OECD/NEA/CSNI Spec. Mtg on Fuel-Coolant Interactions, Tokai, Japan, 19-21 May 1997)

F.10. Response to F. Mayinger (U. München)

General Comment and Highlights

General and unqualified agreement with the conclusions of the work under review.

Point-by-Point Responses

1. Not being an expert in structural mechanics, I shall concentrate my review on the thermo-fluiddynamic part of the report, trying to give an overall assessment.

For my review, I also took into account the report DOE/ID-10503 "Propagation of Steam Explosions: ESPROSE.m Verification Studies", a paper by S. Angelini u.a. on the Mixing of Particle Clouds Plunging into Water /1/ and another paper by Chen u.a. on the Constitutive Description of the Microinteractions Concept in Steam Explosions /2/.

1. Problem

There are many papers in the international literature dealing with the phenomena and the effects of steam explosions. They differ widely in their statement on explosion loads depending on assumptions or predictions for premixing, heat transport between molten fuel and conversion of thermal energy into mechanical loads. Experiments were made with various melts, representing a variety of boundary conditions (from one dimensional to multidimensional) and a wide range of scale.

The report under discussion here does deliberately not make the hopeless attempt to find an agreement or an average between the wide spreading results of the literature. It furthermore is based on carefully planed experiments, performed by some of the authors and on constitutive descriptions of phenomena, involved in steam explosion processes.

Object of the study is the advanced pressurised water reactor AP600 or respectively the integrity of its pressure vessel against hypothetical loads of steam explosions.

Entering the jungle of phenomena and effects connected with and resulting from steam explosions with the aim to come to a quantitative and physically reasonable result with respect to the mechanical behaviour of a pressure vessel is a task, which cannot be fulfilled in a complete, best estimate way on the basis of today's

overall knowledge. This is the case in spite of the fact, that numerous research work has been performed world-wide and that the authors of the report, being under discussion here, made excellent contributions, analysing steam explosion phenomena and effects in a theoretical and in an experimental way. There are many intangibles in steam explosion processes. Being forced to demonstrate the safety margins of a pressure vessel against steam explosion loads in a way, which is resistant against critical questions, it is quite obvious to apply conservative assumptions.

The design of the AP600 "invites" such conservative assumptions, because, besides the low power density, the core is not only surrounded by a pressure vessel with a rather thick wall, but also by a stainless steel reflector inside the core barrel. So the AP600 design can "tolerate" conservative assumptions. By doing this and regarding the results, one has to be very careful with any attempts to transfer the data, obtained for the AP600, to other pressurised water reactors. Conservatisms, assumed when calculating the thermo- and fluid-dynamic situations during steam explosions, could lead to predictions with respect tc pressure vessel failures, which are far beyond the physical reality under such an hypothetical accident. Therefore, inspite of the fine work presented in the report DOE/ID-10541, there is still a lot to do to obtain a still more realistic basis for safety analysis and realistic predictions. However, we must also be aware of the fact, that there always will remain many intangibles within the scenarios of hypothetical severe accidents.

2. Melt relocation characteristics

Melt relocation characteristics are influenced by the heating up of the uncovered core, the transition to a molten pool, the availability or non-availability of downward relocation paths and several melt release conditions. The authors very carefully analysed all processes, preceding or being involved in melt relocation, including blockage coolabilities and the resistance of the reflector and the core barrel against melt-through. The conclusions, drawn from the calculations and physical considerations, are convincing. The two main conclusions, namely that

the failure itself can be expected, that it will be local azimuthally and very near to the top of the oxidic pool and that

the release will occur within a time-period, which is within the coolability of the lower blockage,

are presented in chapter 4 of the report (see page 4-25) and give the good feeling, that the maximum amount of melt, which can interact with the water in the lower plenum, forming a steam explosion, is limited and by this also the energy release and the mechanical load onto the pressure vessel wall would be within a reasonable frame. So the limitation of the energy scenario, by carefully studying melt relocation characteristics, is a very important and very commendable contribution of this report to the state of art in steam explosion analysis.

A further, very important result in this chapter is, that "re-flood scenarios" need no further consideration from a steam explosion standpoint (lower head integrity). This conclusion should and could have consequences for future planning of accident management activities for existing pressurised water reactors, also. It means, that any effort should be undertaken to add water again into the pressure vessel after a beginning core degradation, because it would be of advantage for preventing a further escalation of a severe accident.

3. Quantification of premixtures

The authors of the report came to the result, that for the AP600 the amount of melt, pouring into the lower plenum through the downcomer, would be in the order of a few hundred kg/s. Based on this information, they determined the range of premixtures of melt, water and steam and their distribution on the way to the bottom of the vessel. Their calculations are based on fundamental aspects of the premixing phase, which a part of the authors studied seriously in experiments (the MAGICO-2000), involving well-characterised particle clouds mixing with water /1/. In these experiments, they performed detailed measurements on external and internal characteristics of the mixing zones. Mixing in saturated and in subcooled water was studied. The results of these measurements found entrance into the PM-ALPHA code, which they at first used for interpreting the experimental results and which is the basis for the analysis of quantifying premixtures during a hypothetical steam explosion scenario in an AP600. Interesting phenomena they found were the formation of densely packed regions and of instabilities at the penetrating front (isothermal conditions) and local voiding in the mixing zone, as well as global voiding through the level swell (hot pours).

It should be mentioned here that the original 2D PM-ALPHA code was extended to a three-dimensional version - called PM-ALPHA.3D - version. The results,

predicted for the AP600, showed, that premixing mainly takes place in the downcomer and at its lower end to the lower plenum. The average mixture zone and voidage zone is mostly shorter than 1 metre and the average fuel length scale varies between a few millimetres and 2 cm. It takes a few tenth of a second until enough small molten particles are formed during the mixing process.

This gives hope, that a very first steam explosion will occur, before a larger amount of finely dispersed molten liquid is mixed with the water and that this very first steam explosion produces such a high voidage (steam) in the waterpool, that a further large steam explosion can be avoided. It is obvious, that the authors do not study this possibility, because it cannot be quantified, but it may be allowed to mention it in this review. Roughly speaking, one could perhaps say, that early, small steam explosions are the best guarantors, that large dangerous steam explosions probably won't occur in case of mixing hot melt with water.

Another fact, which limits the momentum of a steam explosion, is the high voidage in the mixing zone, extending over a large part of it. This voidage has a strong damping effect on the migration of pressure pulses, because it offers a compressible volume.

The mixing deliberations and calculations, presented in the report, are physically well based and deserve a high grade of credibility.

4. Quantification of explosion loads

There are two key phenomena influencing loads of steam explosion. These are

- the mixing of particle clouds plunging into water and

- the microinteraction between water and melt.

The first phenomenon was discussed in the chapter before. For describing the microinteractions between melt and water, the authors followed two ways. For describing the microinteraction and for simulating the propagation of steam explosions, they used the computer code ESPROSE.m. This code is based on a series of experiments - the second parallel way - which were performed in the so called SIGMA-2000 facility /2/. Originally the formulations for the microinteraction were based on the assumption, that the rate of coolant mixing between debris and water is proportional to the melt fragmentation rate. This is a reasonable assumption and by this it was possible to produce consistent comparisons from available experiments for a wide range of steam explosion loads, starting

from weak propagations to supercritical detonations. The first formulations were mainly based on experimental results, obtained in the KROTOS facility. This first formulation was done for two-dimensional geometries and could especially also demonstrate the mitigating effect of "venting", due to wave reflection at a free liquid surface. Supercritical detonations were observed in the KROTOS facility with aluminium oxide melt only, pouring at very high temperatures into water.

In a next step, the constitutive equations were assessed by using experimental results, obtained in the above mentioned SIGMA-2000 facility. These experiments were carried out with molten tin drops, having temperatures up to 1800 C, impinging into water. Of course one can argue, that there are scaling effects, if one wants to draw conclusions from the measured and evaluated data, gained in this small experimental set-up, to the steam explosion loads to be expected during a severe accident in an AP600 reactor. According to the reviewer's opinion, these scaling problems however are mainly with the mixing of particle clouds, plunging into water, a problem which was discussed in the chapter before and which was solved by the authors with the help of the computer code PMALPHA.3D.

The SIGMA-2000 facility was experimentally very well equipped and special measuring techniques, like radiography, gave very good quantitative information about the fragmentation of the drop mass and its distribution. The fragmentation, measured with X-ray flash, was reproducible within less than 20%, which is a very good accuracy for such types of experiments. In addition the fragmented melt was collected after freezing and was subjected to sieve analysis. Very fine fragmented particles were analysed via scanning electron microscope photographs. Generally speaking these experiments are a very reliable basis for assessing a computer code like ESPROSE.m-3D, according to the opinion of the reviewer.

In the SIGMA-2000 facility, not only the fragmentation rate, but also the pressure signals of the steam explosions were recorded by using high speed pressure transducers. Due to the small scale of the facility, these pressure signals may be conservative when applied to a large scale geometry, like the downcomer or the lower plenum of the AP600. In a large volume, in which fragmentation of a hot melt starts, there are always voided areas, damping pressure propagation.

The verification of the ESPROSE.m-code is very well documented in the report DOE/ID-10503. This report documents how the various effects in steam explosion progress, like wave dynamics, explosion coupling and integral behaviour were assessed. The report demonstrates how the code is handling pressure waves in single and in two-phase fluids and this not only in a one-dimensional, but in a two-dimensional geometry. Special attention was given to reflection and transmission behaviour. The comparison between predicted data (ESPROSE.m-code) and experimental results showed very good agreement for a wide variety of thermo- and fluid-dynamic parameters. The local situations and the temporal behaviour are well predicted. So, the code is in a condition, that allows to predict steam explosion behaviour also beyond the experimentally verified area.

The extrapolation from the small scale to the large geometry of the reactor were done by using the basic equations for wave dynamics in multiphase media and constitutive laws for microinteractions. The latter ones were refined via experiments in the SIGMA facility, also. The combined theoretical and experimental efforts are a very good basis for predicting and simulating large scale conditions, also.

Finally one has to ask the question on "substance scaling" i.e., the applicability of the data, measured with modelling melts to liquid corium. The experiments were mainly performed with tin and with aluminium oxide. Especially aluminium oxide is very likely to produce supercritical steam explosions when it is mixed with water. The authors of the report DOE/ID-10541 write on page 2-1 (chapter 2 "problem definition and overall approach"):

"Also, it is important to note, that within the limited experience with reactor fuel material (UO2, ZrO2), we have no evidence of explosions, but rather extensively voided premixtures (Huhtiniemi et al., 1 995), nor is it known whether or under what conditions such premixtures can be triggered to explode".

With respect to "substance scaling" the data, presented in the report DOE/ID-10541, on explosion loads, originating from steam explosions are on the safe side without any doubt, because a corium melt/water interaction will produce much softer pressure pulses than experienced in the experiments with aluminium oxide melt/water interactions.

5. Integration and assessment

In the chapter 7 "integration and assessment", there are two very important statements, namely that

- from a more global perspective, the only way "to potentially produce a significant structural challenge on the lower head, would be by having a highly subcooled pool in it" and

- "even a postulated rapid reflood scenario could not produce the condition of concern..." .

After depressurising the primary system, following an hypothetical, severe accident, there is always and everywhere saturated (not subcooled) water in the lower plenum of the pressure vessel. This would be true not only for the AP600, but also for all other pressurised water reactors.

So as long as one can guarantee, that the cooling of the lower core support structure is good enough to prevent it from failing and core melt flows from the side to the lower plenum, steam explosions, originating from it, should not be a problem.

The second statement is as important as the first one, because it eliminates doubts, existing up to now, whether it would be advisable to try to flood a degraded core again after a certain escalation of a severe accident. This point was briefly discussed already in a former chapter of this review. Therefore in future accident management planning, there should be given more effort to in-vessel cooling also after a partial core disintegration.

6. Conclusions

I fully agree with the conclusions presented in chapter 9 of the DOE/ID-10541 report, to the statement of the authors, that "because of the wide margins, due to these controlling physics, it has been possible to bound uncertainties to a sufficient degree...", I would like to add, that these "wide margins" are still on the conservative side and the mechanical loads onto the pressure vessel and its lower plenum would be lower in case of a hypothetical severe accident, than predicted in the DOE/ID-10541 report.

Finally I would like to congratulate the authors to this fine work, attacking a very difficult but important problem and solving it to a great extend from an engineering point of view, but based on controlling physics and on reliable constitutive laws for the fluiddynamics to be expected in steam explosion scenarios.

1 S. Angelini, T.G. Theofanous and W.W. Yuen, The Mixing of Particle clouds Plunging into Water, NURETH-7, Saratoga Springs, NY, September 10-15, 1995, NUREG/CP-0142 Vol. 3, 1754-1778

2 X. Chen, W.W. Yuen and T.G. Theofanous, On the Constitutive Description of the Microinteractions Concept in Steam Explosions, Proceedings NURETH-7, Saratoga Springs, NY, September 10-15, 1995, NUREG/CP-0142

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F.11. Response to F.J. Moody

General Comment and Highlights

General and unqualified agreement with the conclusions of the work under review.

Point-by-Point Responses

1. *The purpose for reviewing the subject report, with several other companion documents, was* **to assess whether "in-vessel retention" is demonstrated to be an effective severe accident management concept for a reactor like the AP600.**

I have reviewed the work, and conclude that **in-vessel retention has been shown to be an effective severe accident management concept for reactors with geometry fluid quantities event sequencing and thermophysical properties similar to those pertaining to the AP-600.**

The documents provided for this review describe the steps taken to understand and predict the complex, multi-faceted subject of steam explosions. Associated phenomena have been closely simulated by experiments, and predicted with deterministic theoretical formulations (causal relations) to a degree of accuracy that makes confident predictions possible for full size AP-600 systems. It appears that all controlling physical effects have been included, even without the need for a complete understanding of the exact timing and conditions necessary to trigger steam explosions. Already known or conservatively estimated ranges have been placed on parameter, timing, and scenario path uncertainties, and still it has been shown that the expected range of lower head steam explosion pressure loads do not intersect the vessel fragility curve.

I was asked specifically to review the material on steam explosion loads, as discussed in

"Propagation of Steam Explosions: Esprose.m Verification Studies"
by T. G. Theofanous, W. W. Yuen, K. Freeman, & X. Chen,
DOE/ID-10503, August 1996.

The documents provided for this review collectively lay an extensive foundation of information, which testifies to the technical stature, competence, thoroughness, and integrity of the investigators. Indeed, the overall work is monumental in its scope and achievement, and it is communicated in a writing style which is one of the most scholarly to be found in reactor safety studies. Both the authors and sponsors should be commended for a carefully formulated investigative

strategy (strong, in-depth, well-blended steps) resulting in the highest value obtained for the time and resources spent. Beyond steam explosions the progress and understanding achieved in this work are likely to exert a major beneficial influence, both methodological and technical, on other significant and complex thermal-hydraulic issues.

SUMMARY

1. The ROAAM has shown that vessel loads, resulting from a comprehensive range of severe accident scenarios, melt conditions, relocation flow, timing of release from the core region, and thermal-hydraulic processes between the melt and surrounding water, lead to the conclusion that vessel failure is "physically unreasonable" in an AP-600 type reactor. Parameters including pool geometry, melt release rate, shock explosive formation and propagation, and venting yield load distributions on the vessel wall which were compared with the fragility curve in order to arrive at this conclusion. It is my opinion that even though all the mechanisms contributing to steam explosions are not fully understood, results embrace the extent of refinements which could eventually be made by further experiments and theoretical model (causal relation) development.

2. I agree that it would be useful to obtain data from the QUEOS experiment for a fully saturated water system) although it would not change the conclusion that vessel failure in AP-600 type reactors is "physically unreasonable." The value in such a test is to fill in a parameter range to give a more complete data base, and permit the technology to be extended to non-AP-600 type systems.

3. One potential benefit of the ROAAM procedure is that it conceivably could be used in reverse. Suppose it was concluded that a system failure probability was larger than acceptable. The ROAAM could be employed to display which parameter(s) dominate the outcome, thus pointing the way for design or procedural changes to reduce the failure probability.

* *

2. 4. How does the ROAAM accommodate different causal relations, such as PM-ALPHA and ESPROSE.m, at different stages in the methodology if they might be strongly coupled through common variables? That is, the behavior of two systems alone may be altogether different when they are coupled together (like two spring-mass systems). The probability distributions of the parameters

involved may combine differently when the separate systems are strongly coupled, leading to different probability ranges on the variables which determine success or failure of a system or process.

No such dependency can be identified here. Breakup and triggering are conservatively bounded with respect to both premixing (PM-ALPA) and propagation (ESPROSE.m).

3. 5. The source term for area production in Appendix A of DOE/ID-10503 is based on the assumption of particle number density remaining constant, while their size changes. A bit more explanation or justification would help. Wouldn't it make more sense to predict interfacial area growth by the formation of more particles as the melt decelerates in water? Taylor instability was employed to obtain the Bond number criterion in interfacial area growth. Could that model be employed to obtain a fastest growing wave length and droplet formation?

During propagation the key mehanism is Microinteractions, and this involves fine scale fragmentation and mixing in the vicinity of all macroscopic particles. This is clearly supported by the SIGMA experiments. Both Taylor and Helmholtz instabilities have been employed in the consideration of hydrodynamic fragmentation. Ultimately it is more appropriate at this stage to rely on correlations suggested by such approaches and the SIGMA data, which fully represent behavior in large scale explosions. This is our approach.

4. 6. It appears that in the heat transfer predictions of PM-ALPHA in DOE/ID-10504, flow regimes are identified by steady state correlations. Are these likely to be nonrepresentative for such transient events as fragmentation, and not provide a conservative characterization of the actual heat transfer?

This question is not clear. Fragmentation is relevant to propagation (ESPROSE.m) not premixing (PM-ALPHA). heat transfer of the fragmented debris is conservatively taken to be infinitely fast (thermodynamic equilibrium assumed in the m-fluid).

5. 7. Convective and radiative heat transfer from the fuel to the coolant is estimated in much detail, drawing from various experimental studies between coolant and heated solid surfaces. Is there a backup analysis to show that for the rapid heating associated with steam explosions, the heat transfer is not limited to how fast it can escape from the molten particles? Are there potential droplet sizes, relative velocities, and fluid properties where internal conduction (or convection) might limit the heat exchange rate?

This really depends on the resulting size of fragments. For 1 μ m particles the time constant is $10^{-2}\mu$s. We believe ignoring this limitation, by assuming instant equilibrium, is appropriately conservative.

6. STRATEGY

The severe accident management strategy addressed involves the retention of core material in the reactor vessel following a postulated severe accident in a reactor like the AP-600 design. Inability to cool the core leads to melting of core material by decay heat, and relocating it in stages to the reactor pressure vessel (RPV) lower plenum. Molten core debris, which may flow to the bottom of the lower plenum can melt through the RPV wall and undergo release to the containment. However, flooding the cavity to submerge the RPV bottom head is expected to be a means of arresting the downward relocation of molten core debris.

Even if downward relocation of molten debris is arrested, there is the possibility that some mass of debris could drop into water present in the lower head region, causing a steam explosion and further damage. Part of the overall study shows that failure of the bottom head by exceeding its structural integrity is "physically unreasonable".

THE RISK ORIENTED ACCIDENT ANALYSIS METHODOLOGY (ROAAM)

A primitive method of handling uncertainties in power systems came in the early 1960's (Moody F. J., "Probability Theory and Reactor Core Design," GE Report # GEAP 3819, US AEC Contract AT(04-3)-361, January, 1962). One of the greater concerns for a nuclear core during normal operation was reaching the "burnout" condition, where a hot spot in the fuel could exceed design limits, and cause fuel damage. The fuel temperature could be expressed as a function of several variables and parameters (causal relations), each with its own degree of uncertainty. If one chose the most pessimistic limit of each variable and parameter, the "burnout" limit could be exceeded. The most optimistic limits the "burnout" limit would not be exceeded. It was suggested that probability methods could be applied to give a reasonable assessment of the likelihood of exceeding the "burnout" limit. Data from power plant operating logs was gathered to obtain probability distributions for certain variables and parameters. Wherever data was not available, "expert opinion" was solicited. The results were then combined by the method proposed in an ASME paper (Kline, S. J., and McClintock, F. A., "Describing Uncertainties in Single Sample Experiments," Mechanical Engineering, January, 1957), which resulted in the expected mean and standard

deviation for the hot spot temperature. Comparison with the established design limit showed that it was "physically unreasonable" to expect "burnout" in most cases.

The ROAAM is an extensive, operational methodology which is more refined than any of its primitive predecessors. It has the capacity for incorporating causal relations (describing equations relating the variables and parameters), based on well-understood physics for the applicable phenomena, with specified parameter uncertainties, scenario bifurcations, and even a diversity of expert opinion. The process leads to a rationally-based prediction of those properties which determine the success or failure of a system or process.

The structure of ROAAM embraces the current phenomenological state-of-the-art, built-in activation response of safety and control systems, man-machine interfaces, and procedural understanding. As new information becomes available, the ROAAM can accommodate it. Where expert opinions may be diverse, the ROAAM provides a means of focusing further research to narrow the disagreements. That is, when experts strongly disagree on the range of a parameter, the ROAAM can be employed as a tool to display the sensitivity, showing if the parameter dominates the outcome, or is only a minor percentage effect on the overall result.

One question about use of the ROAAM involves the causal relations for various phenomena. If the parameters in a causal relation are independent, their probabilities can be combined in a certain way to obtain the expected mean and standard deviations of that function. If the parameters are not independent, the combination is more complicated. The question involves how the ROAAM accommodates the possibility that some parameters appearing in more than one causal relation may not be independent. How would results from ROAAM compare with one deterministic mega-computation where all the parameters are treated by something like a monte-carlo process to obtain the distribution of variables which determine success or failure of a system?

One could always hard-wire all the models in a ROAAM analysis to one mega-computation. There would be no advantage (any dependencies can be handled just as easily), and there could be some important disadvantages; for example, in determining the bounding conditions for rate of breakup and trigger time. More importantly, such a mega-computation would be less scrutable, and much reduced in degrees of freedom practically explorable.

7. ROAAM APPLICATION

I have seen the ROAAM work in two separate campaigns to close severe accident issues, namely the direct containment heating (DCH) issue for one series of PWR's, and the Mark I liner melt issue for one class of BWR containment. It is appropriate that this methodology should be applied to reach a conclusion on the in-vessel retention severe accident management concept.

Application to in-vessel retention embraces possible scenarios, melt conditions, coolant states, structural properties, debris mixing with water, triggering, explosion wave dynamics, and lower head fragility. Parameter ranges are associated with the amount of participating substances, the timing of events, event paths, and state properties of various subsystems. Several analytical tools, based on physical models, provide the causal relations employed, namely PM-ALPHA for enveloping the effect of melt breakup in water, ESPROSE.m for enveloping the effects of fragmentation and microinteractions on steam explosions, and ABAQUS v.5.5 for enveloping the lower head failure criteria. The computer programs used for causal relations to envelope important variables have be compared with other analyses and experimental data to a level where their predictive capability of the tested parameters does not introduce uncertainties which are significant enough to consider.

The following comments are offered to help substantiate my conclusion that in-vessel retention has been shown to be an effective severe accident management concept for systems like the AP-600.

MELT INTRODUCTION AND FRAGMENTATION

Early predictive models provide core melt scenarios and relocation rates with and without reflood, which can arrest the melt progression. However, the melt state which may reach water in the RPV, and the subsequent breakup and penetration largely determine the rate of heat transfer, steam formation rate, and possible shock pressure loads. A quantity of melt arriving at the water can undergo Taylor unstable breakup or droplet formation at the leading edge and Helmholtz breakup or droplet stripping on those surfaces with parallel velocity components. The PM ALPHA model has been developed to incorporate the melt and coolant properties, and provide an envelope for the expected range of momentum, heat transfer, and phase change interactions associated with breakup for premixing considerations.

Single particle and particle cluster experiments have been employed to test predictive capabilities of particle motion and energy transfer dynamics in water (the MAGICO and QUEOS experiments). Particle cloud elongation, steaming, spreading, and mixing with surrounding water are captured by the PM-ALPHA code, which is employed as a causal relation in the ROAAM. Comparisons include particle cloud distortions associated with release door opening time, particle, and void volume fraction contours. Of particular interest is the pinching of the vapor volume behind moving particles, caused by condensation for the particle introduction into subcooled water. Since the condensation acts to reduce mechanical energy transfer, I agree that it would be useful to conduct QUEOS experiments in fully saturated water.

* *

8. One of the most important considerations in fragmentation is the formation of new melt heat transfer area. Appendix A in DOE/ID-10503 describes the "source term" for interfacial area production. Equation (3.69) is based on a change in size of particles for the same particle number density. It seems that before particles have reached a stable size? they would undergo the formation of new particles. This assumption needs more explanation.

The source terms in interfacial areas are very different in breakup during premixing modelled in PM-ALPHA, and in fragmentation during propagation modelled in ESPROSE.m. This comment mixes up these two. The single particle approach, as also explained above, is appropriate for fragmentation. For breakup we, in fact, have included both changes in numbers of large particles, as well as change of their size due to their shedding of very fine particles. This is explained in Appendix A of DOE/ID-10504 (PM-ALPHA verification).

9. STEAM EXPLOSION

The mechanics of steam explosions are described in DOE/ID-10503, detailing melt introduction to water, interfacial breakup and premixing of debris particles with water, the effect of voiding around the particles on heat transfer, the triggering of explosions, and propagation of pressure waves with reflections from rigid mechanical and gas-liquid interfaces. It was earlier found that 1.0 GJ of energy could fail the lower head. However, further understanding has led to a reexamination of the mechanics of steam explosion force generation to determine a more realistic criterion for lower head failure.

It was determined that the AP-600 could withstand 500 bars of pressure for milliseconds without failure. Computations with the ESPROSE program displayed the difficulty in generating such pressure impulses with attenuating phenomena like voiding, which resists triggering, and pressure venting from the water surface. Extensive development of ESPROSE have been performed with both data from the SIGMA and KROTOS experimental facilities. Simpler analytical models have provided assurance that ESPROSE accommodates detonations, shock propagation, and reflection.

Significant effects embraced by ESPROSE result from the physics incorporated, which are consistent with experiments. Calculations show the strong attenuation of shock pressure loads with distance, and time by venting from the water free surface in the AP600 systems. It is also realized that venting may not significantly reduce loads if the water depth is high in the lower head. Strong evidence is supplied that ESPROSE incorporates the appropriate physics, and can be used with confidence to provide the causal relation for enveloping the effect of trigger time on steam explosion severity.

* *

10. The physical mechanisms considered by ESPROSE.m include shock pressure propagation from a trigger, which collapses voids, forces liquid onto the melt, producing fragmentation and microinteractions, escalated heat transfer, further steam formation, and rapid expansion (explosion). A statement on page A-18 of DOE/ID-10503 needs further clarification. Where the pressure increases rapidly ahead of an explosion front, why does the vapor become instantaneously subcooled? (If saturated steam is rapidly compressed, it would tend to follow an isentropic path off the vapor dome into the superheated region, not subcooled.)

What is meant is that the liquid becomes subcooled and steam is to rapidly condense.

11. On the same page, it is stated that behind the explosion front where pressure is decreasing, the liquid can become superheated. (If you decompress saturated water, the path drops into the steam dome.) It would make better sense to me (I can't speak for others) to note that the nonequilibrium states lag behind a steady state in the superheated or subcooled region.

Here, we refer to the liquid that participated thermally in the interaction (part of the m-fluid).

12. LOWER HEAD RESPONSE

Dynamic response of the lower head is based on well established physics of shells, modeled by the ABAQUS program. Mechanical failure of a shell depends not only on the magnitude of an applied load, but also on the frequency content. It is stated that the shock pressure loads which lie in the steam explosion envelope have a short period relative to the structural response, so that the peak strain would be essentially independent of the pressure pulse time profile.

The report has provided some "screening fragility" curves which would be used to determine if predicted steam explosion loads were of such a character that the failure criteria envelope and fragility curve need to be further blended to provide a failure likelihood. It was concluded from the range of pressure loads and the lower head fragility curve, that for all relevant severe accident scenarios, melt conditions, and timing of release from the core region, with ensuing mixing and explosion wave dynamics, steam explosion induced lower head failure in an AP600-like reactor is "physically unreasonable."

REVIEW OF STEAM EXPLOSION LOADS

The verification of ESPROSE.m, based on stepwise experimental measurements and comparison with simplified theoretical methods shows that reasonably conservative assessments of steam explosions are possible in the present version.

The discussions of DOE/ID-10503 provide foundational support of the physical modeling and numerical procedures to predict steam explosion properties for given melt addition rates and states. The basic physics involve wave dynamics, including sound wave propagation and shock development and propagation in a water-filled region. Two-dimensional calculations performed by ESPROSE.m are compared with simplified computations using the method of images and solutions similar to classical waterhammer. Some comparisons are included based on characteristic solutions. The results form a strong basis for concluding that the code is producing reasonable predictions for the expected range of input parameters. Pressure propagation speeds, attenuation from wave interaction at free surfaces) and wave amplification by reflection from rigid surfaces have all played a role in the verification.

Numerous two-dimensional ESPROSE calculational surfaces are compared with solutions obtained from the method of images, and found to be sufficiently similar,

leading to the conclusion that basic physics of explosions are included in the model. Several geometric parameters were varied, as was the source velocity function. Good comparisons were consistently achieved.

The SIGMA tests involved a melt droplet which was triggered at a specific position, leading to local pressure traces. Comparison of the pressure traces with ESPROSE calculations showed reasonable tracking of pressure waves originating from the droplet region to the rigid end of the test section, and reflection back toward their origin. Additional evaluation with the method of characteristics were provided. The wave dynamics, indeed, appear to be properly described in ESPROSE.

One piece of information lack pointed out in the report is that the data base needs expansion for microinteractions with reactor materials.

THE NEXT STEP

I understand that a number of experts are providing reviews of the documents provided. Some may believe (as I do) that even without a complete understanding of all the phenomena, the remaining uncertainties, processed by the ROAAM, still permit a strong statement about failure likelihood being "physically unreasonable." Some experts may feel that the uncertainty of a given parameter should be broader. This is a simple exercise in ROAAM, which would then provide output with a range that accommodates the particular variable uncertainty. Other experts may wish to change the causal relations to reflect various "bottom up" or fine structure effects. This is always a possibility, but may be unnecessary, since the causal relations are based on macroscopic formulations of basic principles. If it were recommended that nonequilibrium models be employed for causal relations. We would be farther behind than using ROAAM in its present structure, because nonequilibrium models would have to be verified by experiments.

When strong disagreements have resulted in physical modeling, small working groups have been formed to reach agreement on acceptable formulation, with appropriate modifications in ROAAM.

Finally, it is possible that some would disagree with the ROAAM structure itself, suggesting that it skews results, or simply blurs our ignorance of phenomena. I would argue strongly that the ROAAM blends (not blurs) uncertainties (not

ignorance) in a way that makes it possible to reach conclusions with a known level of confidence.

OVERALL CONCLUSION

As a curious person who enjoys formulating better theoretical models, based on more complete experimental understanding, I recommend additional experiments (e.g., QUEOS experiments with fully saturated water) to help close the few remaining gaps in our understanding of steam explosion phenomena.

However, I believe that the studies provided for this review give substantial, in-depth evidence to help conclude that in-vessel retention is supportable as a severe accident management strategy in AP-600 type reactors without additional work to close the issue.

* *

F.12. Response to B.R. Sehgal (RIT)

General Comment and Highlights

This is a generally agreeable re iew. Many detailed questions are raised, but these are mostly of a clarification and reinforcing character, rather than strong objections. Also, many useful suggestions and opinions are offered, again in the same light. Perhaps the key point is that the review expressed caution with regard to the maturity of the analysis tools. If this refers to a stage in the "phases of development," table (Table A.2 in Appendix A), we certainly agree.

Point-by-Point Responses

1. *I. Review of the Overall Approach*

This is the fourth time I have had the opportunity to review a body of work that Professor Theofanous and co-workers have produced for the resolution of a specific safety issue, or a specific concern. I believe, this is the most complex of all the issues (or concerns) so far and I believe, Professors Theofanous, Yuen and co-workers have done their finest work so far. This body of work is of greater, and of more lasting, value, than earlier efforts, since a major part of this work is the development and verification of the methodology to describe the steam explosion phenomena, and to predict the loads imposed by the postulated occurrence of a steam explosion. This methodology, and the codes developed, could be applied to other accident scenarios, than the one considered in the present application.

I believe, some comments are in order on the overall approach followed in these three reports, complemented, of course, with the ROAAM method, and the previous work that Professor Theofanous and his teams have performed, (e.g. for the Alpha mode failure of an LWR containment during a severe accident).

Professor Theofanous and co-workers, with their accumulated experience in steam explosion modeling and applications, have developed a very well-focussed overall approach in the body of work presented in the three reports. It is clear that an in-house experimental program was structured to provide the key observations, for the ideas needed, to advance the steam explosion modeling to the point where some meaningful predictions can be made. The innovative experiments performed in the MAGICO facility provided the germane ideas on steam depletion, and on the difficulty of obtaining pre-mixtures, which could lead to very large steam explosions. Likewise, the experiments performed on the SIGMA facility provided the basis for the micro-interactions concept for the steam explosion itself, i.e., the

concept and treatment of the m fluid. I believe, the experimental underpinning of the ideas and concepts employed, and the further verification of the methods used in the codes against the integral experiments, has provided great strength to the overall approach.

The overall approach followed, in the application report, conforms to the ROAAM method and employs the PM-ALPHA and the ESPROSE-m methodology. The extremely high values for the fragility curve made the task much simpler than the earlier applications of the ROAAM methodology, but it is well justified and credible.

Perhaps, the two points of possible short coming in the overall approach, which have also been admitted by the authors, should be stated:

First, is the question of maturity. Clearly, there is not enough separate-effect and integral-effect data to provide sufficient validation of the steam explosion methodology developed. This methodology employs many many correlation and submodels, whose individual verification is a monumental task. Nevertheless, an experimental verification matrix should be developed, with priorization of important effects, and executed, to provide greater verification of the methodology, thereby providing it greater maturity.

Second, a mechanistic treatment of the initial phase of the steam explosion scenario, i.e., the break-up of the melt jet, and its sequential fragmentation, has not been included in the methodology developed so far. The authors claim that this phase of the steam explosion process can be conservatively-bounded parameterically. Perhaps, the authors have done that successfully in this study, however, a more general treatment of the break-up phase, and its linking with the pre-mixture phase, should be pursued to provide greater assurance that all the initial-condition-effects have been taken into account.

The above two points, in no way, diminish the value of the overall approach, and the results achieved. The above two are outlines of further work to solidify the validity of the overall approach followed here, as, I believe, the authors have themselves identified. The present treatment of the physics is the "State of Art." I believe, rapid advances in understanding and modeling will follow the germ of ideas that the authors have provided here. Some of those advances will surely be accomplished by Professors Theofanous, Yuen and co-workers.

* *

2. II. Review of the Report DOE/ID-10504 (Sept. 1996) "PREMIXING OF STEAM EXPLOSIONS: PM-ALPHA VERIFICATION STUDIES" by T.G. Theofanous, W.W. Yuen, S. Angelini

This report is the verification document for the Code PM-ALPHA, which treats the premixing phase of the steam explosion scenario. The report has two important appendices: (a) which describes the PM-ALPHA models and (b), which describes a set of experiments in the MAGICO-2000 facility, in which several kilograms of high temperature particles of a specific material, and of specific diameter, are dropped into water to obtain observations and data on the pre-mixing geometries and void fractions. The front parts of the report provide the comparisons of the predictions with the PM-ALPHA code against the data from selected experiments. In the following paragraphs, I will provide comments on the main sections of this report.

II.1 Appendix B. "MIXING OF PARTICLE CLOUDS PLUNGING INTO WATER"

I am very impressed with the MAGICO facility. I believe the authors have performed outstanding experiments using quite high temperatures and respectable masses of the hot particles. The video pictures are outstanding. I am a bit disappointed with the quantitative data that could be obtained. The X-ray pictures (in reproductions) do not communicate any information and the void fraction data shown in Figures B.23, B.25 and B.27 is rather meager as a validation standard.

Point well taken. It was just too expensive to provide original prints of the X-rays. But we have more data now (see addendum to Appendix B).

3. The comparisons of the PM-ALPHA predictions to the measured data, shown in Appendix B, for the cold runs, show substantial differences in the advancement of the particle front. It appears that a central part of the particle cloud tunnels through the water. This is not predicted well by the code. For the hot runs, it appears from Figures B.26, that the calculations predict that the dense particle cloud also leaves the steam region behind, if a slight subcooling (3° C) is present. There are no comparisons shown for the hot runs, as shown for the cold runs in the Figs. B14 and B.15.

See new data and discussion per above.

4. *The concluding remarks state that the hot tests quantified local voiding in the mixing zone and global voiding through the level swell. Figures B.25 and B.26 indicate that the voiding front is coincident with the particle front, only, for the zero subcooling case. The particle front is substantially ahead of the voiding front for a slight (3°C) subcooling of the coolant. The extensive steam generation, indicated by the axial void profile also may increase the local subcooling by pressurization. I wish there was some quantitative data for the particle volume fractions, to compare in Figures B.25 and B.26. Was it not possible to obtain quantification of the spatial particle volume fractions from the X-ray pictures?*

Yes; see new material in addendum to Appendix B.

5. *In this context, if the PM-ALPHA predictions for the advancement of the particle front lagged behind the measurements in the cold runs (cf. Figs. B.14 and B.15), would they not do the same for the hot runs, since same modeling is employed for both hot and cold runs. I do expect that the steam generation, caused by the radiative heat flux on the coolant from the particle cloud, will retard the advancement of the particles. I believe, this effect is represented in the code, since a radiation heat flux model is employed, however, I can not quantify its effect, on the differences in the particle cloud distribution between the hot and the cold runs.*

In this, and the above two paragraphs, reviewer's concern is on whether the particle-void fronts and their relation are properly calculated. We have more detailed and complete data from MAGICO now that completely address this concern. These can be found in the addendum to Appendix B.

6. *The subcooled coolant is important. The only data shown for the 18°C subcooling case is the lack of measured level swell. I would be interested in the axial void fraction and the particle volume fraction profiles, to understand if there are significant phenomenological differences between the saturated and the subcooled cases, and if these differences can be predicted by the PM-ALPHA Code.*

As noted in the report, there was no measurable void in the 18 °C subcooled case. This was very well predicted by PM-ALPHA. Subcooling effects were key also in the interpretation of QUEOS, MIXA, and FARO tests — see respective sections DOE/ID-10504.

7. All in all, I believe the MAGICO experiments are relevant for the ideas, and data, on the mixing zone and the premixing conditions. I would like to connect the melt jet particulation to the particle-cloud water interaction. This may be in the next phase of authors experimental investigations.

* *

8. II.2 Appendix A. "PM-ALPHA: A (COMPUTER CODE FOR ADDRESSING THE PREMIXING OF STEAM EXPLOSIONS"

PM-ALPHA is a three (melt, coolant and vapour) field code employing separate mass, momentum and energy equations for each field. Thus, it is a very detailed code - more detailed than the codes RELAP-5 and TRAC. It also employs two and three dimensional geometry. Thus, it has capabilities beyond those of the conventional CFD codes, which, generally, employ only a single field. PM-ALPHA is a very advanced and detailed computer code, indeed. There are other codes, currently in development, in Europe, e.g. IVA (Siemens, Germany) and MC 3 D (CEA, France), which are also incorporating similar capability, in order to treat the very complex, and very dynamic, physics of melt-water interaction and steam explosions.

It is a general rule that more detailed the formulation for the description of a process, more detailed the information required to bring closure to the formulation; and more intuitively intelligent approximations have to be made to obtain credible solutions from the formulation. This is quite apparent for PM-ALPHA, when a whole page (A-20) is needed, to show the dimensional groups that appear in the constitutive laws for the fuel to coolant heat transfer. This can not be avoided, however, the collective constitutive laws may provide reasonably-correct predictions for a particular set of pre-mixing circumstances, and not for another set. I believe, that verification on an even less integral level than the MAGICO experiments should be considered by thinking-through, and devising, a set of separate-effect experiments. They should be prioritised, so that the most important are performed first.

We agree with this perspective, and this is why we have gone to great lengths to test the individual pieces as well. For example, the one-page equations referred to here were obtained from an experiment, the MUPHIN, conceived and carried out specifically for this purpose. So, the question really is whether we have left out something important. This is addressed with the reviewer's specific comments below.

9. In the following paragraphs, I will provide some detailed comments.

II.2.1 PM-ALPHA Formulation

The modeling approach is logical and well-thought. The authors admit that the formulation so far, emphasizes the multifield aspects of pre-mixing. The melt jet and particle break-up are treated parameterically.

Two length scales are employed for the fuel field: one large, encompassing the original fuel drops, or fuel-melt jet, which may break-up but still are considered as fuel; and the other small enough to be called a debris, which mixes with water and gets quenched. The decisions about the amount of the 'fuel' and the 'debris' are made with a correlation for the fragmentation rate.

The debris particles assume the same temperature, and velocity, as the coolant, instantly. They are not allowed to sediment down with gravity, as they would normally do. This assumption is justified for the time interval considered, if the particles are of micron size.

* *

10. The large-length-scale fuel particles are assumed to have uniform temperature. There is no treatment of the heat conduction from the fuel particle to the coolant. For the prototypic binary-oxide mixture melt, it is important to determine the solidification front growth into the particle, since it may either prevent fragmentation, or reduce the rate of fragmentation, thereby changing the heat source to the coolant.

Another factor in the treatment of the fuel particles, is the change in physical properties that occurs, as the fuel particles cool down from above-liquidus to below-solidus temperature. The increase in viscosity and surface tension affect the fragmentation characteristics, which in turn affect the terms in the debris mass equation, and in the liquid and debris momentum and energy equations. A paper submitted by Okkonen and Sehgal in the forthcoming FCI meeting in Japan discuss the two factors mentioned above for the behaviour of the fuel drops.

In fact, we have an option in PM-ALPHA for crust growth, and we have used it in the analysis of some ex-vessel explosions in deep water pools. This was not emphasized here, because the times and depths and other conditions are not conducive to significant solidification effects.

11. Recently, we at Royal Institute of Technology (RIT), have performed some experiments on the interaction of relatively low temperature cerrobend (an alloy with density of ≈9000 kg/m^3) jets with subcooled water. We have found that the jet breaks-up into small particles. There is a distribution to the particle size or mass, however, there were no particles of length-scale comparable to the jet diameter. In these experiments the jet breaks-up completely. The FARO experiments show a melt "cake" at the bottom, however, it is not clear whether it is the unbroken jet or an agglomeration of melt droplets belonging to some size distribution, which, perhaps, does not contain length-scales approaching the melt jet diameters.

Summarizing the above discussion, I believe, the treatment of fuel as having two length scales in the PM-ALPHA formulation is valid. However, the source terms in the equations should be reviewed again. The variation of properties of the fuel drops, with temperature, should also be taken into account; and the change in the temperature of the fuel drop should be calculated employing conduction equations. Melt jet, or drop, interactions with subcooled coolant may produce atomization, with no large particles of size similar to that of the melt jet.

As noted, the source term in the equations are varied parametrically to bound the behavior. Solidification effects are not important for in-vessel explosions, and even more so in saturated water pools. Moreover, ignoring this small effect is conservative.

12. II.2.2 Interfacial Momentum Transfer in PM-ALPHA

The drag correlation used in PM-ALPHA for fuel-coolant interface distinguishes between the dispersed and the dense fuel regimes. The latter is taken as that for flow of gas through a densely-packed bed. This correlation, perhaps, should be checked, since predicted penetration of the fuel cloud in the MAGICO experiments is less than the measurements. Also, comparisons could be made with the isothermal tests in the BILLEAU and the QUEOS facilities. The logic diagrams on pages A-16 and A-17 were helpful.

The densely-packed bed regime appears only in particles accumulating against a boundary. For comparisons with QUEOS see DOE/ID-10504. BILLEAU tests are not yet available.

13. *II.2.3 Interfacial Heat Transfer in PM-ALPHA*

There are many regimes of convective heat transfer and many correlations. The authors use the best that they can find. Then, there is the large effect of radiation heat transfer, which was found to be important for the comparisons to the QUEOS test data. Their synergism, and effects of one regime on another, may need further exploration. For example, radiation-absorption will produce vapour which will change the convective flow patterns of the coolant, and, perhaps, change the heat transfer regime. Some separate-effect tests could be designed to test the synergism and the effect of different convective regimes on each other, in order to test the heat transfer correlations package employed.

We have explored these avenues already, but there isn't really much new or surprising. Even for film boiling from single spheres, contrary to what one might expect, the superposition approach works very well. Also, it should be noted that our efforts here were not limited to collecting what we could find. The major components are non-local radiation heat transfer, and film boiling in single- and two-phase media. For the former, we formulated a whole new approach, and for the latter, we conducted the MUPHIN experiments and developed theories and correlations for use in the code.

14. *II.2.4 Fuel Break-Up and Fragmentation Modeling in PM-ALPHA*

I have referred to this earlier in the comments on the PM-ALPHA formulation. The interfacial area equation (3.73) assumes spherical particles on break-up and fragmentation. This may not be appropriate. Perhaps, data from FARO or other fragmentation-break-up experiments could be employed to develop a more prototypic interfacial area representation. In some of our experiments with cerrobend in subcooled water, we do not find spherical particles. Perhaps, in saturated water, with large flows of steam the particle shapes may be spherical.

The cerrobend particles are not spherical, because the material solidifies at such low temperatures. The dimension in Eq. (3.7) should be interpreted as characteristic length, or effective diameter. The source at this time is parametric, because there is no reliable model. However, this is sufficient for our purposes.

15. *The model for fragmentation of fuel drops is based on the Bond number. I believe, data on hydrodynamic and thermal fragmentation of large-size melt droplets may be available in near future. The model could be checked against such data, when available.*

The model for jet and large fuel-drop-break up is parametric with an input-specified parameter, β, whose value is varied in analysis. This approach is, perhaps, adequate for the present. However, it will be desirable to have a phenomenological/mechanistic model.

* *

16. The authors distinguish between fragmentation and break-up as two separate processes. In some of our melt jet-water interaction experiments, we were not able to separate the two processes. The jet breaks-up (or fragments) into particles having a size distribution ranging from submillimeter to 3-4 millimetres. The process appears to be concurrent and not sequential, as assumed in the parametric models described here.

Not at all. Our formulation is for concurrent, not sequential processes. One should be careful in how far tc take the cerrobend data.

17. II.3 VERIFICATION OF the PM-ALPHA CODE

The front part of the report DOE/ID-10504 describes the verification pursued for the PM-ALPHA code by performing analytical tests, and by comparing with the data measured in several experiments. This was a very large effort, and I believe, it has largely achieved its purpose. I will comment on a few comparisons of the data with the code predictions.

II.3.1 QUEOS Experiment

These experiments are similar to the MAGICO experiments. The comparisons shown in Figures 4 to 13 are remarkably good for such a dynamic process. The comparisons appear to be better than those for the MAGICO tests.

It is not clear to me what the experimental image actually implies, in terms of the distribution of hot particles, and of void. The pictures in Fig. 7 at 0.3 and 0.4 seconds seem to show that the experimental hot particle image may be not as advanced as the calculated contour. This also appears to be the case in Fig 6. at 0.3 and 0.4 seconds. The graphs in Fig. 8, however, show very good agreement between measured and calculated front-advance locations versus time.

As noted in the report, the front advancements in Figure 8 are from Lagrangian particles in the calculation. The Eulerian results show some numerical diffusion, and again, as noted, the results in Figure 7 were to be refined by calculating with finer grids. This is done now.

18.   II.3.2 MIXA Experiments

The MIXA experiments employ a Uranium-Molybdenum thermite melt of several kilograms, at 3600K, poured into near-saturated water pools. The melt jet was broken into 6 mm diameter droplets. The MIXA-6, analysed here, used 3 kg melt pour into very nearly (≤1 K difference) saturated water. This, thus, is a prototypic experiment, albeit with small mass.

The comparisons are very good. I am somewhat concerned about the sensitivity of the results to the break-up-cut-off- void-fraction and particle size. The authors recognise this, still, a change of only 5 % (85 % to 80 %), with the particle size of 1 mm, decreases the calculated pressure rise from 0.4 bars to ≈ 0.2 bars. Increasing the particle size from 1 mm to 1.2 mm at the 85 % cut-off level decreases the pressure rise from ≈ 0.4 to ≈ 0.28 bars. Thus, the breakup and fragmentation models appear to be very influential in the very high temperature, prototypic material experiments.

This is absolutely correct, and simply states the obvious fact that the steam production is a strong function of interfacial area. This also provides important perspectives on the whole question of breakup — what can resonably be expected from a prediction, and how far could such possible predictions be taken!

19.   II.3.2 FARO Experiments These are, perhaps, the most important experiments, since they use substantial quantities (≥ 100kg) of prototypic materials; and there are several experiments already performed and more are underway.

The comparisons shown are very good indeed. Unfortunately, FARO does not produce any data on the mixing region, thus the colour figures, presented, show only calculations and no data.

* *

20.   I did not understand why the initial particle size is chosen as 4 cm for a jet diameter of 10 cm. The β value chosen is 50, while for the MIXA test it was chosen as 20. The minimum particle size chosen is 1 mm, while in the MIXA test is was chosen as 1.2 mm.

It is inconceivable that the jet exited the nozzle and travelled all the way to the pool, totally undisturbed. We chose 4 cm as a large enough characteristic length scale. It does not matter really what you choose, as the process is really controlled by β, and only small scale have enough interfacial area to interact.

> 21. *One experimental result, which FARO produces is the fraction of the jet material deposited as a 'cake' on the bottom plate. This is not provided by the authors from their analysis with the PM-ALPHA code.*

As the reviewer notes in an earlier comment, it is not really clear what this 'cake' means, and there are many ways to interpret it.

> 22. *II.4 Numerical Aspects*
>
> *The authors do not provide a discussion on this topic. I believe, this is an important topic. The ICE technique is known to have significant numerical diffusion. It is not clear whether any advanced space-time discretization scheme was employed. Node sizes of several centimetres are generally not fine enough. The authors, perhaps, by now, have investigated the numerical aspects further, and I would welcome a greater discussion of this topic.*

There was nothing special employed, and elsewhere in the report we note that we may introduce such a special scheme at some future time. It was also noted that numerical diffusion can be well enough controlled for our purposes, by choosing fine enough grids.

> 23. *III. Review of the Report: Propagation of Steam Explosions: ESPROSE.m Verification Studies by T.G. Theofanous, W.W. Yuen, K. Freeman and X. Chen*
>
> *This report deals with the next phase in the steam explosion process, after the premixing has been achieved. The report, therefore, deals with the explosion process and develops a methodology to describe the process, and evaluate the energetics, which are then employed to assess the damage potential of the explosion on structures, which surround the explosion. A trigger is assumed, which starts the explosion process, in which intimate contact of the fuel and the coolant leads to production of large amounts of vapour, and the supercritical explosion.*
>
> *The report consists of the front part, where the results of the verification calculations are compared to the observations, and data, obtained in the SIGMA*

facility at U.C. Santa Barbara. The report also contains four important appendices in which the code models, a l-D characteristics model, constitutive laws for micro-interactions and thermal-detonations are discussed.

In the following paragraphs, I will comment on each of the major sections of this report.

III.1 Appendix A. THE ESPROSE.m MODELS

The overall approach of the model development is brilliant. Recognising that the dynamics of a pressure wave, generated by a trigger, coupled with fuel fragmentation, micro (or local) mixing and heat transfer result in energetic steam explosions, the authors have concentrated on those aspects. Perhaps, the SIGMA experiments provided the key observations towards the development of the micro-interaction concept and the m fluid, where the fuel-coolant heat transfer occurs. The energy transferred is then employed in the multifluid treatment to calculate the pressure fields as a function of time and space (2-D/ 3D). The damage potential is, then, evaluated with the calculated dynamic loading imposed in terms of kilo Pascal seconds.

The modeling approach is similar in most respects to that employed for the PM-ALPHA code, i.e., solution of a set of multifield conservation equations, with specified constitutive relations. The fields chosen this time are fuel, liquid and the m fluid. There is an additional mass conservation equation for the debris, i.e. the fragmented material. The m fluid equations contain source and sink terms, which are based on a picture of liquid-entrainment and phase change. Fuel fragmentation is included, which contributes to the increase in interfacial areas. The heat transfer across the fields is included in the energy equations. The system of equations appears to be complete. The constitutive relations between the 3 fields for interfacial drag, heat transfer and phase change, again involve many correlations and dimensionless numbers.

* *

24. I believe the comments that I had made regarding the complexity of the constitutive relations for the PM-ALPHA code also apply here, and the possibility of checking the synergisms between the momentum and heat transfer processes through separate-effect experiments, should be explored. New data may have to be obtained and some prioritisation should be performed.

Actually, in this respect, ESPROSE.m is much less of a challenge than PH-ALPHA. All we have is wave dynamics, which were verified very carefully in a step-by-step approach, and Microinteractions, which is really obtained experimentally under conditions that fully simulate large-scale explosions. So, we really do not see the concern expressed.

25. The fuel fragmentation is treated as in the PM-ALPHA code and is controlled parameterically through β. There is another parameter which enhances the fragmentation for thermal effects. Both of these parameters are user-specified. The entrainment of liquid in the m fluid is controlled through the parameter E, which is taken as a function of the fragmentation rate.

Actually not. The β controlls breakup in PM-ALPHA. In ESPROSE.m, we have constitute law for Microinteractions — see Appendix C, and the new addendum to this appendix.

26. I believe, the parametric treatment is very intuitive, and the authors admit that it is an important component of the micro-interactions concept with somewhat speculative constitutive laws. Since, the m fluid interactions are the basis of ESPROSE-m, I hope that the authors have already obtained additional data from the SIGMA facility to provide greater support for the experimental basis of the parametric treatment.

As noted above, there seems to be a confusion of β, which indeed is varied parametrically in quantifying premixing by PM-ALPHA, and with the Microinteractions laws in ESPROSE.m. These are **not** in a parametric treatment; rather, they are fixed by the SIGMA experiment.

27. III.2 Appendix C. Constitutive Laws of Micro-interactions

This appendix describes the experiments performed in the SIGMA-2000 facility with gallium and molten tin, subjected to high pressure waves, in order to derive the constitutive laws for the micro-interactions, needed for the m fluid.

The experiments are described. They are really very difficult, but precise experiments. Some results are shown as movie, X-ray and SEM images for the change in pre-mixing volume, as n function of time.

The results of experiments are used to derive the values for bf, γ_t, and fe, the entrainment factor. For example, Fig. C-13 shows f_e = 7, 8 and 12 give best fits, respectively, for three isothermal Gallium tests i.e., G/204/45, G/68/45 and G/272/45. The more conservative value $f_e = 7$ is then used to determine the value of $\beta_f = 9$. The value of γ_t derived from Fig C-10, while keeping $f_e = 7$,

and $\beta_f = 9$. It appears that γ_t varies from 1.4 at 68 bar pressure to 4.2 at 204 bar pressure.

The above is a logical but highly empirical determination of 3 parameters from a small number of tests. Perhaps, more data has been obtained from SIGMA to confirm the choices made for these key parameters. Obviously, more data is needed from SIGMA or another shock tube. I believe, different materials should also be tested, in particular, melt drops of binary oxides. Their fragmentation behaviour maybe different, due to changes in properties they experience with a change in temperature.

For new data with iron melts see the addendum to Appendix C. As noted already, data with ZrO_2, and if needed, UO_2/ZrO_2, will be obtained in SIGMA-3000, currently nearing operation.

28. III.3 Appendix D. On the Existence of Thermal Detonations

This is a very interesting re-examination of the Board-Hall model for steam explosions. The micro-interaction model and the concept of the m fluid is employed to show that supercritical steam explosions can be obtained with lean mixtures in highly voided regions; conditions for which the Board-Hall model will predict only very weak explosions.

My understanding of the micro-interactions concept, introduced by the authors, is that they take place in the m fluid in a limited volume. I believe, this results from the observations made from the Gallium drop (also perhaps the tin drop) experiments conducted in the SIGMA facility. The previous concept was that the pressure wave will fragment a melt drop into fine droplets, which will mix with the whole coolant volume. The SIGMA experiments showed that this does not occur in the time frame of the pressure-wave-melt drop interaction. The heat transfer to the m fluid s coolant, in the limited volume occupied by the m-fluid, generates very high pressures. The shock wave then travels into the non-participating fluid around the m-fluid, increasing its pressure to sustain the propagation. This makes possible the supercritical steam explosion with a fuel-coolant mixture, which is lean on an overall-volume basis, but is not so lean on the m fluid volume basis. (C_f. Figures D-8 and D-9, where high pressures are obtained for the coolant to debris mass ratio $f_e = 1$ in the case of tin at 1500°C and for $f_e = 2$ to 8 for the case of UO_2 at 3300°C)

I believe, the authors have provided a very logical explanation and frame work. I am, however, a bit concerned about the value for f_e, which was chosen as 7 in Appendix C, based on the data from the gallium experiments in the SIGMA facility. In Fig D-8, a value of $f_e = 7$ will not produce a supercritical steam explosion. Thus, the value of f_e may be material dependent, and more information is needed to choose an appropriate value.

See response above.

29. III.4 VERIFICATION STUDIES

The front part of the ESPROSE. m report describes the analytical tests, the SIGMA experiments, explosion coupling, integral aspects, numerical aspects, and finally, a comparison with the KROTOS tests. I will comment on these, briefly, individually.

III.4.1. Analytical Tests

These are very valuable exercises and show that the modeling in ESPROSE-m can predict pressure wave propagation. There are many figures. I wish there were more explanations e.g., Figures 17 and 18, both show very good comparison between the analytical and the ESPROS-m pressure distributions for early times, but deviate at later times. Is there an explanation? Similarly, there is a crater in the middle of the pressure wave in Fig 19. Is there a physical explanation for that? This section may be improved by the authors, through some explanatory text. It is very valuable, otherwise.

We do not see any significant change in agreement between early and later times in Figures 17 and 18. The crater in Figure 19 is because this is a Type B source, i.e., insufficient supply of fluid after t_o to maintain high pressures at the source location. This was explained.

30 III.4.2 <u>SIGMA Experiments</u>

These experiments, specially conducted in the SIGMA shock tube provide data for verification of the ESPROSE-m models for pressure wave propagation. The comparisons are excellent. There are some differences for the inhomogeneous cases, which, perhaps, are difficult to fix. All in all, it is a splendid performance for the code for these separate-effect tests.

* *

31. III.4.3 Comparisons with KROTOS Experiments

KROTOS experiments provide the most appropriate data for the verification of the ESPROSE-m models. The KROTOS facility has performed steam explosion experiments by triggering the pre-mixtures of water with several different material melts. The initial conditions, e.g. melt mass, melt temperature, melt superheat, pressure, water subcooling have been varied to provide a reasonably extensive data base. The test program is continuing, and could provide the data base needed for the ESPROSE-m validation. Unfortunately, as in most of these melt-water interaction integral experiments, the data obtained is integral and the premixing and the steam explosion processes are not delineated. Thus, detailed verification and validation of the ESPROSE-m (or any other steam explosion code) may not be possible.

The document provided on the analysis of the KROTOS tests speculates that the melt breakup and quick freezing may be a reasonable explanation for the non-explosivity of the Uranium oxide tests. We reached similar conclusions, and also, evaluated the effects of the change in the surface tension and viscosity of the binary-oxide melt, as it cools down below the liquidus temperature. This has been reported in the 1995 ICONE meeting, and additional work will be reported in the forthcoming FCI meeting.

* *

32. Coming back to the comparisons of ESPROSE-m (using PM-ALPHA premixtures) predictions against the measured data, the authors admit difficulties of representing particle freezing correctly in the PM-ALPHA formulation. The fuel-participation factor chosen affects the result greatly. The pressure wave-shapes versus time appear to be reasonable but there are differences e.g. for K5 there appears to be an earlier venting of pressure wave. I believe, revision of the PM-ALPHA numerical scheme and/or modeling of the heat conduction in the fuel particles (as was mentioned earlier in the comments on PM-ALPHA modeling) may resolve this difficulty. The sensitivity to fuel participation factor is very large, indeed.

Disagree. The key ingredient here is the rate of breakup, which is not known, and will remain so. Also, as we discussed, there are intricate radiation reflection issues peculiar to the KROTOS geometry. These are not code issues; rather, they are physics issues peculiar to the test. The sensitivity to molten fuel content is real, not a code artifact. Again, this should provide important

perspectives as to what is really predictable for these kinds of problems, and correspondingly what should be a viable strategy in safety assessments.

33. III.4.4 Numerical Aspects

I have similar comments as I had for this topic in the PM-ALPHA document. The authors should provide more discussion and, perhaps, comparisons of the use of the ICE technique for similar problems. The numerical diffusion issue is quite important when, tracking pressure waves and/or interfaces. Recently, special numerical schemes have been devised to reduce or eliminate numerical diffusion. The ICE technique does not, compare well to such schemes, in term of its performance, and with respect to numerical diffusion. Perhaps, the authors have implemented another scheme in developing the ESPROSE-m-3D code.

Disagree. This comment is not consistent with the code performance presented in this document.

34. IV. Review of the Report: Lower Head Integrity Under In-Vessel Steam Explosion, DOE/ID-10541 by TG Theofanous, W.W. Yuen, S. Angelini, J.J. Sienicki, K. Freeman, X. Chen and T. Salmassi

This report is concerned with answering the question: "Will the lower head of the advanced passive reactor AP-600 fail, under the dynamic loading imposed by an in-vessel steam explosion, if it were to occur?" This is an important issue for the accident management strategy chosen for the AP-600, i.e. retention of the core melt in the lower head, by employing external cooling of the vessel.

The methodology used to resolve this issue is the ROAAM method developed by Prof. Theofanous, employed most recently to respond to the companion question "Is it possible to retain the molten core of the AP-600 reactor, in the lower head by cooling the vessel externally?" This question was answered in the affirmative by employing the ROAAM method. The ROAAM method has been extended and further clarified by Prof. Theofanous in a recent publication, attached as Appendix A in this report.

Besides the ROAAM philosophy and procedures described in Appendix A, the detailed premixing and explosion results are described in Appendices B and C respectively. Appendix D provides additional pre-mixing perspectives from the THIRMAL code, prepared by Drs. Chu and Sienicki of Argonne National Laboratory. The important chapters, in the main body of the report, are concerned

with structural failure criteria, melt relocation characteristics, quantification of pre-mixtures and explosion loads and finally the assessment of the integrity of the lower head of AP-600.

In the following paragraphs, I have provided comments on the appendices, chapters and conclusions of the report in the order: - Chapter 3: Structural failure criteria

- *Chapter 4: Melt relocation characteristics*
- *Chapter 5: Quantification of pre-mixtures*
- *Appendix B: Detailed pre-mixing results*
- *Appendix D: Additional pre-mixing perspectives from the THIRMAL code*
- *Chapter 6: Quantification of explosion loads*
- *Appendix C: Detailed explosion results*
- *Chapter 8: Consideration of reflood FCIs*
- *Chapters 7 and 9: Integration, assessment and conclusions*

IV.1 Chapter 3. Structural Failure Criteria

This is an important chapter, since it establishes the fragility curve, giving the probability of the lower head failure for dynamic-loads of increasing magnitudes. The impulse loading, of interest, is in the range of 100 to 300 kilo Pascal-seconds.

The authors have employed a commercial structural-analysis code, whose results they have compared with a simple analytical solution. ABAQUS is a 3-D finite element code, able to model the hemispherical lower head and the dynamic loadings imposed. The code provides the strain as a function of time for the assumed loading. These calculated results are, then, converted to a fragility curve, assuming probabilities of lower head failure, when strains of greater than 11 % are reached over certain fractions of the lower head wall thickness.

The ABAQUS calculations are performed for various loading patterns on the lower head. The non-uniformity of loading was found to decrease the strain for a specific impulse. The colour pictures provide very nice strain morphologies.

This chapter provides clear and transparent results. The ABAQUS results are confirmed against a simple model for uniform loading. The fragility curve makes good sense.

* *

35. I am a bit concerned about the very local non-homogeneous loadings of the type predicted, later, in the report. Perhaps, a few ABAQUS calculations could be performed to establish the fragility curve for such a local-loading pattern.

We do not understand this comment. Most if not all of the development in this chapter is for "local non-homogeneous loadings of the type predicted later in the report." See also addendum to Chapter 3.

36. IV.2 Chapter 4. Melt Relocation Characteristics

This chapter provides the initial conditions for the scenario of melt-water interaction in the lower head. The chapter, therefore, deals with the melt pool formation in the original core boundaries and, later, relocation of the melt from the in-core location of the lower head. The quantities needed are the rate of melt addition to the water in the lower head, the jet geometry (diameter, velocity and location in the vessel), the melt composition and superheat and, finally, the timing of this event relative to the other events in the core melt-progression process.

The authors, first point out the differences in the AP-600 core configuration from that of the conventional PWR. The AP-600 has some features which are quite favourable in terms of the melt releases conditions. These are the massive 36 cm thick core support plate, the core reflector, the gap between the core barrel and the reflector on the flat sides of the reflector; and the long unheated section in the fuel elements at the bottom.

The authors have developed a credible scenario of melt pool formation, melt attack on the reflector and the core barrel. It is supported by enveloping models of appropriate complexity, which provide physical insight and transparency. The authors are wise not to use one of the myriad codes, which provide user-motivated results. The analysis is brilliant and quite comprehensive. The melt release conditions of 200 to 400 kg/sec should be bounding values. The melt superheat of 180 K also should be a good bounding value. The location of the release, near the top of the core in the vessel downcomer, may also be credible. The jet

velocity of few meters/second also appears to be sound. I, however, would like the authors to consider the following cautionary points:

* *

37. (i) The timing of the melt release 76 to 91 minutes is much too close to the timing of 100 minutes for evaporation of water in the bottom 25 % of the core height by the radiative heat flux imposed.

In addition, there is a huge margin in the heat capacity of the massive core support plate. See also addendum to Chapter 4.

38. (ii) The core plate is massive but it is also loaded heavily. If the core plate temperatures go beyond 700° C, the yield strength will deteriorate.

The core plate is fully supported by the core support structure. Also note (addendum to Chapter 4) that it really takes a long time for the lower part to heat up.

39. (iii) The melt pool with ≈40 to 60 % unoxidized zirconium and some stainless steel, will probably form a primarily metal layer on the top. This layer is thin and will focus the heat flux to the sides. Recent work at RIT has evaluated the heat transfer from the metal layer to the vessel (which is of a thickness similar to that of the reflector) with a two-dimensional code, and found that the highest heat flux is still at the corner of the oxide pool just below the metallic layer. Thus, the failure could be below the metal layer.

We do not agree with such a result, but we need to look at the RIT analysis mentioned.

40. (iv) While, I agree with the authors that the flat part of the reflector being closest to the core centre is most likely to be attacked first by the pool. The oxide pool however may not be axially symmetric and there may be azimuthal regions in the core, where fresh fuel and high power are dominant. Evaluation of a possible attack on the non-flat parts of the reflector should be considered.

Failure at the corner would produce a more localized release. Failure on the flat is conservative.

41. (v) The draining and freezing of the metallic layer into the well between the flat part of the reflector and the core barrel, without participation in any melt-water interaction, is very likely, but sounds too convenient. Additionally, in the absence of water above the core plate in the well, the thermal loading imposed

by the superheated metallic melt on the core plate, or on the core barrel region directly above the core plate should be evaluated.

There is no water between the core barrel and the reflector at this time in such an accident. As we have shown, the core plate still would be cooled by water.

42. Summarizing, I believe, the authors estimates for the range of melt-release-characteristics is y credible, however, additional evaluations may help to put these estimates on a more solid footing.

* *

43. IV.3 Chapter 5. Quantification of Pre-mixtures / Appendix B. Detailed Pre-mixing Results

The chapter 5 develops the rationale for the pre-mixing that results from the release of the $U0_2$ - ZrO_2 melt from near the top of the core, through the down-comer, into the water pool of the lower head. The water level is assumed to be a few centimetres above the top of the core support plate. Melt release rates of 200 and 400 kg/sec, reaching the velocity of 5 m/sec at entry into water are considered. The melt superheat is assumed as 180K.

The oxide melt jet is distributed over an effective radial width of 10 cm in the downcomer, with an initial melt volume fraction of ≈25 % at water impact. This would translate to a melt stream of dimensions ≈10 cm x 16 cm for the release rate of 200 kg/sec and ≈10 cm x 32 cm for the release rate of 400 kg/sec.

The expanded melt jet is then allowed to traverse 20 mm in water, before break-up ensues. The break-up rates are parameterized from no break up to very rapid break-up (forming 2 mm size particles within 10 cm of travel in water.)

The above initial conditions were employed in the PM-ALPHA code to provide results on pre-mixture characteristics i.e. the melt and the void volume fractions and the fuel length scale, as a function of time, and position. The integral quantity of interest is the number of kilogram of melt mixed with coolant, before the triggering and explosion.

The Appendix B presents a number of colour pictures and many graphs giving detailed results. The graphs of fuel length scale, fuel volume and void fractions are presented for more β values and for times up to ≈ 1 sec. These pictures and

graphs provide good back-up for the results, and arguments, presented in chapter 5.

I believe the authors have presented a clear method of evaluation and the results are credible. I do have the following comments.

* *

44. (i) The melt-through failure of the reflector and core barrel are assumed to be near the top of the melt pool in the original core boundary. If the failure is lower, the starting velocity for the melt jet would be higher, and so will be the velocity at water impact. This may be beneficial for break-up.

We do not expect much difference. Twice the initial depth would increase initial velocity from 1 to 1.4 m/s, and the velocity of water impact remains essentially unchanged at 5 m/s.

45. (ii) The initial impact area on the water surface is quite large. The jet going through the 2 meter steam region should not break up, to that extent.

There is also a splash off the wall. More concentrated pours create higher voiding in the premixture, so we try to bound the behavior here, too.

46. (iii) Both the very fast and the no-break-up cases show (Cf. Figs. 5.4 (a) and 5.4 (b)) that for the initial 0.1 sec the fuel front is more advanced than the void fraction front. This was also observed in the PM-ALPHA verification report. Later on, the void fraction front seems to catch up with the fuel front. For the C 1-10 case at 0.4 seconds (Page B.3-3) a large fraction of fuel seems to be hung up in the voided zone. The same is true for C 1 - nb case (Page B.3-5). In the C1-10 case, there would be a large steam flux rising, which could retard the descent of the fuel particles. For the C 1 - nb case the steam flux should he smaller, and the fuel particles of 2 cm should be ahead of the void fraction front.

* *

47. Summarising, I believe the break-up assumptions, both, in the steam during descent from the original core boundary, and during water interaction, play a crucial role and, perhaps, this part of the pre-mixing analysis could be strengthened. The no-break-up case appears to produce approximately the same results as the high break-up case. This has been recognised, also, by the authors (Page 5-10). Perhaps a physical explanation of why these cases produce such similar results may be provided by the authors.

The results show that all cases become highly voided. This result is really expected, given the radiative power of such melts. See also addendum to Chapter 5.

48. IV.4 Appendix D. Additional Premixing Perspectives from the THIRMAL Code

In this appendix, the THIRMAL code has been used by C.C. Chu and J.J. Sienicke of Argonne National Laboratory to provide a perspective on premixing. The code had to be modified to describe the melt jet-water interaction in the confined geometry of the down comer. The calculations were performed for melt release rates of 14 to 220 kg/sec, with corresponding jet diameters of 18 mm to 73 mm. The 220 kg/sec case resulted in median droplet size of 2.75 mm, with a mixing zone radius and void fraction at pool surface of 160 mm and 74 %, respectively.

These results are not too different from what were obtained from the PM-ALPHA Code, although the jet entry conditions are different. THIRMAL calculates jet entry diameter of 6 cm (i.e., no break-up in the down comer steam zone). Models for break-up in THIRMAL must be quite different from the parametric model employed in the PM-ALPHA Code.

The melt exists the pool horizontally, hits the vessel wall, and then falls into the water. The initial jet used in THIRMAL calculations ignored this sequence of events.

49. IV.5 Chapter 6. Quantification of Explosion Loads / Appendix C. Detailed Explosion Results

The chapter 6 and Appendix C present the results of explosion-propagation calculations performed with the ESPROSE-m code, using. as initial conditions. the pre-mixture configurations calculated with the PM-ALPHA code. The trigger time is chosen as very short. since during the early time the void fractions of the coolant around the fuel particles are relatively low. Later. the void fractions increase substantially. and would inhibit fuel break up and triggerability.

The results are presented for the C-1 and C-2 scenarios with three values of β and a set of trigger times. For the no break-up case these times vary - from 0.05 sec to 1.0 sec, while for the break-up cases, they vary from 0.04 to up to 0.19 seconds.

The results on pressure, impulse and effective area are shown for various locations in the lower head. Peak loadings histories are also shown as a function of trigger

times. The extreme sensitivity to trigger time is evident from Table 6.1. If the trigger is delayed by 0.06 seconds for the C1-10 and C2-10 cases, there is only a very weak explosion. For the Cl -20 and C2-20 cases, there appears to be a time interval of only 15-30 msec for the trigger to generate a supercritical explosion. Thus, triggering time appears to be the deciding factor. A physical explanation for this extreme sensitivity should be provided by the authors.

See addenda to Chapters 5 and 6.

50. The Appendix C gives very nice pictures of the pressure wave traversing through the lower head. The pressure signals at various points in the lower head are shown and the peak pressures and impulse loadings are shown as graphs versus time. Theses pictures and graphs were very helpful in the review of Chapter 6.

Summarising, I can say that the authors have performed logical analyses of the loadings imposed by the steam explosion, and have provided very nice results. I have not understood the reasons for the extreme sensitivity of the calculated results to the trigger time. The peak loadings shown in Table 6.1 are, in general, modest. The highest loading is found to be ≈200 k. Pa. s. Is it possible that for $\beta = 30$, *a higher value than 200 k. Pa. s. is calculated?*

The sensitivity issue was answered in the addenda to Chapters 5 and 6.

51. IV.6 Chapter 8. Consideration of the reflood FCI's

This chapter deals briefly with the stratified steam explosions that may result, if the reflood is effective, and a layer of water is brought on top of the melt pool, which has a metallic layer, at top.

It was found that the stable water layer may not exceed 10 cms, due to the low reflood rate and the time to freeze the upper metal layer. Any stratified explosion will be easily vented.

I believe, the authors have a good argument. Certainly the peak pressures in such an explosion should be low and reflood FCI s may not be a problem.

IV.7 Chapter 7. Integration, Assessment / Chapter 9. Conclusions

These chapters combine the results achieved in the previous chapters and appendices to provide an overall assessment. This work was already practically done

by the results achieved, since the maximum impulse loading was below the minimum of the fragility curve. This was also confirmed by performing ABAQUS calculations for the peak loading for the actual cases and finding that the lower head strains were very low.

The authors conclude that for the saturated water case, the lower head integrity can not be compromised by a steam explosion. Having highly subcooled water is the only possible way to, potentially, involve a larger mass of melt, and produce a more energetic explosion. The authors conclude that obtaining highly-subcooled water, even in reflood scenarios for the AP-600 is not credible.

* *

52. V. Concluding Remarks

In this section, I would like to provide a few concluding remarks after the review of the three reports.

I must congratulate the authors for producing such a fine and comprehensive body of work treating the tricky and controversial area of steam explosions. While, most of the researchers in this area are still trying to understand the fundamentals, the authors have leaped ahead with new concepts, advanced codes and considered judgements to provide a reasonably robust estimation of the damage potential of a steam explosion. They have combined this with structural analysis to show that AP-600 lower head can withstand the dynamic loads imposed.

The authors have, also, noted the peculiarities of the AP-600 configuration and employed the advantages and disadvantages they confer on the analyses. Some of these peculiarities (differences) provide great advantages e.g. in the core melt progression and the melt release characteristics. These sound a little bit too convenient and, perhaps, should be re-visited.

The work was done without any regard to the "convenience" of the results.

53. The authors have modelled the fuel break-up and fragmentation process only parametrically. This may be a weak point in the whole development; since those processes provide the initial conditions for both the pre-mixing and the propagation phases of the steam explosion. Perhaps, the analyses are well-bounded for these processes; however, the sensitivity of the results to the break-up and the fragmentation modeling is very large.

We do not agree with the thrust of this conclusion. Only breakup is treated parametrically, not fragmentation. The main result is that premixtures void, and this is not too sensitive on the breakup used. We bound the behavior with respect to this parameter, and this is much more reliable than trying to assert the result of some predictive model. Such models, even if eventually developed, could never be verified at the appropriate level — i.e, the dynamics of breakup as it occurs under realistic conditions. Our treatment of fragmentation, on the other hand, derives directly from the directly applicable and well-characterized SIGMA experiments.

> *54. Then, there is the question of maturity and of validation versus verification. I believe the methodology and the data presented, robust as they are, are still very new. The comparisons presented against test data are not extensive, and I think, the authors recognising this, have wisely titled the reports as verification reports. Further experience with this methodology and further comparisons with separate-effect (e.g. SIGMA, MAGICO, BILLEAU and QUEOS) data and integral-effect (e.g. FARO and KROTOS) data would provide validation and maturity to this methodology. In particular, the constitutive relations, being so many for such complicated phenomena, need greater experimental back-up. I believe, the authors are already busy in achieving such experiments in the MAGICO and SIGMA facilities.*

Actually, we did not intend to make a distinction between "verification" and "validation." In the sense that the validation term is described here, we believe the two codes have been adequately validated in the "fitness for purpose" sense. Of course, work will continue, and thus maturation will gradually develop.

> *55. Lastly, I must say that I have enjoyed reading the reports and learned much from them. I think, I now understand the concept of micro-interactions and the m fluid. I have made constructive (hopefully) critical comments at places, to provide input to authors towards improvement of the reports. I believe, they have largely achieved the objective they had set out to achieve.*

* *

F.13. Response to P. Shewmon (OSU)

General Comment and Highlights

General and unqualified agreement with the conclusions of the work under review.

Point-by-Point Responses

1. The analysis of head failure sets up a model of the lower head using ABAQUS (a well established finite element code) to relate the stress pulse from steam explosion to local strain. The vessel material (ferritic SA508 steel) will undergo large amounts of strain (elongations of 50 to 100%) before fracture occurs. Whether or not the vessel undergoes any plastic strain depends on the yield stress of the metal and the impulse from the steam.

* *

2. For reason never explained or discussed, the authors chose 330 MPa for the yield stress of the vessel. They state that the conservative 'Code Allowable' is 345 MPa and the actual value (found in a conventional tensile test) is 450 MPa. The choice of 330 MPa introduces a large conservatism (safety margin) since a best estimate should use 450 MPa.

The real reason for using 330 rather than the actual 450 MPa value for yield stress is that we could not find the actual value until much of the work had been done with 330 MPa. The additional margin due to this is now discussed in the addendum to Chapter 3.

3. The impulse applied to the steel in the lower head would have a rise time of a few milliseconds. When ferritic steels are loaded this quickly their yield stress is substantially greater than that observed in a normal tensile test. The authors quote references that show the yield stress at this strain rate is about 40% greater than that found in a tensile test. They take full credit for this strain-rate increment, which is justified and appropriate.

In summary, the analysis of head failure seems to be competently and conservatively done, and the conclusions drawn are appropriate. I have also looked at the discussion of loads and loading. I am less of a specialist in this area, but it also seems to be well done.

* *

4. Though no mention of radiation effects is made in the reports, the analysis should be made for the vessel at end of life (40 years?). The temperature of the head during the accident considered would be less than 212 F. This is beneath the RNDT for the beltline of some of the vessels now in service, i.e. such material might well behave in a brittle manner during an accident of the type considered here. I considered this, but feel such radiation effects are not germane in the case of the AP600 for at least two reasons:

1) The fast neutron and hard gamma flux in the lower head will be at least a couple of orders of magnitude less than that in the beltline region of the vessel, so radiation effects should be negligible.

2) The steel to be used in the AP600 vessel should be appreciably lower in the elements than have lead to radiation embrittlement (copper, and phosphorous) in the older vessels now of concern in plants in the U.S.A.

With this in mind, I believe there is every reason to believe that the material in the lower head would behave in a ductile manner and that the analysis given in the report is appropriate for (would apply to) a vessel in the AP600 after 40 years of service.

The end-of-life RTNDT values for AP600 steel forging at the beltline region is specified as 23 °F. The lower head, less irradiated, would be better still. At the time of interest, the lower head would be between 50 and 100 °C.

F.14. Response to B. Turland (AEA)

General Comment and Highlights

This review is hard to interpret at this stage. Concerns are raised about almost every aspect of the analysis and supporting documentation, yet we also obtain the impression that these are offered in the spirit of further improving the basis for the conclusions rather than in challenging them. The key point appears to be the one made in closing the first section of the review (Overall Comments), that is, "my residual concerns relate to the confidence in having low pour rates and the possibility of operator actions leading to some subcooling". In this response, as well as responses to several other related questions by other reviewers, we present additional material that hopefully will be found helpful in coming to a resolution, or if not, to better focusing any remaining concerns.

Point-by-Point Responses

1. OVERALL COMMENTS

This report and its associated documents represent the culmination of several years work by Prof. Theofanous and his colleagues. They have now demonstrated that the basic framework for a steam explosion assessment in realistic geometry is in place. This is a major achievement.

The reliance on detailed modelling codes makes the reviewer's task difficult - in the end one can look at the validation offered and consider whether the results presented look reasonable. In the supporting documents the authors make good use of the available experimental data to benchmark their calculational models. However, it is accepted that some of the constitutive physics used in the premixing and propagation codes is uncertain, as are, to some extent, the melt pour characteristics. A review, such as this, can indicate that the codes appear 'fit for purpose' but cannot give a full endorsement for all the models they contain, without significantly greater effort.

The situation considered in the application presented, a modest pour of melt into saturated water at ambient pressure, is not conducive to large steam explosion loadings, and this is demonstrated by the calculations presented. Sufficient parameter variations are investigated to indicate that this is likely to be a robust result for these conditions. As indicated below, my residual concerns relate to the confidence in having low pour rates and the possibility of operator actions leading to some subcooling.

* *

2. *SPECIFIC COMMENTS*

Chapter 2: Problem Definition and Overall Approach

1. Although the text makes clear that it was an intentional conservatism not to claim credit for lower head venting in the Sizewell B study, it is wrong to interpret this in the phrase lower head failure cannot be dismissed as readily any longer. We found that the previous claims for lower head failure could not be substantiated as large explosions did not necessarily imply sustained high pressures.

* *

3. 2. It is arguable whether the 'essential basis for the current work' is the progress made in modelling explosion propagation and the pre-mixing phase, or in the assessment of melt progression.

Look at the pressure pulses predicted, the fragility, some of the reviewer's comments, including this one, and then let us imagine where we would be **without** such progress in premixing and propagation (microinteractions in particular).

4. 3. The statement that between 3 to 5 tons of fuel must participate to produce a 1 GJ explosion, and consequently incipient lower head failure, raises the question of whether larger explosions are possible that do not fail the lower head.

This is an old result. Probably yes, but this is not our concern here.

5. 4. I agree that the size of any breach is indeed a tough question. I consider it to be the key question unless the mixing/propagation analysis by itself can be shown to be sufficient. I do not believe that this has been shown to be sufficient (as yet?).

See specifics below

6. 5. The higher explosivity of a premixture with reduced voiding appears to be a conjecture that is not fully supported by the experimental evidence. Reported explosions in the JAERI ALPHA facility occurred with large voids in the mixing region.

Explosivity refers to intensity, not likelihood. In any case, there will be peripheral zones of low void fractions and nothing prevents these regions from initiating and propagating explosions. The voided regions simply damp the energetics, and reduce the amounts of fuel exploding coherently.

7. *6. I need to be convinced that we need only to be worried about the first relocation event. I think that it depends on the timing of any subsequent relocation events.*

Once the path opens, subsequent relocations should be essentially continuous.

8. *Chapter 3: Structural Failure Criteria*

1. In principle, the loading may have components both shorter and longer than the natural timescale of the vessel. Indeed the constrained expansions considered in the early studies do have a longer timescale. One needs to refer ahead to the results of the propagation modelling to justify the assumption through early venting of the explosion region.

A big part of the argument is due to the voided and highly localized nature of the premixtures.

9. *2. The boundary conditions on the ABAQUS model are not specified - from later examples they appear to be symmetry conditions at the equator of a sphere. As potential explosions may occur near the join of the lower head to the cylindrical section, it is not clear that this provides a good choice (apart from validating the simpler model - which could have been done in 1-D).*

The explosion loading occurs well below the hemispheric/cylinder juncture, so our choice is reasonable, and an economical approach to develop the necessary understanding for localized loads. In Chapter 7, we show full-vessel simulations also.

10. *3. From a non-expert viewpoint, the analysis presented in this Chapter appears a reasonable approach. However, I did note that Figure 3.9 was not consistent with Figure 3.4. To support the mitigative factor for local loading, more highly localised ABAQUS calculations should have been performed. To avoid ϵ_{3D} falling to zero for finite values of I and d_o/D_s. I suggest assuming that energy dissipation is proportional to the magnitude of the effective impulse.*

Valid point. More calculations were carried out. There was a problem with plotting figure 3.9. In the corrected figure the ϵ_{3A} goes to zero properly. See Figure 3.9 attached. The suggested idea gave about the same quality of representing the calculational results. The new, more localized ABAQUS results support well the generalization in Figure 3.9 (see addendum to Chapter 3).

11. *Chapter 4: Quantification of Melt Relocation Characteristics*

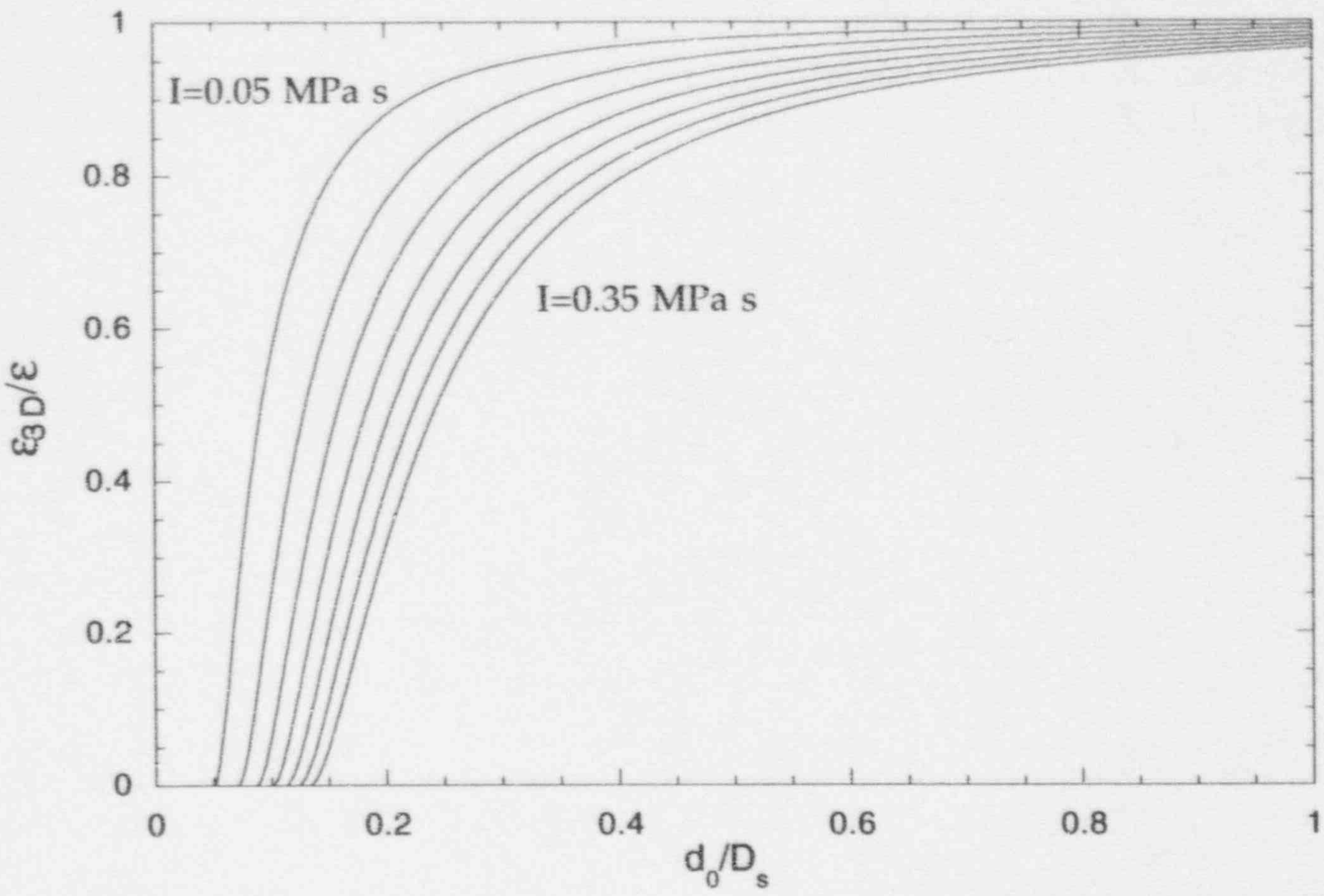
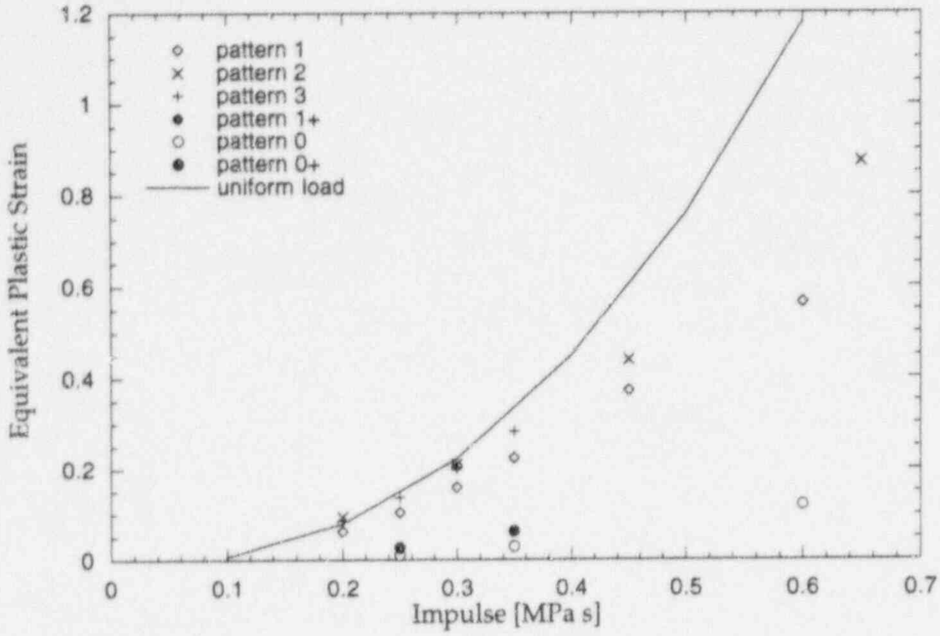
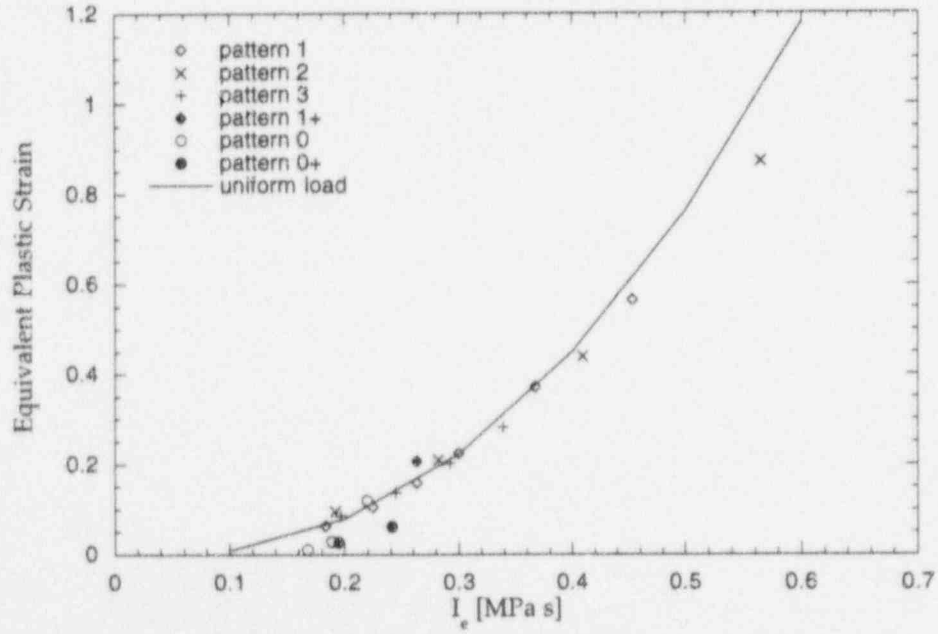


fig 39



pattern 1
pattern 2
pattern 3
pattern 1+
pattern 0
pattern 0+
uniform load
Equivalent Plastic Strain
Impulse [MPa s]

Fig 3.4



pattern 1
pattern 2
pattern 3
pattern 1+
pattern 0
pattern 0+
uniform load
Equivalent Plastic Strain
I_e [MPa s]

Fig 3.8

1. it would be useful to give an indication of the diameter of the cooling holes.

They are ~1 cm in diameter.

12. 2. I do not see that the heat sink associated with the core support plate plays a significant role, as water provides the major heat sink. In the absence of water, melt passage through the plate would depend on the diameter of the flow channels. Melt appears to have passed through relativelt small diameter holes in the presence of water at TMI-2. If these holes are similar to that of the hole in a PWR lower core plate, then they probably offer little resistance to melt flow. The lower core plate would prevent large diameter pours penetrating the lower plenum, if downward relocation were to occur.

No. The heat sink is important in delaying the blockage, above, from melting, after the water has vaporized, to a level **below** the core support plate. Yes. The holes in the plate itself would offer no resistance to melt penetration.

13. 3. At this stage (page 4.1) translating We expect this path to be blocked into 'Downward relocation is physically unreasonable' still appears a large step.

Yes, but the statement gives a preamble of where we are going in this chapter.

14. 4. Reference to TMI-2. Looking again at the TMI-2 melt relocation event, I am struck by how far melt managed to progress downwards through the core, given the water inventory that is generally believed to have been present (minimum of 0.5 m above the base of the core). For instance at position K9 near the centre of the core there was evidence of previously molten material between rods near the first spacer grid and in the spaces around the lower end fitting [TMI-2 Core Bore Acquisition Summary Report, EGG-TMI-7385, rev 1, February 1987, page B-30]. This relocation was physically reasonable, as it occurred, but I do not see how it differs substantially from the claim that the APR-like core downward relocation is 'physically unreasonable.' I am happy with the notion that relocation into the bypass (most PWRs) or downcomer in the APR-600 is most likely - it is the degree of certainty that I question.

We really mean "physically unreasonable" to penetrate through the bottom of the core, and the TMI information noted does not conflict with this; indeed, it supports it. In the present case we have also the Zr end-plugs as a further cold trap.

15. 5. The low melting point control rod materials are expected to escape early (page 4-4). This seems counter to other arguments about heat sinks. However, if they do form a blockage, this may be relatively weak, giving the potential for a later downward relocation.

Yes, but these are intermediate states in melt progression. We are interested in how far this can go.

16. 6. While the results on blockage formation appear realistic, the thermal equilibrium assumption in equation 4.1 is inconsistent with the growth of thermal boundary layers in the solid represented by equation 4.2. This may lead to an underprediction of the plugging time, particularly for cooler structures.

The error is negligible in the context and timing of this freezing.

17. 7. Table 4-1 - what is the meaning and significance of Melt freezing capacity as multiple of the fuel rod volume? For comparison (I think) one needs the channel volume divided by the fuel rod volume to ensure that there is sufficient heat capacity to form a blockage.

We think ours is an interesting measure because it includes thermal effects.

18. 8. Page 4-6: The effective thermal conductivity is taken as the volume-weighted average. Here and elsewhere it would be useful to indicate what physical properties were used. The use of a volume weighted average is probably reasonable for this application (but not generally so). Was any allowance made for the porosity of the blockage in this evaluation.

Yes. See addendum to Chapter 4.

19. 9. Page 4-7. While low melting point components of the core such as control rods are expected to relocate as they melt, this does not apply to the major metallic component - Zr. Best fits to experimental data indicate that relocation following clad breach occurs at temperatures in the range 2400 - 2450 K. Relocation involves a significant fuel component.

This is still well below the oxides melting temperature, and the fuel content is limited by the time available for melting and dissolution.

20. 10. Nomenclature: Equations 4.8 and 4.9 refer to $C_{r,w1}$ while Figure 4.6 and 4.7 have $C_{rad,w,1}$ etc.

Typo corrected.

21. 11. The radial heat up calculations (Section 4.2) are qualitatively in line with similar calculations that we performed for Sizewell. Was a radial power deposition shape factor used? We found that somewhat different results were obtained when we did the same calculation using a 2-D, rather than cylindrically symmetric model, that took account of the proper core geometry and the power rating of individual assemblies. A difficulty with both your and our model is the absence of relocation, which may invalidate the model once any melting of material occurs.

Yes. Radial power factors were used as shown in the report. Clearly we do not expect to predict the details of relocation with this model, but this is not our purpose nor is it needed. Once relocation begins overall energy conservation is sufficient to take the relatively smaller way up to the melt pool formation.

22. 12. The assumption of a fully oxidised pool (Section 4.3) may be inappropriate for a low pressure sequence. This raises issues on the interactions of the corium with a more metallic blockage (partially addressed in the MP tests). However, to conclude that something is 'physically unreasonable' all processes that may have an impact should be discussed.

The point is that the cold trap at the bottom forms and sustains the blockage. Any interactions with the oxides on the top of it can do nothing to violate its integrity. All we need is the heat flux from above, as we have done.

23. 13. The proposed melt release conditions and mechanism appear reasonable. The dimensions and the pour rate are no more than educated guesses (I would probably have made similar guesses). I note that to achieve the melt flow rate of 1m/s, only a 5 cm driving head is assumed, although there is no quantification of how close to the top of the pool the breach might occur. It is desirable to analyse whether heat transfer from the melt stream through the breach may deepen the breach and lead to an increase in pour rate.

Certainly will, but not significantly in a matter of ~1 second. It is the time coherence here that determines the reasonableness of the "educated guess" as a conservative bound.

24. Chapter 5: Quantification of Premixtures

1. Is there any likelihood for this plant of subcooled water in the lower head (eg in an extended accident sequence with some injection)?

This is addressed in Chapters 4 and 8.

25. *2. The comment that the break up parameter β set to 10 produces very rapid break up in $\sim$10 cm of water suggests that the modelling is somewhat more efficient at producing fragmentation than originally desired (break up in a specified fall distance taken as the smaller of the actual fall distance or $\beta.D_f$). This also depends greatly on the assigned value of D_f - here set to the initial particle size (20 mm). If the melt was assumed to fall as a thinning sheet (quite possible) then the initial penetration of the water may be more local than represented in the PM-ALPHA calculations. However, I am happy with the range chosen for β.*

The range of β chosen is intended to cover the range of breakup behavior. The more "localized" penetration as suggested by the review is covered.

26. *3. Please note that in Figure 5.2 and Appendix B the void is represented by shading, the fuel by contours. Explain the contours that follow the domain boundary.*

Please see extended calculations and new, improved representation in the addendum to Chapter 5.

27. *4. Specify the boundary conditions for the calculation. What pressurisation is predicted?*

Due to the large volume of the system, the localized nature of interactions, and short times, no pressurization is predicted. A constant pressure outlet boundary condition at the top of the downcomer was imposed.

28. *5. The length scale increase referred to on page 5-5 is not evident in Figure 5.4. The area averaged over is not clear, it is obviously not the whole cross-section. Since writing this I found the 1% fuel volume fraction limit on the region considered in the text - for clarity add to caption of Figure 5.3.*

Clarification made as requested.

29. *6. Middle of page 5-10: 'Only a very small fraction of the coolant is found to co-exist with the water' - I know what you mean! It is clear though that here we have the key result anticipated for the mixing codes. This implies that the*

key region to seek validation of the code is in the production of the high void fraction.

Yes.

30. 7. In my view the THIRMAL calculations raise as many questions as they answer, because of the poor validation status of any jet break up model. However, I do not think this is a key part of the argument.

* *

31. Chapter 6: Quantification of Explosion Loads

1. Where is the trigger cell?

At the bottom of the premixtures in each case.

32. 2. At what time was the effective area evaluated - that of peak pressure? If not, you obtain larger effective areas than ~0.1 m^2.

Yes, at the time of the main portion of the pressure pulse.

33. 3. The question raised by the calculations is how far is one from the danger zone? Could we get there by a modest increase in system pressure (what value was assumed?) and/or varying the value of β*?*

More calculations presented in the addendum and their interpretation should help this kind of question. We used 1 bar, and calculations done since, at 3 bar (see addendum), gave the same results. Further, inbetween β values were considered initially, before deciding the cases to be examined in detail. The results are understandable about how and where the high pressure pulses are produced, so it is unlikely that we missed something that could "unexpectedly bring us to the danger zone."

34. Chapter 7: Integration and Assessment

1. The conclusions reached are justified on the basis on the analysis presented. On the basis of current knowledge I am still not comfortable with the observation that downward relocation scenarios are 'physically unreasonable'.

See above responses and other reviewers' questions and our answers in this area.

35. 2. I agree that there is a greater threat from subcooled conditions. It is not obvious, though, that a 'highly subcooled pool' is necessary. Perhaps this

might be illustrated by a calculation with modest subcooling (eg 10 degrees) to show there is not threshold effect.

We now have such a calculation (see addendum). However, please note that the possibility of creating subcooled conditions has been addressed in Chapter 4. In this context, generating a highly subcooled condition is just as difficult as generating a 10 K subcooling.

36. Chapter 8: Consideration of Reflood FCIs

1. This chapter has not been considered in any detail. The arguments presented appear persuasive provided that are no other means of fast reflooding not considered by the authors and that crust formation proceeds in the way that they envisage.

⋆ ⋆

37. Chapter 9: Conclusions

1. I have indicated above that my principal reservations lie in the areas of the downward blockage and in ensuring that there are no operator actions that may prejudice the assumptions made in the analysis. I agree with the authors that consideration of additional pathways is unlikely to change the conclusion.

⋆ ⋆

38. 2. For this application, the supporting analysis ought to concentrate on the melt relocation scenario. This would include obtaining a better understanding of melt relocation in TMI-2 (eq why did it occur after reflooding the vessel?), to demonstrate that the processes are indeed understood.

On TMI-2, see Epstein and Fauske (1989).

39. 3. It would have been useful to have an indication of the effects of uncertainty in the constitutive laws (eg microinteractions) to determine where confirmatory studies are required.

Presently we have chosen very conservative parameters for microinteractions.

40. Comments on DOE/ID-10503: Propagation of Steam Explosions: ESPROSE.m Verification Studies

Only a limited time was available to review this supporting document.

Much of the document is concerned with the ability of the ESPROSE.m code to represent the wave dynamics correctly for single and two phase regions in one and two dimensions. The information presented, along with the comparisons with the SIGMA experiments with a voided expansion region, indicate that this part of the code is doing its job correctly, even when relatively coarse (~0.01 m) meshes are used. This does not surprise me. Numerical studies we performed when extending CULDESAC from one to two dimensions indicated good capabilities to capture the wave dynamics with relatively simple numerical schemes (the numerics of propagation are simpler than those of premixing). I am therefore satisfied with the code's capabilities in this area and would expect that the 3-D version of the code would also perform satisfactorily in this respect.

* *

41. While Chapter 2.1 uses a homogeneous model for the two-phase behaviour (by forcing large drag between the phases), it is unclear whether the calculations reported in Chapter 2.2 still use this model. If not, it would be interesting to compare how much better the full model performs against the experimental data, compared with the homogeneous model.

* *

42. The authors of ESPROSE.m have implemented an, at the time, novel approach to cover lack of thermal equilibrium in the coolant during the propagation. This approach is physically based and can be considered to be well-justified.

* *

43. The application of the ESPROSE.m code to steam explosions depends on the assumed constitutive physics. As Appendix D (particularly figures D8 and D9) illustrates, the assumed parameters of the microinteraction model can have a major impact on the prediction (eq changing the parameter for coolant entrainment can change the C-J pressures by two orders of magnitude). Appendix C provides results from a series of experiments with one high temperature simulant, that has been used to modify a hydrodynamic fragmentation model to take account of thermal effects. This approach is acceptable, but the range of uncertainty in the model parameters needs to be allowed for in any assessment.

The authors note that 'the main need identified is for constitutive laws for microinteractions with reactor materials' [Abstract] - I agree. They also claim that

'reasonably conservative assessments are possible' - however the main report does not indicate what parameters were used to obtain a sufficiently conservative assessment.

See Appendix C and addendum.

44. *I would have expected to see more discussion of the comparison with KROTOS experiments in the report as originally supplied, rather than a reference back to the study. Although there are some limitations on knowledge of the initial conditions and, in most of the tests with explosions, some loss of data, these provide the greatest confidence in the application of any model to the steam explosion propagation phase. The calculations for KROTOS-38 provided as a supplement are useful. With current knowledge it is more important to be able to demonstrate conservatism in the calculations rather than good agreement through parameter adjustment. Recently I saw calculations with TEXAS-IV for this test, where a very different melt distribution was calculated that led to very good agreement with the observed pressurisation following the trigger. Until there is a visual record of such tests it is not possible to determine which simulation is closer to reality.*

* *

45. *In reading the material, I noted a number of examples where detail was not clear to me. These are listed here for convenience, but have no impact on my overall assessment of the methodology:*

1. In chapter 2.1 what value is used for P_1? Figure 6 a implies 100 bar, but elsewhere finite results are given when P_2 is only 10 bar.

For results in Figures 1 through 5, $P_1 = 1$ bar. $P_1 = 100$ bar for results in Figures 6 through 9.

46. *2. In figs 7 and 8 of Chapter 2.1 α is shown as varying. I assume α is a void fraction - of which region?*

α is the void fraction of the region ahead of the shock.

47. *3. Chapter 2.1 presents results with an without phase change of the gas. It would have been instructive to see a direct comparison to illustrate the importance, or otherwise, of the phase change on wave propagation.*

Results presented in Figures 4 and 5 are for a 10% steam/water mixture with phase change.

48.   4. I had difficulty understanding the location of the pre-voided region discussed in Chapter 2.2. Note that Fig 7 is incorrectly referred to as Fig 8 in the text. If for Fig 3 the pre-voided region stretches to the base of the tube, I do not understand the respective difference in timings of (1) the time between the shock arriving at PT3 after PT1 and (2) the time between the shock arriving at PT3 and its reflection from the base arriving at PT3. Note that you have offset the pressures in the figures for ease of presentation.

The typo on Figure 7 is corrected. The pre-void region is the region between L = 100 cm to L ~185 cm, as shown in Figure 3. The bottom of the tank is at L = 300 cm. The region between L = 0 and L = 100 cm is the driver section, as shown in Figure 1

49.   Additional Comments on DOE/ID-10504: PM-ALPHA Verification Studies by T G Theofanous, W W Yuen and S Angelini

INTRODUCTION

This document represents the culmination of a substantial piece of work to develop a mixing code for steam explosion studies and to validate it against the experimental data. The report makes good use of the (still rather limited) experimental data available for this purpose. The report concentrates on the presentation of results rather than their evaluation. It would benefit from a leading chapter on the philosophy of the verification/validation process, accompanied by a matrix indicating which of the code's models are tested, and to what extent, by the comparisons reported. It would further benefit from a longer concluding chapter that draws together the results in the context of this matrix.

We think the key aspects of the philosophy have been explained clearly enough.

50.   It is noticeable that efforts are made to compare isothermal particle-water predictions with accepted correlations. There ought to be scope to include similar material on two-phase flow in the absence of particles; this is probably more important in establishing the reliability of the code to predict voiding behaviour.

Such tests can be meaningfully be done for the dispersed regimes (bubbly, droplet), and the particle cases considered are already quite sufficient for this purpose. The drag laws used for fluid or gas "particles" are slight modifications of these and they are supported by wide data bases.

51.   While there are many detailed comments below, these should not detract from the achievement of the authors. The comparisons performed indicate that

the code has the ability to make reasonable predictions for reactor conditions. However, the results should still be used cautiously, as the data currently do not exist to provide full validation of the model.

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52. Specific Comments

Chapter 2

<u>Singel particle settling</u> While tracking a representative particle in a Lagrangian fashion gives the expected analytic result, melt mass is usually tracked through the volume fraction. This can be much more diffusive.

These were simple tests made to begin with. For numerical diffusion see below.

53. <u>Settling of particle clouds</u> I have tried to check the consistency between the drag law for particles given by equations 3.14, 3.21 and 3.22 of Appendix A with the drift flux formulation, but have been unsuccessful. There appear to be inconsistencies between equation 2.4 and Figures 2 and 3. Taking $V_\infty = 0.487$ m/s, gives the liquid superficial velocity for $\alpha = 0.5$ as 0.093 m/s. Figure 2 shows this as 0.12 m/s, while Figure 3 indicates 0.19 m/s. This suggests that it is not the superficial velocity that is being plotted in Figure 3 but the flow velocity, which would be 0.186 m/s from equation 2.3. My evaluations of the drag coefficient given in Appendix A for this case give a relative velocity of 0.286 m/s, or a superficial velocity of 0.143 m/s. However, PM-ALPHA has produced, according to Figure 3, a value close to 0.2 m/s. My hand calculations indicate that the PM-ALPHA model is not as close to the drift flux model as implied by Figure 3.

The closeness in Figure 3 is correct. The confusion was generated because there was a mislabeling of the vertical axis in Figures 2, 3, and 4. It is the velocity, **not** the superficial velocity plotted. Also, 0.186 is in good agreement with 0.2, isn't it? Finally, we found also a slight plotting error in Figure 2 regarding the drift flux line. The correct figure is attached.

54. <u>Settling of particle clouds</u> The comparison with the drift flux model is clearly important as it goes some way to establishing the reliability of the drag coefficient modelling in PM-ALPHA (although it should be noted that the particle volume fraction is unlikely to exceed 20%, where the enhanced drag due to

particle-particle effects is not that significant). It is less clear what one is expected to learn from the material presented on transient analysis regarding the validity of the code's models. It would have been useful instead/in addition to perform the same comparison with the drift flux model for gas-water interactions where the form of the drag coefficient is rather different.

Several reviewers found these results very interesting!

55. Section 2.2.2: MAGICO experiments It would have been useful to have a short synopsis of the conclusions drawn about the model from the analysis of the MAGICO tests. Besides the qualitative agreement (and general quantitative agreement) on the nature of the interaction. I think the most significant finding is the prediction and measurement of large void fractions (greater than 70%) illustrated in Figure B23). It would be useful to provide a statement on the specific code models that these observations are believed to validate (eq water-steam drag, film boiling, radiative heat transfer??).

All play a role. These are integral comparisons carried out for a wide range of conditions (see also addendum to Appendix B), and the results speak for themselves. We don't think it is very fruitful or even appropriate to assign significance. Note that each model stands on its own merits, and these comparisons show that when put together the result is very consistent with reality. This should not be too surprising.

56. The QUEOS Experiments: This looks a very interesting analysis of these tests. The presentation of results in Figure 4 etc gives an excellent way of qualitatively comparing code results and experimental observations. Perhaps some comment should be made about the apparently coherent release of large gas/steam volumes, seen eq at 0.41 s in Figure 4; also on the water spout effect predicted at this time (this seems to provide the mass difference between Meyer's interpretation of the water fraction in the mixing region and the PM-ALPHA values). The acceptability, or otherwise of numerical diffusion, is a complicated matter, because of non-linear feedbacks through the drag laws; it is very easy to underpredict the peak particle volume fraction. Figure 5 does not give units for the liquid flux. Condensation in PM-ALPHA looks too effective at later times in Figure 6 compared with the experimental image.

The liquid flux units are cm/s. See finer grid results for QUEOS in the addendum to Appendix B.

57. *Chapter 3*

Comparison with CHYMES: It is only fair to note that this comparison was only possible by turning off sub-cooling in PM-ALPHA. Much of the detail of the PM-ALPHA predictions depend on the modelling of sub-cooled boiling. The observation that PM-ALPHA often only produces any void somewhat behind the particle front, whereas other codes tend to produce some voiding wherever there are hot particles can have significant implications on the initial flow of water. For instance, we did not reproduce the so-called ETHICCA effect with CHYMES. In addition, CHYMES drag laws were modified for the comparison. However, the main result - water depletion is predicted by both codes (at least for low pressure systems close to saturation temperature) - is robust.

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58. *The MIXA Experiments: I will try to clarify the question of time origins for the data. The experimental report, which I have, has unequivocal timings, with an origin starting at the ignition of the pyrofuse for the thermitic reaction. On this timing the melt first contacted the water at 3140 ms, the peak (measured) steaming rate was at 3810 ms and the peak pressure occurred at 4215 ms. The authors have adopted a timescale (their figure 4) where the time of first melt contact is taken to be zero. This is the same timescale used in Figure 1 of Fletcher and Denham for the measured pressure in the gas space - so the comparison given for pressure in the top frame of Figure 6 (page 3-21) is correct. However, the transient steaming rate figure (figure 8 of Fletcher and Denham) does not use this time basis - this is because it was derived from the CHYMES calculations with the experimental data over-plotted). There is a significant outflow of gas before the melt reaches the water surface as shown in this figure. This may be due to (i) preheating and expansion of the gas in the test vessel; (ii) evaporation of a water film on the test vessel wall (the favoured explanation for similar observations in FARO), and/or (iii) evaporation from the water surface. The experimental data on the middle and lower frames of Figure 6 should therefore be shifted to the left by about 0.32 s (error on this is only from my reading of the graph in Fletcher and Denham - it is no more than 0.02 s). The effect of this is to move the measured peak steaming rate ahead of the measured peak pressure. However, I now believe that the measured steaming rates become increasingly unreliable (as quoted) due to carry-over of a two-phase mixture; similar behaviour has been observed in*

PREMIX. Unfortunately, while the experimenters noted water carry-over post test, and observed a reduction of water height in the vessel post-test of 25 mm (the measured steam would produce a reduction of only about 4 mm), there is no information to determine how much of this occurred because of evaporation during the heating of the water. The same comments apply to Figures 7 to 10.

While the PM-ALPHA calculations are as good as or better than any I have seen for MIXA-06, I am not convinced that the real behaviour in the test is being captured. The most noticeable features are the radial expansion of the melt as it enters the water and the apparent lack of any visual record of droplet break-up. Both of these effects seem to be connected with sudden expansions of the melt region, due to enhanced steam generation, giving much more coupling between melt and steam that accounted for in CHYMES, and, by the look of it, in PM-ALPHA. I conjecture that droplet fragmentation is occurring during these rapid events. The formation of smaller particulate then encourages another process of melt spreading. Smaller particles are carried upwards by the central steam flow, move outwards, and fall in the periphery, thus extending the melt envelope outwards.

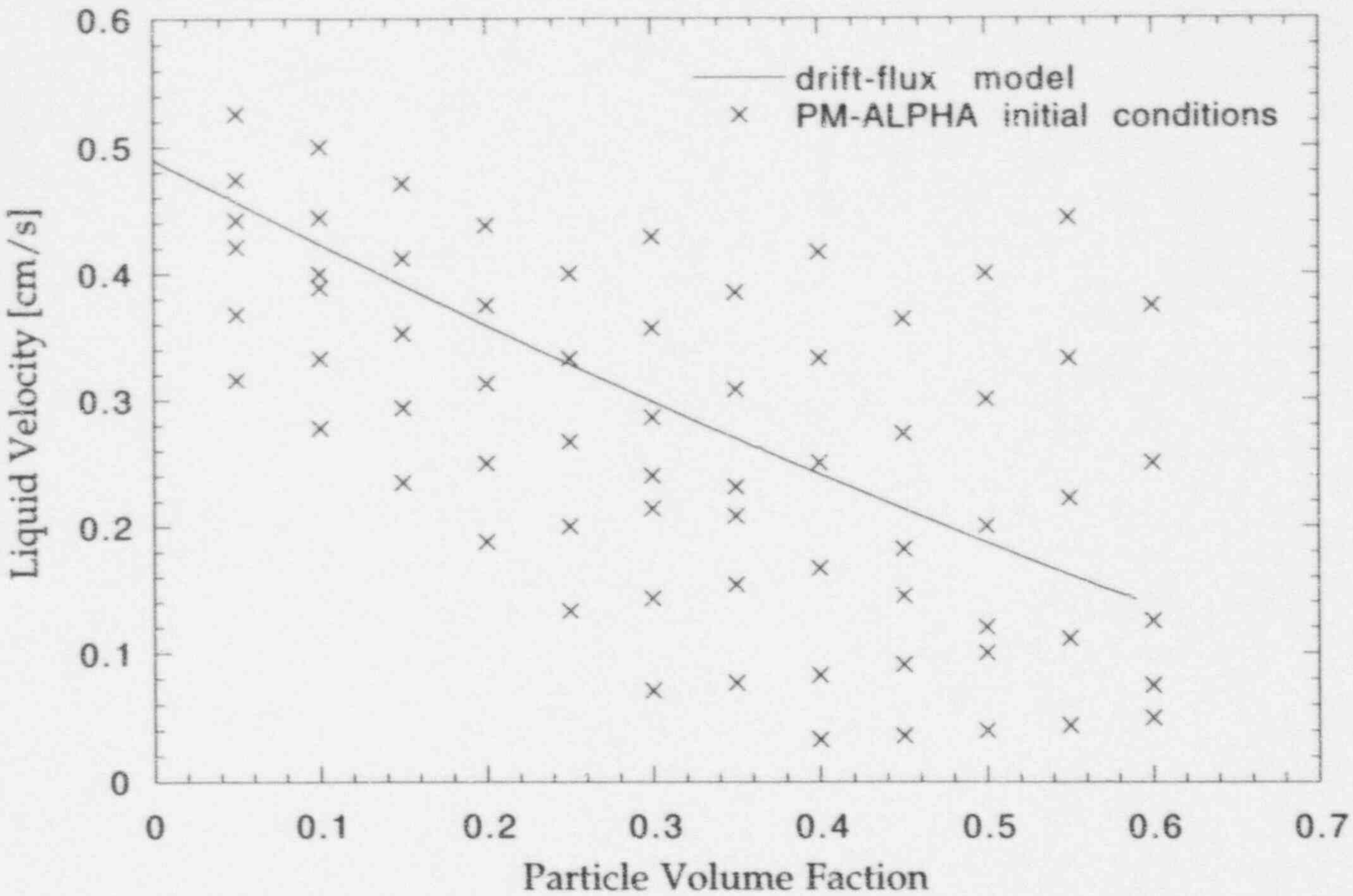
There is no visual evidence of the predicted extensive voiding atound the melt region - the leading droplets appear to be falling through water - the steam generating region is large because of the spread of the melt droplets.

I agree on the sensitivity of calculations to assumptions on break-up. Has the predicted mean particle size been compared with the experimental value of about 3 mm?

This section should contain discussion/conclusions on implications of the comparison for model validation.

With so many uncertainties in the test how can we reasonably draw conclusions?

59. <u>The FARO Experiments</u>: Clearly the initial melt droplet size is very uncertain, as is the spread of the melt. L-14 appears to be the test in which the melt stream was best collimated, but one cannot tell whether the stream contracted as it poured through the gas space, or underwent a mild expansion (in L-11 the melt stream appeared to undergo a major expansion). If it is believed that the melt jet contracted (note typo: steam for stream 4 lines from end of page 3-25), then the radial meshing with $\Delta r = 5$ cm is too small. The choice of break-up



0.6
0.5
0.4
0.3
0.2
0.1
0
Liquid Velocity [cm/s]
drift-flux model
PM-ALPHA initial conditions
0
0.1
0.2
0.3
0.4
0.5
0.6
0.7
Particle Volume Faction

fig 2

parameters appears arbitrary - presumably these were selected to give reasonable agreement with the experimental data. More detailed modelling of the melt release vessel indicates that the melt exit velocity was close to 3 m/s for most of the pour; this will not be replicated by the model shown in Figure 2. I am surprised that a Weber number criterion did not limit the droplet size; with the CHYMES implementation of this criterion we almost always get mean particles close to those observed in experiments (typically 3 - 5 mm). The comment on the absence of significantly superheated steam in the experimental data seems to me to be special pleading - it might be right, or the steam flow might be much less concentrated on axis than predicted by PM-ALPHA, giving steam closer to saturation conditions. It is difficult to relate the scales on the coloured contour plots in Figures 10 and 11 to the colour-scale, particularly because of interpolation effects. Is break-up still occurring after the particles have settled (unless they have solidified)?

No. Actually at this stage reagglomeration of any inadequately solidified particles begins.

60. Again, this section should be supplemented by an evaluation of the implications for the reality of PM-ALPHA predictions. I think a word of caution is necessary, as although PM-ALPHA, with the assumption used, performs well against experimental data, it predicts a highly two-dimensional configuration. Alternatively, good comparisons against the data have also been produced with the one-dimensional code, TEXAS-IV. Until we see the nature of the interaction zone (I expect it to be between these two computational extremes) then it is not possible to say that one simulation is better than the other.

We do not think it would be appropriate for us to shed vague doubts on the validity of the PM-ALPHA comparisons because the 1D TEXAS-IV was made to produce good comparisons too. By the last sentence we hope the reviewer doesn't really mean that the behavior in the experiment is 1-1/2D. The situation **is** 2D and it should be rather obvious that a 2D computational framework is a **necessary** starting point, before one begins to examine any further the degree of "simulation" obtained.

61. Chapter 4

I agree with the general comments on break-up modelling. As implied in my comments above, backing out break up behaviour from the experimental data may compensate for other errors in the modelling. As I also noted, it is unclear,

even with the visualisation, what break up processes were occurring in MIXA; I suspect the processes are much more dynamic than are currently embodied in the models, and coupled strongly with events of enhanced steam generation (coolant trapping?). New FARO tests with visualisation should provide information on the coherency of the initial pour, besides evidence of any subsequent break-up.

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62. Numerical aspects

Our experience is not as comforting as that presented by the authors. I think that numerical diffusion is probably not an important issue for large-scale mixing calculations. However, it becomes important in comparisons with smaller-scale experiments, which are often dominated by leading edge effects. Numerical comparisons that we have performed (external to CHYMES) show that upwinding schemes run below the material Courant condition lead to very poor predictions of peak particle fraction, and thus drag. Higher order schemes have to cater for possible discontinuities at the leading edge. Lagrangian approaches, as used by the authors for their front tracking, provides much better accuracy, both for velocity and peak volume fractions. I believe that current schemes in the mixing codes can be improved substantially using physically based Lagrangian limiters, rather than mathematical limiters. Fully Lagrangian approaches have the greater benefit of handling a spectrum of particle sizes. This may be the best way to treat jet break up and is necessary if one is going to capture the role of the smaller droplets in spreading the melt, as observed in MIXA-06.

The current presence of numerical diffusion makes the code results difficult to interpret (eq how far back is the predicted peak concentration from the melt leading edge in the MIXA-06 calculations?). Our experience with more refined meshes is that numerical diffusion is indeed reduced, but the calculations are much more prone to instability of the resulting interface; this numerical instability probably reflects the actual instability of interfaces observed in experiments.

This whole discussion reflects the reviewer's own experiences with CHYMES, and it should not be confused as being applicable to what we have presented. Our results should be judged on their own merits, and there are ample comparisons to allow one to comment directly on the **present** experience.

63. Concluding Remarks

I would expect a more detailed technical evaluation of the calculations presented. I am surprised that questions related to the radiation transport modelling, which was clearly important in the FARO simulation, have not been highlighted. I would have liked to see more explicit bounds on models emerge form the work.

We wanted to keep our concluding remarks brief and to the point. If we were to provide highlights in this section, we would have to include much more than the non-local radiation model (actually it is even more important in MIXA, as discussed in the respective section). The last sentence indicates a degree of dissatisfaction, we think, with how far, quantitatively, we could go based on the comparisons. This is a question of judgment, and one thing for sure is that we do not wish to oversell what we have been able to show. We believe the comparisons, and the whole array of situations considered, speak for themselves, and adequately loud to be understood, both in their success, as well as their limitation by those involved in this kind of work, and at this state we are satisfied with that.

64. PM-ALPHA Models

The details of the correlations embodied in PM-ALPHA will not be reviewer in detail.

I believe the modelling approach is sound. I note that reactor geometries may impose strongly three-dimensional flow regions, so a 3-D code is needed for detailed applications (if found to be necessary). I get the impression that the modelling philosophy falls between two stools. At places it is admitted that the model necessarily contains many simplifications and constitutive physics that is uncertain, but only in the field of jet breakup is a parametric approach used. I would prefer a broader approach to treatment of uncertainties.

This comment forgets that we have indeed a 3D code. We did our best to catch and present a correct picture regarding uncertainties. If the reviewer has particular aspects beyond those presented that he would like to see, we would be happy to undertake the computations needed.

65. With sub-cooling implemented in CHYMES, it is closer in concept to TRIO-MC rather than PM-SLPHA. (EVA should be spelled IVA).

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66. Elsewhere we have queried the use of the drag coefficients for droplet and bubbly flow. These are derived for bubbles rising at terminal velocity in a gravitational field. It is found that the shape factor for the bubble causes the

drag per unit mass of gas to be independent of length-scale. It is noticeable that no effect of melt droplet shape appears in the corresponding formula for drag coefficient for the melt phase (equation 3.21). A completely different form for the liquid-vapour drag is used for intermediate values of void fraction; this may give large changes in drag when the transition void fractions are crossed. It is not evident that there are such sudden changes in flow regime in plenum geometry.

For drag coefficients we used the best available correlations. In all our experience with these models we find no reason to raise significant questions to them.

67. I have not had the time to consider the radiation treatment in detail; also the relevant appendices are not included in the exerpt. For dense clouds of particles, the self-absorption effect will be very important. I would like assurance that this does not allow the region to emit more radiation externally than that of a black-body covering its surface at the same temperature.

As explained in the text, self-absorption is included in a consistent manner.

F.15. Response to M.F. Young (SNL)

General Comment and Highlights

General and unqualified agreement with the conclusions of the work under review.

Point-by-Point Responses

1. *This is a massive piece of work which includes, in addition to steam explosion loads, some areas with which I am not particularly familiar, such as probability methodologies and plugging behavior of molten materials. I will therefore comment mostly on the steam explosion loading.*

The approach taken in this report to determine steam explosion loads is essentially the one that has been recommended by most if not all steam explosion researchers: use of computational models validated against experiments to determine bounding envelopes for reactor accident scenarios. Prof. Theofanous has taken an additional step here in simplifying the probabilistic framework with his ROAAM method; I think this is entirely in keeping with the use of these type of calculations in risk assessment and rulemaking. I believe that the work described in this report has successfully accomplished the goal of enveloping the steam explosion loading. The usefulness of the results in rulemaking, however, therefore depends on the confidence placed in the initial conditions of the accident scenario and in the analytic tools used.

In regard to the initial conditions, it is very important to the conclusions reached in the report that the melt be introduced through the side of the reflector and that the lower core structure and support plate be plugged. As I mentioned before, plugging is not my area of expertise, so I will not comment further other than to point out again that confidence in the initial conditions is very important.

In regard to the analytic tools used for calculating steam explosion loads, I have some comments concerning possible gaps in the cases considered and in verification of certain parameters used in the models.

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2. *First, I see that trigger timing was varied parametrically but not trigger location; I assume that the cases were triggered near the bottom of the mixture region next to the wall; although I suspect that this is probably the most severe case, I am wondering about the consequence of other trigger locations.*

We did not do extensive variations on loation of trigger, but what we have seen agrees with what we expect: it is the premixture composition rather than the location or magnitude of trigger that controls the energetics.

3. Second, in ESPROSE.m, there are three parameters, that must be set from experiment: an entrainment factor, a fragmentation constant, and a thermal enhancement factor. There appears to be some dependence on the melt material for these factors, so the lack of data with reactor materials to set these parameters concerns me. I believe this was also pointed out by Theofanous et al. in the "Concluding Remarks" section of the ESPROSE.m verification report, in regard to expanding the microinteractions database to reactor materials. Use of parameters that had been set from experiments using reactor materials would enhance confidence in this aspect of the calculations. In regard to the microinteraction model itself, I believe that this model is sufficiently close to reality that experimental results can be extrapolated to reactor scale.

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4. Third, the lack of a stratified mixing case bothers me, or rather, the lack of data to properly model this case. I do not doubt that what the authors say is correct: if ESPROSE.m were run with a stratified case, it would probably produce a very nonenergetic steam explosion for the reasons stated. However, if memory serves, a stratified explosion in a foundry involving water dripping into a "car" containing molten iron produced an explosion strong enough to take out some of the walls of the plant. This incident seems in contrast to what would be predicted with ESPROSE.m, although the melt is different (iron versus reactor material) and the water was undoubtedly subcooled. The incident mentioned probably involves mixing of the stratified material caused by the province of PM-ALPHA? Maybe the authors could comment on this, or maybe it indicates the need for some stratified experiments.

The incident mentioned occurred at the Farthingham Foundry. It involved water pouring in a steel "torpedo," already pretty full of molten steel, through a small opening on top. The explosion occurred as the torpedo was set in motion, to transport the melt (apparently not realizing the accidental presence of water). The water became trapped in a highly confined geometry and pressures developed made the torpedo explode. This type of phenomenon is not relevant to the stratified interaction considered here.

5. Fourth, on page 6-4, there is a conclusion that the size of the impulse does not depend strongly on the size of the mixture region. I think that this is in contrast to first principles, which would suggest that it would be directly proportional, ignoring other effects like the varying void fraction, and to the results in Table 6.1, which indicate a strong variation, ignoring the time, between cases C1 and C2: 90 vs 120 for $\beta = 10$*, and 120 vs. 200 for* $\beta = 20$*. Or did I misread the sentence? Also, how do the results compare in magnitude to the impulse of the initial trigger itself?*

The results do not depend on the magnitude of the trigger. The results in Table 6.1 show that, indeed, the impulse does not depend on the size of the premixture (note that bigger premixtures are more voided). There is only one case, the 200 kPa-s one, that stands out. This case has been reexamined and discussed in an addendum to Chapter 6.

6. Fifth, in the section on stratified layer of molten steel and reflood, Chapter 8, there appears to be one piece missing to make the case that the scenario is impossible: the thickness of the crust is not mentioned. Specifically, is this a "thick" crust that is stable, or is it a "thin" skin that could be broken?

The thickness of the crust will depend on the time after addition of water. It will get thicker with time, and hence less likely to allow contact, as more water accumulates with time.

7. All in all, this appears to be a very complete piece of work.

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8. Following are minor points and typos.

1. In the discussion of the ABAQUS model of the lower head, it is referred to as a shell model; this is somewhat confusing at first, as shell models are normally thought of as meaning <u>thin</u> shells, i.e., no bending moments are supported. This could be clarified by calling the model a thick shell, for instance.

We have mostly used thin shell elements. We ran a comparison for loading pattern 1+, using thick element and found a slight decrease of plastic strain from 21.3 to 20.6%.

9. 2. On p. 4-4, there is a comment about approximately 25% of the fuel remaining uncovered. Is there a reference for this?

Typical result of computations such as with MAAP.

10. 3. On pp. 5-6 through 5-9, it is hard to compare the graphs chosen because of varying z axis scales and varying times for the plotted lines. For instance, the C1-nb plots start at 0.4 s whereas the Rc1-nb plots end at 0.12 s. I see that there are other plots with overlapping times in the Appendix, so maybe one of these would be better.

The point is well taken. See better representation in the addendum to Chapter 5.

11. 4. On p. 5-10, it says "only a very small fraction of the coolant is found to coexist with the water"; should this be <u>melt</u>?

Yes; correction made.

12. 5. In the graph for C2-10 a on p. B2-7, the last time is given as 215 s instead of 0.215 s.

Typo corrected.